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SCIENCE PROGRESS

THE UPPER ATMOSPHERE

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RECENT progress in the investigation of the upper regions of the Earth's atmosphere has been mainly along two different lines. The first is connected with what may be called the meteorological state of the atmosphere, namely its temperature, density, etc., while the second has to do with its electrical characteristics. Many balloons carrying meteorological instruments have been sent up, but the great majority of these have failed to reach a height of 25 kilometres. In a few cases where specially large balloons were used a height of 30 kilometres has been reached and a balloon sent up in Russia has recently been reported to have reached 40 kilometres. In order to obtain information about the atmosphere at still greater heights it is necessary to employ indirect methods of exploring the atmosphere. Fortunately, several such indirect methods are available and it is our present purpose to describe them and the results that have been obtained from them.

METEOROLOGICAL CONDITIONS BELOW 20 KILOMETRES.—From the large number of sounding balloons which have been sent up in many countries carrying recording instruments the general meteorological conditions of the air up to 20 kilometres are now fairly well known. In the lower part of the atmosphere the temperature falls with increasing height at a rate of about 6° C. for every kilometre rise. The exact rate at which the temperature falls differs from day to day and may be different at one height from that at another height. The fall of temperature with height does not, however, continue indefinitely, but at some fairly well-defined height it stops and the temperature from this point upwards is found to change but little as the balloon ascends to its greatest height. The height where the temperature ceases to fall—known as the Tropopause—is usually between 8 and 14 kilometres in Europe.

The results from recording balloons thus show that the atmosphere is divided into two parts by its thermal structure; the lower part, in which the temperature falls with increasing height, is known as the Troposphere and the upper part as the Stratosphere. The conditions in these two regions do not remain constant from place to place nor from day to day. Over the Equator the lower air is naturally warmer than that over the Polar regions, but over the Equator the Troposphere extends to an average height of some 17 to 18 kilometres. As the temperature is falling at a rate of some 6° C. for every kilometre throughout all this height, it is very low by the time the Stratosphere is reached, being about 80° C. below zero. In Polar regions the Stratosphere comes relatively low down and begins at a height of 6 to 8 kilometres, with the result that though the temperature of the air at ground level is very low there is no great difference between the air at the surface and that in the Stratosphere and the latter has a temperature of about 40° C. below zero. The temperature of the Stratosphere over the Polar regions is thus much warmer than that over the Equator, and as there is little change of temperature with height in the Stratosphere we find the rather surprising result that at a height of 16 to 20 kilometres the air over the Polar regions is much warmer than that at the same height over the Equator. Indeed, the coldest air in the atmosphere is probably that at a height of 18 kilometres over the Equator.

The conditions of the air in both the Troposphere and Stratosphere also vary from day to day, and these variations are closely associated with the meteorological situation. When a well-defined depression has just passed across the country it will be found that the Troposphere is colder than usual, that the Stratosphere is abnormally low and that its temperature is above the average. On the other hand, when a well-marked anticyclone covers the country the Troposphere will be warm, the Stratosphere cold and the Tropopause high. It will be seen that these changes are similar to the changes with latitude, so that the conditions in middle latitudes in the rear of a depression are similar to those in Polar regions and the conditions in an anticyclone are similar to those in Equatorial regions.

Meteorology of the Upper Atmosphere.—What has been described above has now been known for several years, but until recently little was known about the conditions above 20 kilometres, and it was usually supposed that the temperature would remain roughly constant up to very great heights. This view was questioned when the results of observations of meteors passing through

the upper atmosphere were used to give a rough idea of the temperature at these heights. Contrary to expectation, it appeared that the temperature rose again above the Stratosphere and became as warm or warmer than the air at ground level. These results were, however, very rough, chiefly because it was not possible to get accurate measurements of the brightness and speed of the meteors.

The idea that the air at a height of some 50 kilometres might be warm was found to fit in with the observations of sound heard at a great distance from its source, and such observations have now been used to give us a much better knowledge of the temperatures at heights of between 40 and 70 kilometres above the surface. When a large explosion occurs, the sound is heard for many miles around, but not to such great distances as might be expected. That the sound is not heard farther away is largely due to the fact that sound travels faster in warm air than in cold air. Since, in the lower atmosphere, the temperature falls with increasing height, sound travels faster through surface air than through the air a little higher up. Thus the sound waves near the surface run ahead of those above, with the result that the sound ray is bent upwards, away from the ground. The bending of sound waves upwards is well seen in the fact that sounds in a valley are often heard much more clearly on the hills on either side than at places the same distance away in the bottom of the valley. Now, in the case of very large explosions it is frequently found that while the sound cannot be heard for more than 20 or 30 miles round the explosion, it is heard again at places very much farther away, perhaps a hundred miles from the explosion, and often heard quite loudly. Such cases as the firing of salvos by the Fleet often show this phenomenon, and in a recent case when the Fleet was off Portland Bill the firing was heard as far away as Surrey and Oxfordshire but not at many intervening places.

The sound waves which are heard at these great distances are found to have taken some two minutes longer over their journey than they should have done had they travelled direct through the lower atmosphere. From this and the fact that they are not heard at intermediate distances, it seems clear that the sound has travelled up into the upper atmosphere and then been deflected back to the ground. This curious behaviour had been known for a long time and several explanations had been suggested to account for the fact that the sound is deflected back to earth by the upper air. One suggestion was that the upper air was largely composed of hydrogen, and since it is known that sound travels faster in

hydrogen than in oxygen and nitrogen, this would account for the downward bending of the sound rays. None of these explanations, however, carried conviction until Dr. Whipple pointed out that if the temperature rose again at very great heights, as was suggested from the meteor results, then the sound rays would be bent down on entering this upper warmer region in just the same way as they are bent upwards by the fall of temperature in the air near the ground.

This last explanation seems quite satisfactory and its importance is that it gives a method of measuring the temperature of the

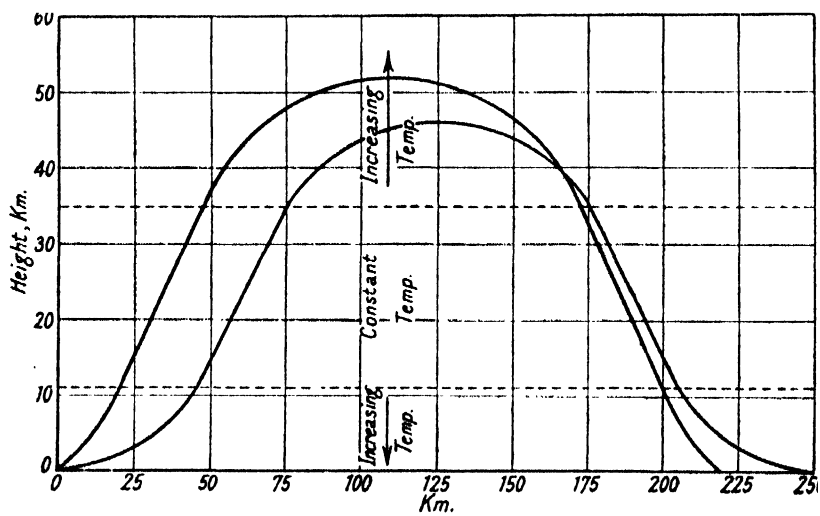

FIG. 1.—The passage of sound waves to great distances. (*After* Whipple.)

The diagram represents a vertical section of the atmosphere and shows two paths by which sound waves may travel to great distances via the upper atmosphere. The source of the sound is at the bottom left-hand corner. The sound waves are bent upwards in the lowest region and downwards in the upper region owing to the opposite temperature gradients in these regions. In the middle region of constant temperature the sound travels straight.

air at the great heights reached by the sound waves. By using special microphones instead of the ear it is possible to detect the waves from the firing of one big gun at a distance of a hundred miles or more. It is easy to measure the total time taken by the sound to travel along its path to the upper atmosphere and back, and further, by a suitable arrangement of three microphones at the receiving station, it is possible to measure the angle at which the downcoming sound ray strikes the earth's surface. Given such observations, it is not difficult to calculate most of the details about the path travelled by the sound, such as the maximum height above the ground and the speed of sound at that height. Then,

since the speed depends on the temperature of the air, it is possible to estimate the temperature of the atmosphere at these great heights.

Not infrequently two or more sets of downcoming waves are recorded at a great distance which all come from one single explosion, the waves being separated from each other by several seconds and the angle at which they strike the earth being different in each case. In such cases it is clear that the waves have travelled through the upper atmosphere by different paths, reaching different maximum heights before finally converging on to the recording microphones. Such a complication instead of obscuring the results only gives increased information, since each set of waves can be treated separately and the temperature can be found for the different heights to which they have penetrated. Thus we get not only the temperature at one height but that at two or more heights and so the rate of rise of temperature with height. This rise of temperature is found to begin at a height of about 35 kilometres and is at a rate of about 6° C. per kilometre of height and is therefore roughly the same as the rate of decrease of temperature with height in the Troposphere. It is not yet certain how far this increase of temperature with height continues, but it appears that the temperature has risen to about 100° C.—the boiling-point of water—at a height of 60 to 70 kilometres and that it is still higher above.

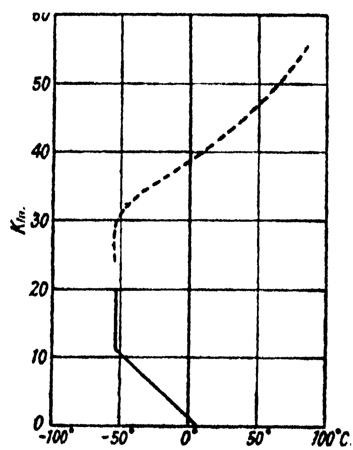

FIG. 2.—Probable distribution of temperature with height over England.

The continuous line from the ground level to 20 km. shows the average temperature as obtained from balloon ascents. The dotted line indicates the probable temperature as obtained from sound waves.

Up to the present time most of the observations of the upper air temperatures by means of sound waves have been made in Europe, but it seems now to be established that the upper warm region extends over the Equator at much the same height as in Europe, while it has been found in Polar regions also. When it is possible to accumulate more observations it will be interesting to determine the diurnal and annual variation of temperature at these great heights, as well as to see if there are day to day changes in temperature associated with weather conditions similar to those found at lower levels. Unfortunately, the cost of special explosions is high and most of the observations made in this country have

utilised the sound from big guns which were fired for other purposes. It is a curious circumstance, probably due to wind at great heights, that the sound is hardly ever heard to the west of the source in winter nor to the east of it in summer.

CAUSE OF THE WARMTH AT GREAT HEIGHTS.—The temperature of the air in the Troposphere, where there is constant mixing of the air between various levels, is governed largely by the supply of heat constantly received by contact with the warm ground. In the Stratosphere, and above, the conditions are different and there is little or no mixing with the air below. Here the temperature depends on the absorption and emission of radiation. Radiation from the sun—largely in the form of visible light—passes inwards through the atmosphere and an equal amount of energy passes outwards from the earth in the form of dark heat radiation. Only certain gases in the atmosphere take part in this absorption and emission of radiation ; nitrogen, for example, is nearly transparent to all the types of radiation passing through the atmosphere, but oxygen, ozone and water vapour strongly absorb and radiate certain particular types, being transparent to others. Thus oxygen strongly absorbs the radiation of the very shortest wave-lengths received from the sun, namely the extreme ultra-violet radiation. Ozone strongly absorbs radiation of rather longer wave-lengths though still in the invisible ultra-violet region of the spectrum, and also absorbs a little in the yellow-green, and again a little in the long-wave infra-red region. Oxygen and ozone together absorb about 6 per cent. of the total energy of the sun coming to the earth, and since the absorption is very strong this energy is absorbed by the air at a very great height in the atmosphere. Water vapour is nearly transparent to all the visible and ultra-violet radiation received from the sun, but strongly absorbs most of the long-wave infra-red radiation emitted by the earth.

The radiation from all bodies at temperatures below red heat takes the form chiefly of infra-red radiation. Oxygen and ozone can emit little radiation of this kind, so that the loss of heat by emission of radiation is chiefly due to the water vapour present. If only water vapour were present it would absorb little energy from the sunlight but would absorb the infra-red radiation emitted by the earth and would bring the air to a temperature of about 50° C. below zero, at which temperature it would be absorbing and emitting equal amounts of radiation so that its temperature would remain constant. It is this process which chiefly governs the temperature of the Stratosphere, as oxygen and ozone do not play any appreciable part here, because all the solar radiation which

they could have absorbed has already been absorbed much higher in the atmosphere.

Passing now to the extreme upper limits of the atmosphere we come to the region where oxygen and ozone absorb the 6 per cent. of the sunlight mentioned before. This amount of energy is very large and at these heights there is not much water vapour, so that the atmosphere cannot easily lose heat by radiation. The result naturally is that the temperature of the air rises very much until the small amount of water vapour present is able to emit as much energy as is absorbed. Thus it will be seen that the actual temperature at any level depends on the amount and character of the radiation absorbed and radiated away and therefore on the relative amounts of water vapour and oxygen and ozone which are present.

If we knew the amount of oxygen, ozone and water vapour present at every height in the atmosphere it would be possible to calculate the temperature at all levels. The vertical distribution of oxygen can be assumed without any great error and the distribution of the ozone has been measured, but we are still without reliable knowledge of the amount of water vapour at heights above 20 kilometres. Assuming various distributions of these gases Dr. Gowan has calculated what would be the temperature of the air at great heights. His results show that there should be a marked rise of temperature of the air in the region about 40 kilometres, agreeing reasonably well with the actual temperatures found from sound-wave observations.

THE OZONE IN THE ATMOSPHERE.—Both oxygen and ozone are responsible for causing the air to be warm at great heights, but the effect of oxygen is greatest in the upper parts of the warm region, while the effect of the ozone is greatest in the lower parts of that region. A point of great interest is that while the amount of oxygen is constant, the amount of ozone varies greatly. Further, these variations in the amount of ozone are found to be closely associated with the weather conditions as seen on the weather maps for ground level. The total amount of ozone in the atmosphere is very small, but since it absorbs the ultra-violet part of sunlight very strongly its effects are of great importance. How very small the amount of ozone is may be seen in the following way: if all the air in the atmosphere were formed into a layer of uniform density, equal to that of surface air, there would be a layer 8 kilometres deep. If all the oxygen were separated from the other gases and formed into a similar layer by itself, it would make a layer about 1,700 metres deep. If the same were done for the ozone in the atmosphere, we should find a layer only about 3 millimetres deep on the average.

In other words the ratio of ozone to oxygen is as 3 millimetres to 1,700 metres. This small amount of ozone is not distributed uniformly through the atmosphere but is chiefly found at great heights. There is a little present in the surface air and the proportion of

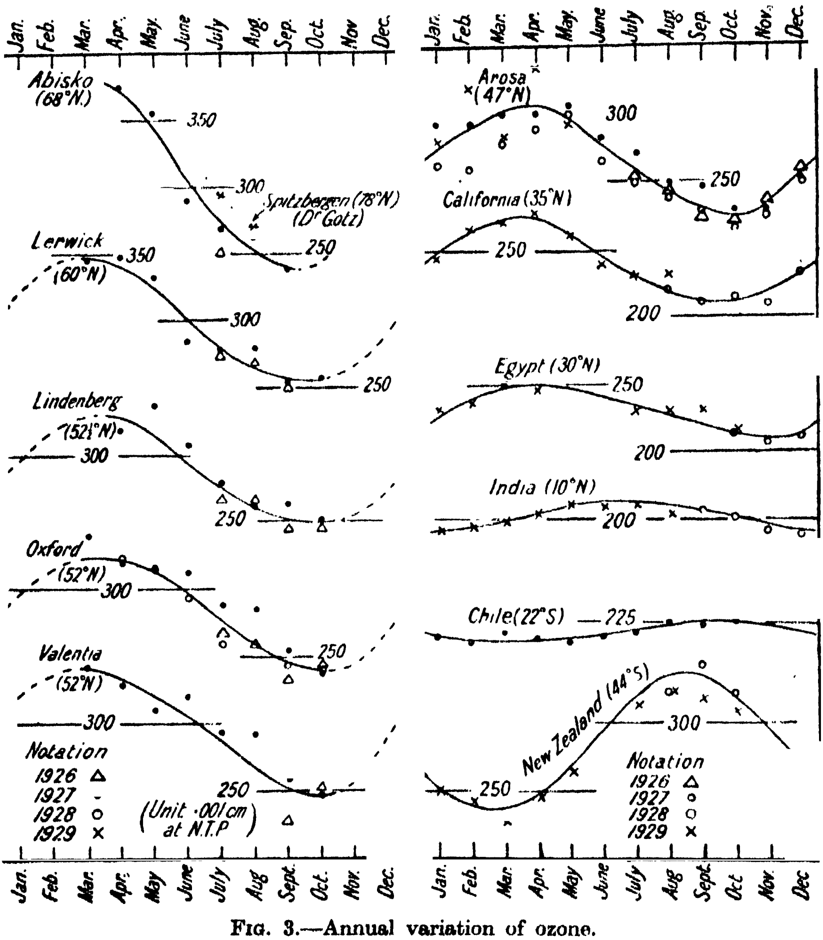

FIG. 3.—Annual variation of ozone.

The curves show the smooth annual variations in the total ozone content of the atmosphere over different parts of the world. The points represent the mean monthly observed values. Note that the amount of ozone is large in spring and small in autumn in both hemispheres.

[Figures 3, 4, 5 and 6 are reproduced from the *Proceedings of the Royal Society* by permission of the Council.]

ozone to the other gases increases with height until the maximum proportion is found at a height of about 35 kilometres.

Besides varying with the weather conditions, the amount of ozone in the atmosphere has a well-marked seasonal variation and also varies from one part of the earth to another in a regular manner

according to the latitude. Both these effects are shown in Figs. 3 and 4, from which it will be seen that the amount of ozone is generally large in the spring and small in the autumn. This annual variation increases from almost nothing near the Equator to a

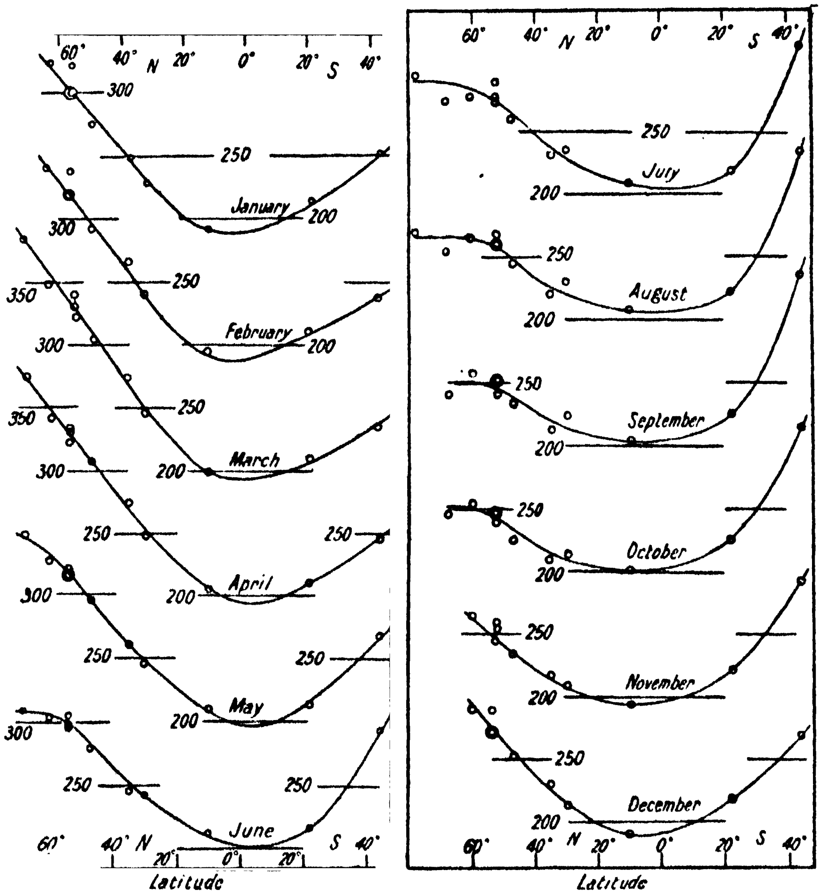

FIG. 4.—Variation of ozone with latitude.

The curves depend on the same observations as were used for Fig. 3, but now arranged to show the variation of the ozone content with latitude. Note the rapid increase towards polar regions in spring.

large range in high latitudes. Moreover, the total amount of ozone is, in general, least at the Equator and greatest in high latitudes.

It is, however, the changes in the ozone content of the atmosphere with weather conditions that is of the greatest interest. Figs. 5 and 6 show a typical depression and anticyclone, the thin continuous lines being isobars. The thick broken lines indicate the ozone distribution in these two pressure systems. The figures do

not relate to any one particular case, but are the result of measurements on a large number of occasions. Any one particular depression or anticyclone may show minor differences, but the same general features are found in nearly every case.

The amount of ozone in the atmosphere is measured by spectroscopic observations of sunlight, in which the amount of the absorption of the ultra-violet light by the ozone is determined. To obtain the results used to construct Figs. 5 and 6 a number of spectrographs were made and distributed over Europe and spectra were taken of sunlight on every day that the sun was visible. Naturally, on many of the days when, from the meteorological conditions, we

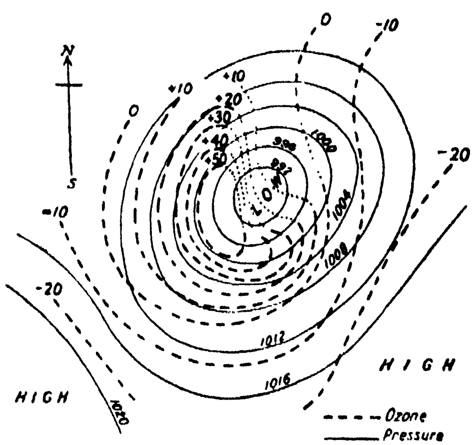

FIG. 5.—Ozone in cyclonic areas.

The continuous lines are drawn to represent a typical cyclonic depression of middle latitudes. The thick broken lines show the distribution of ozone: plus values show that the ozone is above the normal, while minus values show that it is below the normal. Note the marked concentration of ozone to the west and south-west of the centre of the depression.

should most have liked to have measurements, the sun was completely hidden by clouds and observations were impossible. More recently another instrument has been made by which the amount of ozone can be obtained on almost any day with great ease. Unfortunately these new instruments are expensive and it has not yet been possible to make a number of them and to have measurements made regularly at a number of places in Europe in order to study in detail the connection between the amount of ozone and the weather conditions. Such a study might reveal the real nature of this connection and be of great value in weather forecasting.

Already we know that the amount of ozone is very closely associated with many meteorological conditions in the upper air. When

the Troposphere is warm the ozone content is usually high and vice versa. It is also closely connected with the pressure of the air at great heights, the amount of ozone being small when the pressure is high. The closest connection yet found is between the amount of ozone and the density of the air at a height of about 18 kilometres,

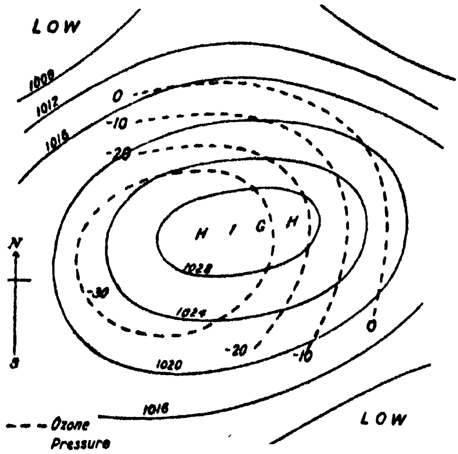

FIG. 6.—Ozone in anticyclonic areas.

The continuous and dotted lines are as for Fig. 5. Note that the ozone values are low over the whole area.

or—what is nearly the same thing—the amount of heat the air has absorbed. The reason for these connections is not known at present, but they may clearly have an important bearing on meteorology when they are thoroughly understood. At present further progress is largely dependent on money with which to make the necessary instruments.

Electrical Characteristics of the Upper Atmosphere.

Results from Terrestrial Magnetism.—The first suggestion that the upper atmosphere was a good conductor of electricity resulted from a study of the magnetic field of the Earth. Accurate measurements of the strength and direction of the magnetic force of the Earth show that this force is not constant but varies both in strength and direction. These variations which are only small and require delicate instruments to show them, can be divided into two quite distinct classes. In the first class are the regular diurnal and annual variations, while in the second are much larger irregular fluctuations which occur occasionally and which, when very large, are known

as magnetic storms. While the main permanent magnetism of the Earth appears to have its origin within the Earth, these variations are due to currents very high above the Earth's surface.

One of the simplest magnetic elements to measure is the declination, or the angle between the geographical and magnetic north. A sensitive compass needle can easily be made by suspending a bar magnet by a single thread so that it is quite free to turn in any direction. If a small mirror be attached to it and a beam of light reflected by the mirror on to a scale, very small movements of the magnet can be seen. With such an instrument it can be shown that on many days the magnet goes through a regular movement which is repeated each day, reaching a maximum in one direction in the morning and a maximum in the other direction in the afternoon, while it moves but little through the night. Such days are known as magnetically quiet days. The total movement of the magnet is quite small, being only about a sixth of a degree in summer and only about $\frac{1}{20}$th of a degree in winter in England. If other characteristics of the Earth's magnetic field are measured, such as the total intensity of the field or the angle between its direction and a horizontal surface, the same type of effects will be observed.

On other days there will be pronounced irregular variations of the magnetic force which affect the whole world. These latter fluctuations are much stronger in Polar regions than in low latitudes and there is now little doubt that they owe their origin to streams of charged particles shot out from the sun. The reason that they are felt more in high latitudes is that the magnetic field of the Earth deflects the charged particles so that they strike the earth only in the higher latitudes, where they produce a visible effect, namely the aurora.

Both the regular and irregular variations of the earth's magnetic field are connected with sunspots. Sunspots are dark markings on the surface of the sun which can easily be seen with a small telescope, while a large spot may occasionally be seen with the eye. The exact nature of sunspots is at present unknown, but they indicate in some way the activity of the sun, and go through a fairly regular cycle of about eleven years length. Thus, in some years there may be a large number of spots so that nearly every day one or more spots can be seen. Then the number will decrease during the next years and about six years afterwards no spots may be seen for many days running. Later still the spots will become more frequent and in another five years or so will have reached their maximum. Sunspots are carried across the sun's disc as the sun rotates on its axis,

so that a spot which lasts sufficiently long will reappear every 26 days, this being the time taken by the equatorial part of the sun to rotate, as seen from the Earth.

The connection between the irregular variations of the Earth's magnetism and sunspots is seen in the fact that magnetic disturbances tend to recur at intervals of 26 days. Also the number of magnetic disturbances increases during those years when there are many sunspots. Certain individual magnetic disturbances seem to be associated with definite spots.

The regular changes in the Earth's magnetic field also show a connection with sunspots. Thus, the average amplitude of the regular daily swing of the compass needle for any year is most closely associated with the average number of sunspots for that year. We believe that the regular diurnal magnetic variations are

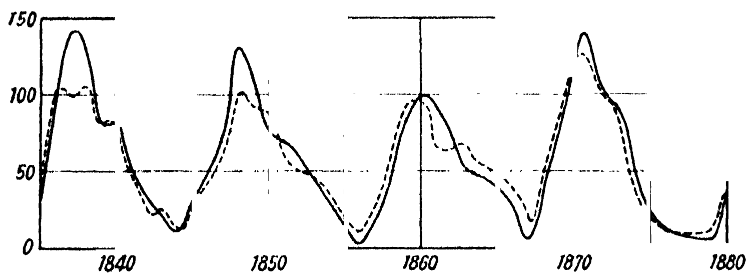

FIG. 7.—Relation between terrestrial magnetism and sunspots.

The continuous line shows the changes in the mean annual sunspot number, while the dotted line shows the mean annual range of the daily swing of the compass needle. The latter may be taken to indicate also the conductivity of the upper atmosphere. Note that the eleven-year period in both cases is marked, but not quite regular. Note also the close connection between the two phenomena.

connected with changes in the conductivity of the upper air and that this conductivity is due to the action of the sun's ultra-violet light on the upper air, whereby free electrons are produced. There is a small lunar diurnal variation of the earth's magnetic field which is apparently due to tides set up by the moon in the upper air. These tides should have no effect on the magnetic field unless the air is a conductor, and it has been shown that during the night when sunlight is cut off, the lunar tides have no effect, their presence only being seen during the hours of daylight.

Results from Radio Measurements.—While the study of terrestrial magnetism gave the first indication that the upper air was an electrical conductor, our knowledge has been rapidly extended in recent years by the observations of radio waves. It would be impossible to send wireless signals to distant parts of the Earth if the wireless waves were not bent round to follow the Earth's surface. The fact that wireless waves are bent round the Earth is

due to the existence of the electrically conducting region at a great height, which bends these radio waves downwards in much the same manner that the upper warm region bends the sound waves. If a very short signal be sent out from a transmitter, a neighbouring receiver will receive the signal by the direct path along the ground and shortly afterwards may receive another signal by means of waves which have travelled straight up into the atmosphere and have been reflected back again by the upper conducting region. Since we know how fast wireless waves travel, it is possible to measure to what height they have been before being reflected back again.

The conductivity of the upper atmosphere is due to the presence of free electrons, formed, as we have said, by ultra-violet light from the sun The concentration of these electrons can also be found because it requires more electrons to reflect back a short wave-length signal than a long wave-length signal. If a series of tests are made in which the wave-length of the signal is gradually reduced, we find that at first the height to which the signal goes before reflection, slowly increases. This is because it has to go somewhat higher before it reaches a place where there are enough electrons to reflect it, showing that the concentration of electrons increases with height. But as the wave-length of the signal is still further reduced, something new is found, for instead of the signal being reflected a little higher, it is now reflected at a very much greater height and no signals are reflected from the intermediate height. This shows that there are two regions where the concentration of electrons is high. The first is at a height of about 100 to 150 kilometres and the second from 200 to 400 kilometres, according to conditions.

Such experiments with radio waves show that the number of free electrons in the upper atmosphere—and hence its conductivity—varies greatly through the day. During the night the lower conducting layer (100 to 150 kilometres) is largely absent since the electrons quickly attach themselves to air molecules and no more are formed. About sunrise the number of electrons rapidly increases and remains large through the day. As would be expected the number of electrons present during the daytime is greater in summer than in winter. The daily variations of the number of free electrons in the upper conducting region (200 to 400 kilometres) are less regular than those of the lower region. During the night the free electrons here also attach themselves to air molecules so that their number decreases, but more slowly than those lower down because of the reduced density of the air. Shortly before sunrise on a

winter day a radio signal may have to go to a height of 400 kilometres before it is reflected back. In such conditions signals of still shorter wavelength may not find enough free electrons at any height and consequently will not be reflected back at all.

While much of the conductivity of the upper atmosphere is due to electrons formed by the sun's ultra-violet light, the charged particles from the sun, which cause magnetic storms, also form free electrons. The electrons formed by this means are found largely at a height of a little above a hundred kilometres, *i.e.* at much the same level as the lower layer formed by sunlight. In the middle of the night when ultra-violet sunlight is cut off, a strongly conducting region may suddenly be formed at a height of about 100 kilometres at times of magnetic disturbance.

The charged particles which cause magnetic storms travel from the sun to the earth much more slowly than light, hence it has been possible, at the time of an eclipse, to establish definitely that ultra-violet light and not charged particles is responsible for most of the conductivity regularly present in both the 100 to 150 kilometre and probably also the 200 to 400 kilometre region, in temperate latitudes. In Polar regions, as might be expected, the effect of charged particles is much more marked and much more frequent.

It has been suggested that the electrical conductivity of the upper atmosphere is dependent to some extent on the weather conditions at the Earth's surface: also that thunderstorms play an appreciable part. We are not yet, however, in a position to state anything very definite about this and further observations are needed.

Auroræ.—While observations of the auroræ have, as yet, provided little additional information about the state of the upper atmosphere they must be mentioned since they are clearly due to the same stream of charged particles which causes magnetic storms. The aurora is very closely related to magnetic storms, bright auroræ being usually seen at times of magnetic disturbance.

From the work of Professor Störmer in Norway and others, we now know accurately the heights of auroræ. The heights are measured by taking simultaneous photographs of the aurora from two distant stations. It is found that while the tops of the rays may go up to great heights—400 kilometres or more—there is a much sharper boundary at the bottom, at a height of about 100 kilometres. It will be remembered that radio measurements showed that the conductivity due to charged particles was most marked at just about this level.

ARTIFICIAL RADIOACTIVITY

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1. Introductory

At the beginning of 1934 about 40 elements were known to be radioactive; of these all except three, potassium, rubidium, and samarium, had atomic numbers greater than 80. The radioactivity of these elements was a natural phenomenon: all specimens of a particular element, no matter what their origin, decayed at a characteristic rate and gave characteristic products. This position has undergone a remarkable change within the last eighteen months. By suitable treatment it has been found possible to stimulate radioactivity in most of the common elements from carbon with atomic number 6 to uranium [1] with atomic number 92. These new activities can be excited at will in inactive specimens of an element and, in many cases, one kind of atom can be made to yield different radioactive products by suitable changes in the agents used to excite the activity. Hence it seems legitimate to describe the new phenomena as "artificial radioactivity," although it has often been convenient to use the radiations from natural radio-elements as exciting agents.

Radioactivity is a nuclear change and the constancy of the rate of decay of the natural radio-elements over a wide range of temperatures shows that a very large amount of energy is needed to affect the behaviour of a nucleus. Since nuclei are very small this energy must also be concentrated into a small volume. The most concentrated form of energy available in the laboratory is furnished by α-particles, and it was by the aid of these particles that nuclei were first disintegrated. In 1919 Rutherford first showed that nitrogen atoms could be disintegrated; the nuclear reaction which took place was expressed by the equation [2]

$$_7N^{14} + {_2He^4} \longrightarrow {_8O^{17}} + {_1H^1}.$$

[1] Uranium is excited by neutrons to give a β-ray activity which is superimposed on its natural α-ray activity.

[2] In this and later equations the nuclear charge is indicated by the subscript number on the left of the symbol and the nuclear mass by the superscript number on the right.

The protons were emitted with a high velocity and were detected by the scintillations they produced on impact with a screen of zinc sulphide. Later Rutherford and Chadwick found that bombardment with α-particles of most of the light elements from boron to potassium gave an emission of fast protons. In 1925 Blackett succeeded in photographing the disintegration of nitrogen atoms in the Wilson cloud chamber and from measurements of the tracks showed that the equation given above represents the reaction accurately.

During the last ten years a large amount of information about nuclear processes has been obtained from experiments of this kind using α-particles and later, artificially accelerated protons and deuterons as projectiles. In all the cases studied the interaction between the colliding nuclei took place in a time which was too short to measure and the products of these reactions were stable elements. Thus Blackett's photographs of the nitrogen disintegration showed that the proton is emitted within 10^{-9} second of the impact of the α-particle and that the product O^{17} does not disintegrate within a time of the order of 10^{-2} second. It is now known, of course, that O^{17} is a rare but stable isotope of oxygen.

In 1932 two new fundamental particles were discovered, the neutron by Chadwick and Joliot, and the positive electron by Anderson and by Blackett and Occhialini. Both have played an important part in the discovery of artificial radioactivity. The positron was first observed during the study of cosmic rays ; it was soon shown, however, that positrons are produced when γ-rays are absorbed by heavy nuclei, *e.g.* lead, and by the impact of α-particles on boron and aluminium. The latter reaction was studied by Curie and Joliot, who found that neutrons and positrons were produced in addition to the protons noted by Rutherford. In 1934 Curie and Joliot found that the positron emission did not cease at once when the exciting source of α-particles was removed but decayed slowly, following an exponential law. With aluminium the activity fell to half-value in 3·25 minutes, with boron the half-life was 14 minutes. It was also found that the activity increased exponentially to a limiting value when inactive specimens of boron and aluminium were exposed to α-particles for increasing periods of time. It was clear from these results that the products emitting positrons were unstable elements which decayed in a measurable time, in other words radioelements produced by the action of α-particles.

This fundamental observation was quickly followed by the discovery of other artificial radioactivities excited by α-rays or by swift protons or deuterons. A few months later Fermi found that

the neutron was a most powerful agent for exciting stable nuclei to give radioactive products and by this method alone more than 40 new radioelements have been obtained. The study of this large number of new activities is still only in its initial stages, but it has already given us a great deal of information about nuclear processes.

2. Detection and Measurement of Feeble Radioactivities

The distinction between radioactivity and disintegration in a time which is too short to measure has probably no fundamental significance. Both involve the disruption of a nucleus and we are familiar with α-ray changes of the natural radioelements which have very small half-lives. The discovery of artificial radioactivity has, however, great practical importance, since a radioactive element with a measurable rate of decay can be detected and identified by comparatively simple experimental methods.

All the artificial radioelements so far examined emit positive or negative electrons, and no case has yet been found in which a heavy particle (α-ray or proton) is the product of a disintegration which takes place after a measurable period of time. The intensity of the activity depends, of course, on the strength of the exciting source. With 100 millicuries of radon in contact with beryllium as neutron source a few elements can be excited to an activity which is about one-tenth that of uranium oxide in equilibrium with uranium X, but most elements give much weaker activities.

The instruments available for studying these feeble activities are the ionisation chamber, the Wilson cloud chamber, and the Geiger-Müller tube counter. Special forms of ionisation chambers in conjunction with sensitive electrometers have been used to study some of the stronger activities, and the Wilson chamber in a magnetic field has given valuable information about the sign of the particles and their energy. The Geiger-Müller tube counter is, however, the instrument which has played the most important part in this work, since it gives a very high sensitivity and enables observations to be made rapidly and continuously. A suitable form of this instrument consists of a cylinder about 1·5 cm. in diameter and 8 cm. in length, made of metal foil thin enough to be penetrated by the β-rays. The ends are closed by gas-tight insulating end-pieces, which carry a fine tungsten wire stretched along the axis of the cylinder. A small glass tube passes through one of the end-pieces, so that the cylinder can be evacuated, filled with dry air at about 8 cm. pressure and sealed off. The outer wall is maintained at a

suitable negative potential, usually of the order of 1,000 volts, and the central wire connected through a small condenser to the grid of the first valve of a simple amplifier and a recording system. The latter may be a gas-filled relay which operates a mechanical counter. The operation of the counter tube may be briefly described as follows. If the potential difference between tube and wire is a little above a certain critical value, any ions produced inside the tube, for example by the entry of a β-particle, are rapidly attracted to the wall or the wire according to the sign of their charge. The negative ions in the neighbourhood of the wire are in a region of very high potential gradient and attain a speed which rapidly causes the production of a large number of new ions by collision with gas molecules. Thus the entry of a single β-particle produces a momentary discharge from the wire of considerable amplitude. This impulse is transmitted through the amplifier and operates the mechanical counter.

For the detection and measurement of β-rays the specimen is mounted as a thin cylinder close to the outer walls of the tube. With this arrangement a considerable proportion (usually about one-twentieth) of the β-particles emitted in all directions pass through the tube and are recorded. The sensitivity of the instrument may be illustrated by the following figures. One milligram of uranium oxide in equilibrium with uranium X gives about 40 kicks per minute due to the penetrating β-rays from uranium X2. (The β-rays from uranium X1 are too soft to penetrate the walls of the tube.) Since a milligram of uranium is equivalent to 3×10^{-7} milligrams of radium, it will be realised that great care has to be taken to avoid contamination of the counter or specimen with traces of strongly radioactive substances.

Once the counter has been set up and the potential on the tube adjusted, observations can be made with great ease and rapidity. Not only can radioactivity be detected and measured, but a particular product can be identified by its rate of decay. If the half-life is not too short, the atomic number of a radioactive product can often be determined by carrying out chemical separations with small amounts of known elements and observing which substance carries the activity after precipitation, etc. The value of this chemical evidence will be appreciated from some of the examples discussed later.

3. Activation by Positively Charged Particles

Curie and Joliot obtained the first artificial radioelements by bombarding boron and aluminium with α-rays from polonium.

The disintegration of aluminium may be represented by the following scheme.

$$ {}_{13}\mathrm{Al}^{27} + {}_{2}\mathrm{He}^{4} \to {}_{1}\mathrm{H}^{1} + {}_{14}\mathrm{Si}^{30} \quad . \quad . \quad . \quad (1) $$

$$ {}_{13}\mathrm{Al}^{27} + {}_{2}\mathrm{He}^{4} \to {}_{0}n^{1} + {}_{15}\mathrm{P}^{30} \xrightarrow{e+} {}_{14}\mathrm{Si}^{30} \quad . \quad . \quad . \quad (2) $$

Reaction (1), which may be termed the Rutherford reaction, accounts for the emission of protons ; the other product is a known stable isotope of silicon. Curie and Joliot had also observed the emission of neutrons. If this reaction is set out formally, as in (2), by balancing the equation for masses and charges, the other product is an unknown isotope of phosphorus P^{30}, which may disintegrate slowly with the emission of positrons to give the same Si^{30} as reaction (1).

This scheme is supported by chemical evidence. If irradiated aluminium is dissolved in hydrochloric acid, the hydrogen, collected in a thin-walled glass tube, is found to carry with it the activity probably in the form of phosphine. When a mixture of hydrochloric and nitric acids is used as solvent the activity remains in the solution. Both these reactions may, of course, be ascribed to an isotope of silicon which also forms volatile hydrides. A more distinctive test is based upon the insolubility of zirconium phosphate in acid solutions. Irradiated aluminium is dissolved in nitric and hydrochloric acids, a little sodium phosphate added to give a visible precipitate and then a solution of a zirconium salt. The activity is found entirely in the precipitate.

The yield in these nuclear reactions is very small. Only 1 in 10^5 of the α-particles used brings about disintegration, and reaction (1) occurs more frequently than reaction (2). The branching ratio is about 20 : 1 for the aluminium disintegration.

A number of the lighter elements have given radioactive products when bombarded with α-particles ; the half-lives and other data for these elements are collected in Table I. All but one of these products emit positrons ; with magnesium two radioelements are obtained, one of which emits positrons, and the other electrons. These are ascribed to the disintegration of two isotopes of magnesium according to the following scheme :

$$ {}_{12}\mathrm{Mg}^{24} + {}_{2}\mathrm{He}^{4} \longrightarrow {}_{0}n^{1} + {}_{14}\mathrm{Si}^{27} \xrightarrow{e+} {}_{13}\mathrm{Al}^{27} \quad . \quad . \quad . \quad (3) $$

$$ {}_{12}\mathrm{Mg}^{25} + {}_{2}\mathrm{He}^{4} \longrightarrow {}_{1}\mathrm{H}^{1} + {}_{13}\mathrm{Al}^{28} \xrightarrow{e-} {}_{14}\mathrm{Si}^{28} \quad . \quad . \quad . \quad (4) $$

Reaction (3) with Mg^{24} is precisely similar to reaction (2) with

aluminium. Reaction (4) with Mg^{25} consists of the emission of a proton as in the Rutherford reaction, but the residue is a new isotope of aluminium which undergoes radioactive decay, emitting electrons. The same isotope, Al^{28}, decaying with the same period, was later obtained by Fermi in a number of disintegrations provoked by neutrons.

During the last few years great advances have been made in the production and handling of high voltages, and beams of protons and deuterons with very high velocities and energies of the order of a million electron volts can be generated in the laboratory. By their aid a number of disintegrations have been studied, and in some of them radioactive products have been obtained. When boron is bombarded with high-energy deuterons, a branched disintegration is again found ; one of the products is a radioactive isotope of carbon which emits positrons and decays with a half-life of 20 minutes. The nuclear reactions appear to follow the scheme shown in equations (5) and (6).

$$
{}_5B^{10} + {}_1H^2 \begin{array}{ll} \nearrow\ {}_1H^1 + {}_5B^{11} & \quad \ldots \ldots \ldots (5) \\ \searrow\ {}_0n^1 + {}_6C^{11} \xrightarrow{e+} {}_5B^{11} & \quad \ldots \ldots (6) \end{array}
$$

With carbon both protons and deuterons give the same product, N^{13}, which emits positrons and decays with a half-life of 14 minutes.

$$
{}_6C^{12} + {}_1H^1 \longrightarrow {}_7N^{13} \xrightarrow{e+} {}_6C^{13} \quad \ldots \ldots (7)
$$

$$
{}_6C^{12} + {}_1H^2 \begin{array}{ll} \nearrow\ {}_1H^1 + {}_6C^{13} & \quad \ldots \ldots (8) \\ \searrow\ {}_0n^1 + {}_7N^{13} \xrightarrow{e+} {}_6C^{13} & \quad \ldots \ldots (9) \end{array}
$$

The ultimate product of reactions (5) to (9) is an isotope of the original element with an extra unit of mass. With carbon the product, C^{13}, is a known stable isotope, but with other elements, and particularly those of odd atomic number, the product may be radioactive. Thus sodium bombarded with high-energy deuterons gives Na^{24} which emits negative electrons and decays with a half-life of 15·5 hours.

$$
{}_{11}Na^{23} + {}_1H^2 \longrightarrow {}_1H^1 + {}_{11}Na^{24} \xrightarrow{e-} {}_{12}Mg^{24} \quad \ldots (10)
$$

Recently Lawrence, using a beam of deuterons with an energy of

nearly 2,000,000 electron volts, has obtained this radioactive sodium with a very high activity approaching that of preparations of radium. It seems possible that developments in the technique of high-voltage work may, before long, provide intensely active preparations of artificial radioelements which can be used instead of radium for medical and scientific purposes. It should be noted that Na^{24} with identical properties is produced in a number of the disintegrations provoked by neutrons (Table II).

Up to the present it has not been found possible to disintegrate the nuclei of elements with atomic numbers greater than 19 by bombardment with positively charged particles. This is explained by the rapid increase with increasing atomic number of the electrostatic forces of repulsion between the nucleus and the approaching positively charged particle. Modern developments in the theory of the nucleus seem to indicate, however, that the limit has not yet been reached at which high nuclear charge prevents interaction with an α-particle. Recent studies of the bombardment of aluminium and other elements with α-particles of a range of known energies have revealed some remarkable phenomena of interaction with comparatively low energy particles. The most abundant production of protons in the Rutherford reaction was obtained by using α-particles with energies greater than that of the potential barrier which surrounds the aluminium nucleus, but a number of swift protons were also found with particles of lower energy. If the number of

TABLE I

ARTIFICIAL RADIOELEMENTS EMITTING POSITRONS

Element.	Half-life.	Origin.	Observer.
${}_5B^9$	?	${}_3Li^6 + {}_2He^4 \rightarrow {}_0n^1 + {}_5B^9$	M.
${}_6C^{11}$	c. 20 min.	${}_5B^{10} + {}_1H^2 \rightarrow {}_0n^1 + {}_6C^{11}$	C., G. & W.; L., C. & H.
${}_7N^{13}$	14 min.	${}_5B^{10} + {}_2He^4 \rightarrow {}_0n^1 + {}_7N^{13}$	C. & J.
		${}_6C^{12} + {}_1H^1 \rightarrow {}_7N^{13}$	C., G. & W.
		${}_6C^{12} + {}_1H^2 \rightarrow {}_0n^1 + {}_7N^{13}$	
${}_9F^{17}$	1·1 min.	${}_7N^{14} + {}_2He^4 \rightarrow {}_0n^1 + {}_9F^{17}$	W.
${}_{11}Na^{22}$	7 sec.	${}_9F^{19} + {}_2He^4 \rightarrow {}_0n^1 + {}_{11}Na^{22}$	C. & J.
${}_{13}Al^{26}$	7 sec.	${}_{11}Na^{23} + {}_2He^4 \rightarrow {}_0n^1 + {}_{13}Al^{26}$	F.
${}_{14}Si^{27}$	?	${}_{12}Mg^{24} + {}_2He^4 \rightarrow {}_0n^1 + {}_{14}Si^{27}$	C. & J.
${}_{15}P^{30}$	3·2 min.	${}_{13}Al^{27} + {}_2He^4 \rightarrow {}_0n^1 + {}_{15}P^{30}$	C. & J.
${}_{17}Cl^{34}$	40 min.	${}_{15}P^{31} + {}_2He^4 \rightarrow {}_0n^1 + {}_{17}Cl^{34}$	F.
${}_{21}Sc^{42}$	180 min.	${}_{19}K^{39} + {}_2He^4 \rightarrow {}_0n^1 + {}_{21}Sc^{42}$	Z.

M., Meitner, quoted by Curie and Joliot; C., G. & W., Cockcroft, Gilbert and Walton, *Nature*, 1934, **133**, 328; L., C. & H., Lauritsen, Crane and Harper, *Science*, 1934, **79**, 324; C. & J., Curie and Joliot, *Int. Conference Phys.*, 1935, I, 78; W., Wertenstein, *Nature*, 1934, **133**, 565; F., Frisch, *ibid.*, 721; Z., Żwy, *ibid.*, **134**, 64.

swift protons expelled is plotted against the energy of the α-particles, several maxima are found at lower energy values. It appears that α-particles with certain critical energies can pass *through* the potential barrier and interact with the nucleus. The chance of disintegrations achieved in this way is small, but this may be compensated by the large number of high-energy protons which can be used as projectiles with the new technique developed by Lawrence, and by Cockcroft and Walton. One may therefore anticipate that the present limit of atomic number for disintegration by positively charged particles will soon be overstepped. At the same time, it seems unlikely that the methods available at present will be capable of disintegrating the heavier elements. This question is of theoretical interest, since at present bombardment by positively charged particles is the only method which gives radioelements emitting positrons.

4. Activation by Neutrons

Since a neutron does not carry an electrical charge, it may be expected to interact with the nuclei of both light and heavy elements. This supposition has been amply confirmed by the remarkable work of Fermi and his collaborators during the last year. More than 40 new radioelements have been obtained with atomic numbers ranging from 7 to 92, and all emit negative electrons or β-rays. In addition, a number of elements which do not give radioactive products show remarkable phenomena of absorption of neutrons and emission of γ-rays. The neutron is clearly a most potent agent for initiating nuclear transformations.

The technique used is exceedingly simple. A convenient source of neutrons consists of a small glass tube containing powdered beryllium in contact with radon. Fermi has worked with quantities of the order of 500 millicuries of radon, but some of the activities detected are so intense that they can be detected and measured with sources of much smaller intensity. Neutrons are expelled from the beryllium by the impact of α-particles from the radon and its disintegration products, probably by the reaction

$$_4\mathrm{Be}^9 + {}_2\mathrm{He}^4 \longrightarrow {}_0n^1 + {}_6\mathrm{C}^{12}.$$

The substance to be examined is placed as close to the neutron source as possible and is irradiated for a time depending upon the half-life of the product. The activity produced and the rate of decay are then measured by means of a Geiger-Müller counter, or, if the product has a high activity, by means of an ionisation chamber and electrometer.

Four main types of interaction between neutrons and nuclei have been recognised. The first and simplest consists of the capture of a neutron to give a *stable* isotope with one unit of mass greater than the parent nucleus. This may be formulated generally as follows:

$$_{Z}A^{W} + {_0}n^1 \longrightarrow {_Z}A^{W+1}$$

The extra energy carried into the nucleus by the neutron is emitted as a γ-ray. Yttrium and cadmium behave in this way. They show a very large absorption for slow neutrons and at the same time emit γ-rays. They do not form radioactive products with a measurable rate of decay.

The second type of reaction consists of the capture of a neutron followed by an immediate disruption of the nucleus into stable products. Boron undergoes this type of reaction. Fermi showed that boron was a powerful absorber of slow neutrons, but did not emit either β- or γ-rays. Chadwick and Goldhaber, using a special type of counter and amplifier suitable for detecting α-rays, showed that heavy particles were emitted when boron and lithium were bombarded with neutrons. They suggest that the products are given by the following equations:

$$_{3}Li^{6} + {_0}n^1 \longrightarrow {_2}He^4 + {_1}H^3$$
$$_{5}B^{10} + {_0}n^1 \longrightarrow {_2}He^4 + {_2}He^4 + {_1}H^3$$

One of the products of these disintegrations is a still heavier isotope of hydrogen of mass 3. This interesting isotope has been found in other nuclear transformations, notably in the bombardment of deuterons by deuterons.

$$_{1}H^{2} + {_1}H^2 \longrightarrow {_1}H^3 + {_1}H^1$$

The third type of reaction of neutrons with nuclei consists of the capture of the neutron to form a radioactive nucleus. This type of reaction occurs with light and heavy nuclei and has produced most of the new radioelements. For example, Fermi found that iodine irradiated with neutrons gave a product which emitted β-rays and decayed with a half-life of 25 minutes. Chemical separations showed that the radioactive product was isotopic with iodine. Thus ammonium iodide after irradiation was dissolved in water, soluble antimony and tellurium compounds added, and the iodine precipitated either as silver iodide or by oxidation with nitric acid. The radioactivity followed the iodine and was not associated with the elements of neighbouring atomic numbers. The reaction was therefore formulated

$$_{53}I^{127} + {_0n^1} \longrightarrow {_{53}I^{128}} \xrightarrow{e^-} {_{54}Xe^{128}}.$$

The product of the decay is a known stable isotope of xenon.

The neutrons emitted from a radon-beryllium source are not all of one velocity, but have kinetic energies covering a wide range up to 8,000,000 electron volts. Fermi and his collaborators have found that the β-ray activities produced by the capture of a neutron are very greatly increased in intensity if, during irradiation, the specimen and source are surrounded by a large mass of a hydrogen compound. (In practice one uses a large tank of water or a block of paraffin wax.) This effect is ascribed to a rapid reduction in the velocity of the neutrons by collisions with hydrogen nuclei; the process continues until the velocity approaches that of a gas molecule. The slow neutrons will be captured more readily than those with a high velocity and thus give a higher activity in the specimen. Fermi terms this the "hydrogen effect" and measures it roughly by a coefficient α, which is the factor by which the activity of a specimen is increased when it is surrounded during irradiation by a large mass of water. Values of α range from uranium $\alpha = 1{\cdot}6$, iodine $\alpha = 7$, silver (20 sec. period) $\alpha = 30$ to vanadium (4 min. period) $\alpha = 40$.

If the element bombarded by neutrons contains two isotopes, both may be activated and two products with different periods of decay may be produced. Thus silver with isotopes Ag^{107} and Ag^{109} gives two periods with half-lives of 20 seconds and 2·3 minutes. These are ascribed to unstable isotopes Ag^{108} and Ag^{109}. It is not possible at present to determine which isotope gives the longer and which the shorter period. Similarly bromine gives two periods of 20 minutes and 4·5 hours, which are ascribed to Br^{80} and Br^{82}.

Szilard and Chalmers have devised an interesting method of concentrating the radioactive isotopes of certain elements. A typical example is furnished by the concentration of radioactive iodine. Ethyl iodide containing a trace of free iodine was irradiated with neutrons. The free iodine was separated and was found to contain most of the radioactivity. The explanation is as follows. The impact of the neutron on the iodine atom disrupts the ethyl iodide molecule and leaves a residue, probably an ethyl radical, which soon recombines with free iodine to re-form ethyl iodide. Since, however, the concentration of inactive free iodine is immensely larger than that of radioactive iodine, it is the former which combines with the organic residue. The radioactive isotope is left to be extracted with the free iodine at the end of the experiment, and if

only a small amount of free iodine is used, a considerable degree of concentration of the active product can be achieved. This method was also used to concentrate the radioactive isotopes of bromine; later Fermi and his collaborators used it to concentrate the active isotopes of chlorine, manganese and arsenic.

In one case at least there seems to be evidence for a series of successive β-ray changes. Uranium, which contains only the isotope of mass 238 in appreciable amount, gives no less than four periods of decay with half-lives of 10 seconds, 40 seconds, 13 minutes and 90 minutes. Fermi has obtained chemical evidence which indicates that the product giving the 13 minutes' period is not isotopic with any of the elements with atomic numbers 86 to 92. This evidence has been criticised by von Grosse and Agruss, but the criticisms have been answered by Fermi. It is clearly possible to account for a number of periods by a series of disintegrations of the type

$${}_{92}U^{238} + {}_{0}n^{1} \longrightarrow {}_{92}U^{239} \xrightarrow{e^-} {}_{93}U^{239} \xrightarrow{e^-} {}_{94}U^{239} \text{ etc.}$$

At the same time it must be remembered that some of the naturally radioactive elements undergo a branching disintegration. Much more experimental work is needed on this very interesting problem.

The fourth type of reaction of neutrons with nuclei consists of the capture of a neutron followed immediately by the expulsion of a positively charged particle and the formation of a radioactive residue. The positive particle which is expelled may be either a proton or an α-particle. Thus magnesium reacts as follows:

$${}_{12}Mg^{24} + {}_{0}n^{1} \longrightarrow {}_{1}H^{1} + {}_{11}Na^{24}:$$

The radioactive product is Na^{24} with a half-life of 15·5 hours. The isotopy of the active product with sodium was tested as follows. Irradiated magnesium oxide was dissolved in acid and a small amount of a sodium salt added. The magnesium was precipitated as phosphate, and was found to be inactive whilst the sodium salt recovered from the filtrate contained the activity.

When aluminium is irradiated with neutrons, the same product is obtained, as well as products which decay more rapidly. The production of Na^{24} from this element involves the expulsion of an α-particle, according to the scheme

$${}_{13}Al^{27} + {}_{0}n^{1} \longrightarrow {}_{2}He^{4} + {}_{11}Na^{24} \xrightarrow{e^-} {}_{12}Mg^{24}$$

Here again the chemical evidence is clear. Irradiated aluminium

was dissolved in acid and small amounts of magnesium and sodium salts added. Aluminium separated as hydroxide and magnesium separated as phosphate were found to be inactive, whilst the sodium salt in the filtrate was found to carry the activity.

It should be noted that aluminium can also react with the emission of a proton, giving Mg^{27} with a half-life of 10 minutes. With slow neutrons aluminium gives Al^{28} with a half-life of 2·3 minutes. Since aluminium bombarded with α-particles gives the positron emitter P^{30} with a half-life of 3·2 minutes it will be seen that

TABLE II

ARTIFICIAL RADIOELEMENTS EMITTING ELECTRONS

Nucleus.	Half-life.	Origin.	Observer.
$_7N^{16}$	9 sec.	$_9F^{19} + {_0n^1} \rightarrow {_2He^4} + {_7N^{16}}$	F.
$_9F^{20}$ (?)	40 sec.	$_9F^{19} + {_0n^1} \rightarrow {_9F^{20}}$	,,
$_{10}Ne^{23}$ (?)	40 sec.	$_{11}Na^{23} + {_0n^1} \rightarrow {_1H^1} + {_{10}Ne^{23}}$	,,
		$_{12}Mg^{26} + {_0n^1} \rightarrow {_2He^4} + {_{10}Ne^{23}}$	
$_{11}Na^{24}$	15·5 hr.	$_{11}Na^{23} + {_0n^1} \rightarrow {_{11}Na^{24}}$	,,
		$_{12}Mg^{24} + {_0n^1} \rightarrow {_1H^1} + {_{11}Na^{24}}$	,,
		$_{13}Al^{27} + {_0n^1} \rightarrow {_2He^4} + {_{11}Na^{24}}$	,,
		$_{11}Na^{23} + {_1H^2} \rightarrow {_1H^1} + {_{11}Na^{24}}$	L.
$_{12}Mg^{27}$	10 min.	$_{12}Mg^{26} + {_0n^1} \rightarrow {_{12}Mg^{27}}$	F.
		$_{13}Al^{27} + {_0n^1} \rightarrow {_1H^1} + {_{12}Mg^{27}}$	,,
$_{13}Al^{28}$	2·3 min.	$_{13}Al^{27} + {_0n^1} \rightarrow {_{13}Al^{28}}$	,,
		$_{14}Si^{28} + {_0n^1} \rightarrow {_1H^1} + {_{13}Al^{28}}$	,,
		$_{15}P^{31} + {_0n^1} \rightarrow {_2He^4} + {_{13}Al^{28}}$	C., J. & P.
		$_{12}Mg^{25} + {_2He^4} \rightarrow {_1H^1} + {_{13}Al^{28}}$	,,
$_{14}Si^{31}$	2·4 hr.	$_{14}Si^{30} + {_0n^1} \rightarrow {_{14}Si^{31}}$	F.
		$_{15}P^{31} + {_0n^1} \rightarrow {_1H^1} + {_{14}Si^{31}}$	,,
$_{15}P^{32}$	14 days	$_{16}S^{32} + {_0n^1} \rightarrow {_1H^1} + {_{15}P^{32}}$	,,
		$_{17}Cl^{35} + {_0n^1} \rightarrow {_2He^4} + {_{15}P^{32}}$	,,
$_{17}Cl^{36}(^{38})$	35 min.	$_{17}Cl^{35}(^{37}) + {_0n^1} \rightarrow {_{17}Cl^{36}(^{38})}$	,,

F., Amaldi, D'Agostino, Fermi, Pontecorio, Rasetti and Segré, *Proc. Roy. Soc.*, 1934, A, **146**, 483; 1935, **149**, 522; L., Lawrence, *Phys. Rev.*, 1935, **47**, 17; C., J. & P., Curie, Joliot and Preiswerk, *Compt. Rend.*, 1934, **198**, 2089.

one stable nucleus, Al^{27}, can by suitable treatment be made to yield four different radioactive nuclei.

The reactions of neutrons with nuclei in which positively charged particles are emitted occur only with the lighter elements, and have not been observed for nuclei with atomic numbers greater than $Z = 21$. It is assumed that the potential barrier which hinders the approach of positively charged particles also resists the expulsion of positively charged particles from the nucleus. Furthermore, these reactions show no hydrogen effect, *i.e.* neutrons of considerable energy are needed to bring about these reactions. It would appear that the neutron must carry a certain amount of

energy into the nucleus; when this occurs the α-particle or proton gains enough energy to surmount the potential barrier and escape.

A detailed table of the products of neutron activation and their properties has recently been given by Fermi and his collaborators (*Proc. Roy. Soc.*, 1935, A, **149**, 554), hence it will suffice to quote here the data for the lighter elements up to Z = 20. These elements give nine radioactive products as shown in Table II. From this table it will be seen that not only can one element give several radioactive products, but that a particular product can be obtained in several ways. Thus Na^{24} has been found as a product of four different nuclear processes.

5. Stable and Unstable Isotopes

It will be evident from this purely descriptive account that the discoveries of the last two years have furnished an immense amount of new experimental data concerning the behaviour of nuclei. The interpretation of this new knowledge and its incorporation in a comprehensive theory of the nucleus presents a number of difficulties; at the same time the relations between the different types of nuclei revealed by the production of artificial radioelements show a number of regularities which may ultimately lead to far-reaching generalisations. It would be out of place here to attempt to give an outline of current theories of the nucleus, but one simple aspect of the general problem may be referred to briefly.

The fundamental constituents of the nucleus are now, following Heisenberg, supposed to be protons and neutrons and not, as in earlier theories, protons and electrons. Probably some of them are combined to form secondary particles such as α-particles which might be formulated p_2n_2, and according to some writers deuterons, pn (p = proton, n = neutron). For our present purpose the total number of protons and neutrons will suffice, and their association into more complex aggregates need not be considered.

The charge of the nucleus is due entirely to the protons, hence the number of protons is given by the atomic number Z. If W is the mass of the nucleus, then the number of neutrons is W − Z. The lighter nuclei contain approximately as many neutrons as protons, but as the atomic number increases an excess of neutrons gradually accumulates. That is, the atomic mass becomes greater than twice the atomic number.

In order to compare different types of nuclei, it is convenient to plot the excess of neutrons over protons, namely W − 2Z against the atomic number Z. This has been done in Fig. 1 for all the known nuclear species up to Z = 20. The numbers give the masses

of the isotopes and those in a vertical column belong to one element which is indicated by the chemical symbol at the top of the diagram. The stable isotopes are indicated by circles and to these are added the positron emitters indicated by squares and the negatron emitters indicated by diamonds. The arrows indicate the end products of the decay of these unstable isotopes.

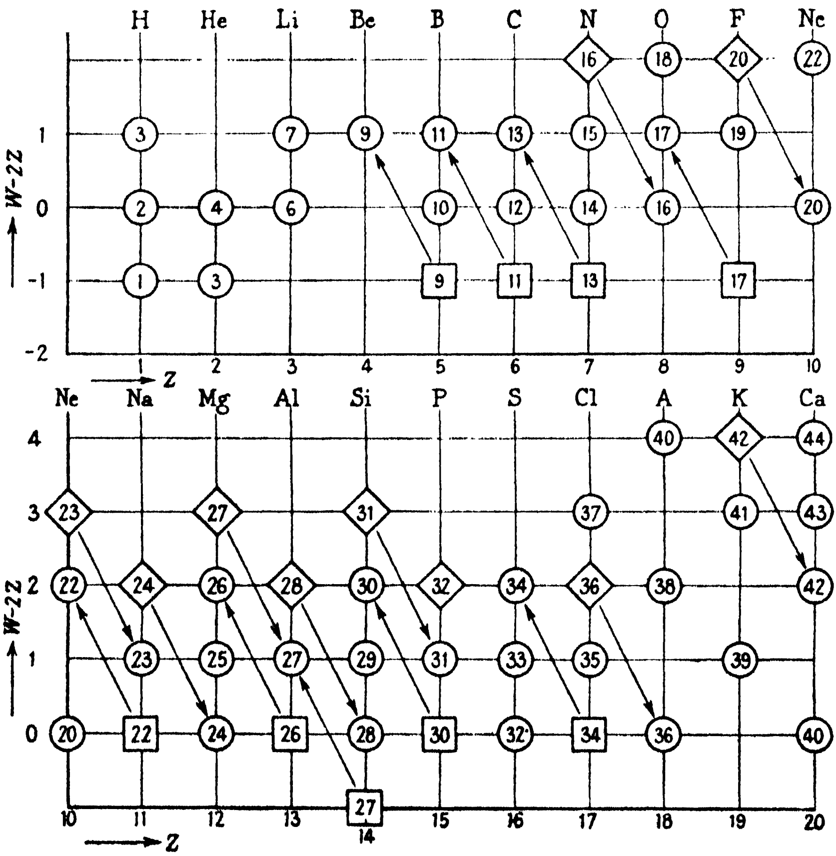

FIG. 1.

Inspection of the diagram reveals a number of remarkable regularities. In the first place all the nuclei which emit positrons contain a *smaller* number of neutrons than the stable isotopes of the same atomic number, whilst the nuclei which emit negatrons (β-rays) contain more neutrons than the stable isotopes of the same element. This indicates strongly that nuclei owe their stability to a balance between the number of neutrons and protons. The nuclear change

giving rise to positron emission may therefore be in all cases the conversion of a proton into a neutron and a positron.

$$ {}_1H^1 \longrightarrow {}_0n^1 + e^+ $$

Similarly, the process giving rise to the emission of a β-ray may be the conversion of a neutron into a proton and an electron.

$$ {}_0n^1 \longrightarrow {}_1H^1 + e^- $$

The diagram exhibits a number of other suggestive regularities. Thus nuclei which contain equal numbers of protons and neutrons are stable up to $Z = 10$ (Be^8 is not known). From $Z = 11$ to $Z = 20$ such nuclei are stable if Z is even, and unstable emitting positrons if Z is odd. The one known positron emitter not included in the diagram, Sc^{42}, falls into this group.

All the nuclei up to $Z = 20$ which have one neutron in excess of the number of protons are stable. In this range of atomic number an excess of two neutrons gives stable nuclei if Z is even and unstable nuclei emitting β-rays if Z is odd. Similar but less complete relations can be traced for elements with $Z > 20$, but in this region only the nuclei which emit electrons are known and can be used to indicate the limits of stability.

One can predict with fair certainty the properties of the nuclei which have not yet been obtained. Thus Be^8, F^{18}, Ne^{21}, A^{37}, A^{39}, and Ca^{41} will probably be stable; O^{15}, Ne^{19}, Mg^{23}, S^{31}, A^{35}, and K^{38} if they are ever produced in nuclear changes will probably be unstable and emit positrons. Similarly O^{19} and S^{35} should decay with the emission of electrons.

Enough has been said to indicate the nature and the limitations of the regularities which can be traced between the *composition* and the stability of nuclei; a complete theory must obviously take into consideration nuclear energies and spins. We are probably not far from a periodic theory of the nucleus in which successive shells of fundamental particles play an important part. At the moment experiment is in advance of theory, but the clue which will solve the puzzle of the nucleus will probably be revealed by further study of artificial radioelements.

HYDRAULIC CEMENTS

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THE most elementary form of cement used in constructional work is to be found in the early Egyptian buildings, in which bricks made from clay dried in the sun were bound together with layers of moist clay. Bitumen was used as a bonding material by the Babylonians and Assyrians, whilst in their later practice the Egyptians introduced the use of mortar composed of a cementing material mixed with sand. The cementing material of the Egyptians was crudely burnt gypsum, and it seems that this was adopted rather than lime because of the much greater ease with which it could be burnt. The use of lime is found in Grecian, and later in Roman, civilisation and the burning of limestone to form quicklime, and its slaking to a lime putty, became a highly developed art.

Limes prepared from pure limestones which are almost free from silica and alumina harden only by atmospheric carbonation and, though mortars made with them eventually become very strong, the process is very slow. Under water such mortars do not harden. Both the Greeks and the Romans were familiar with a very valuable property possessed by some volcanic ash deposits. These materials when mixed in a finely ground condition with a pure lime yield a mixture which will set and harden under water without the assistance of atmospheric carbonation. One such material used by the Romans came from volcanic deposits at Pozzuoli near Naples, and from this source is derived the name *pozzolana*. This term is now applied to a group of materials, none of which are cements, but all of which have this characteristic property of combining with slaked lime in the presence of water to form a hard mass. Vitruvius in his *De Architectura*, written probably before 27 B.C., says of pozzolana [1]: "There is also a kind of powder which, by nature, produces wonderful results. It is found in the neighbourhood of Baiae and in the lands of the municipalities round Mount Vesuvius. This being mixed with lime and rubble not only furnishes strength to other buildings, but also, when piers are built in the sea, they set under water." The

Romans made use of powdered tiles and pottery as pozzolanas when natural volcanic earths were not available, for burnt clays were also found to possess this same pozzolanic property. The interest shown in pozzolanas to-day and their increasing use in certain kinds of constructional work is but another chapter in a long history.

The Italian natural pozzolanas are still widely used in Italy, both in lime mortars and in concrete with Portland cement, and many of the open pits from which they are obtained have been worked continuously for hundreds of years. The German trass which is obtained from a consolidated volcanic ash deposit occurring in the neighbourhood of Andernach on the Rhine is the principal pozzolana of northern Europe. Artificial pozzolanas prepared by the burning of clay by crude methods have long been used in India and Egypt under the names of *surkhi* and *homra*.

Modern investigations on the production of burnt clay pozzolanas have shown that careful control of the burning process is required to obtain the most active products, and that without such control only materials of variable and uncertain activity are likely to be obtained. Cement manufacturers are at the present time directing their attention to the production of these artificial pozzolanas, and are thus reproducing under controlled conditions the materials which the Romans made and used in this country 2,000 years ago.

The mechanism of the reaction of lime with pozzolanas is still unsettled, and it seems questionable whether it is similar in type for natural volcanic ash and burnt clay pozzolanas. Alternative theories of base exchange, of substitution of lime for replaceable hydrogen in aluminosilicate compounds of the nature of Phillipsite, $CaO.Al_2O_3.5SiO_2.5H_2O$, and of direct combination with free and active forms of silica and alumina, still remain controversial issues.

The development of modern cements may be traced back to a discovery made by Smeaton when engaged in the erection of the Eddystone lighthouse in 1756. Smeaton found that on burning limestones containing clay the lime produced had hydraulic properties and would set under water. From Smeaton's discovery to the first production of Portland cement by burning a prepared mixture of limestone and clay a period of some seventy years elapsed, the original patent for Portland cement, that of John Aspdin, being taken out in 1824.

The hydraulic cements of the present day include Portland cement, aluminous cement, and Portland blast-furnace cement as well as hydraulic limes and various mixtures of lime and granulated (quenched) blast-furnace slag, and lime and pozzolana. In addition, pozzolanas are widely used abroad in combination with Portland and

Portland blast-furnace cement ; this latter cement is itself a mixture of Portland cement and granulated blast-furnace slag. It is not possible within the limits of the present article to survey all these cements, and attention must be mainly confined to Portland and aluminous cements.

The necessary constituents of all constructional cements are lime, alumina and silica, and of these only two, lime and silica, are essential for Portland cement, and lime and alumina for aluminous cement. The zones covered by various cement compositions in the ternary system $CaO-Al_2O_3-SiO_2$ are shown in Fig. 1.[1] The Portland cement zone is separated from the aluminous cement zone by a region in

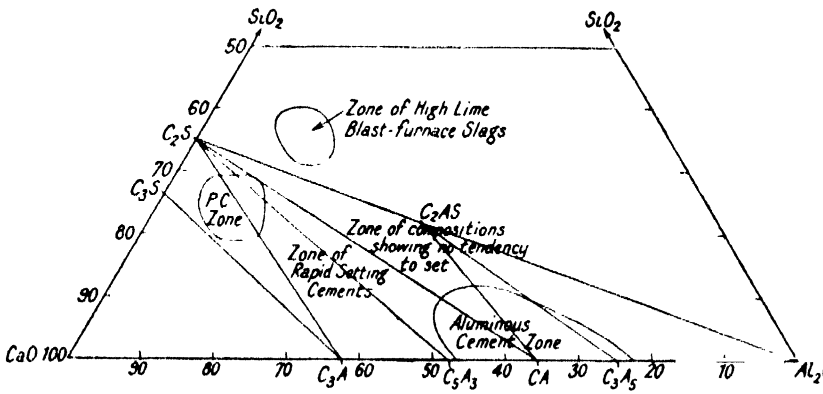

FIG. 1.—Cement zones in system $CaO-Al_2O_3-SiO_2$.

which the cements obtained have such a rapid set as to be useless. The cements in this zone contain a large proportion of the two most basic calcium aluminates $3CaO.Al_2O_3$ and $5CaO.3Al_2O_3$, both of which react with water and set with great rapidity. The ternary compound $2CaO.Al_2O_3.SiO_2$ has no cementitious properties, and compositions in its neighbourhood show no tendency to set with water. On the high silica side of the Portland cement zone the composition zone of the blast-furnace slags of high lime content is found. These slags show little or no cementitious properties when slowly cooled and crystallised, but when chilled very rapidly to a glassy condition they have excellent latent cementing properties. These rapidly chilled, or granulated, slags require the addition of a small proportion of lime, or of an alkali or alkaline earth sulphate,

[1] In both Fig. 1 and Fig. 2 the symbols *C*, *A*, *S* are used to denote the oxides CaO, Al_2O_3, SiO_2 when representing the various compounds formed by these oxides.

to convert them into cements. The mechanism of the setting action in this case is not understood.

Portland cement is commercially by far the most important of constructional cements. In its manufacture the raw materials, such as finely ground limestone and clay, are heated to a temperature which varies from 1,350° to 1,500° C. At this temperature only a minor proportion of the mix, amounting to some 20 to 30 per cent., becomes liquid and it coheres into small pea-sized particles of cement clinker. This clinker, after grinding to a fine powder, with the addition of a small proportion of gypsum to control the speed at which it sets, forms the Portland cement of commerce. In a well-burnt clinker, combination of the lime with the silica, alumina, and other clay constituents is complete and no uncombined lime remains. A certain amount of uncombined lime, varying from zero to a few per cent., is, however, usually found in commercial cements. The composition of commercial cements ranges from about 60 to 67 per cent. CaO, 17 to 25 per cent. SiO_2, 3 to 8 per cent. Al_2O_3, 0·5 to 6·0 per cent. Fe_2O_3, 0·1 to 5·5 per cent. MgO, 0·5 to 1·3 per cent. Na_2O and K_2O, 0·1 to 0·4 per cent. TiO_2, and 1 to 3 per cent. SO_3.

The investigation of the nature of the compounds present in Portland cement can be said to have commenced with the work of Le Chatelier, published from 1883 onwards, but it is only within the last few years that some general agreement as to the constitution of Portland cement has been reached. Present knowledge of the constitution of Portland cement rests on the one hand on the results of phase equilibrium investigations on systems containing the various oxides present in Portland cement, and on the other hand on confirmatory evidence obtained from the study of cements themselves.

Portland cement is composed to the extent of over 90 per cent. of lime, alumina, and silica. The complete equilibrium diagram of this ternary system was worked out at the Geophysical Laboratory of Washington and published in 1915. The results of this investigation form the basis from which the present knowledge of the constitution of Portland cement has been developed. The equilibrium diagram of this system [2], with some later minor corrections, is given in Fig. 2. The compounds which are to be predicted from this diagram in a Portland cement mix of lime, alumina and silica which has attained equilibrium, are $3CaO.SiO_2$, $2CaO.SiO_2$, $3CaO.Al_2O_3$ and $5CaO.3Al_2O_3$. Later investigations on the ternary system [3] $CaO–Al_2O_3–Fe_2O_3$ showed that a compound $4CaO.Al_2O_3.Fe_2O_3$ existed, and that its presence was to be expected in Portland cement. The quaternary system [4] $CaO–2CaO.SiO_2–5CaO.3Al_2O_3–4CaO.Al_2O_3.Fe_2O_3$ has also recently been investigated. It has been

found that in this system, which covers all Portland cement compositions as far as the four oxides CaO, Al_2O_3, SiO_2, Fe_2O_3 are concerned, no new compounds appear. The conclusion has therefore been reached from phase equilibrium studies that the compounds to be expected in Portland cement are the two silicates $3CaO.SiO_2$ $2CaO.SiO_2$, the iron-containing compound $4CaO.Al_2O_3.Fe_2O_3$, and the aluminate $3CaO.Al_2O_3$ together sometimes with the next less basic aluminate $5CaO.3Al_2O_3$. Uncombined calcium oxide may also be present in small amounts if equilibrium is not quite fully attained.

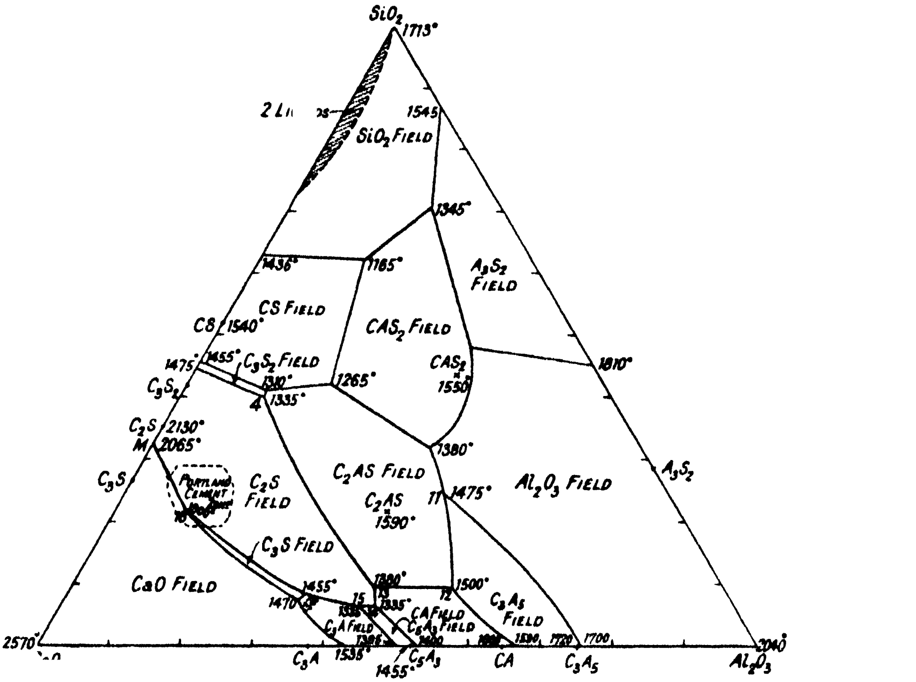

FIG. 2.—System $CaO–Al_2O_3–SiO_2$.

Another constituent which is present to some extent is a glass resulting from the failure of the liquid formed during burning to crystallise completely on cooling.

The minor constituents of Portland cement which have not been considered are magnesia, the alkalies and titania. There is evidence that the magnesia remains, at least in part if not in whole, in an uncombined condition [5], but little is known concerning the mode of combination of the alkalies and titania. The total amount of these, however, only averages about 1 per cent.

The evidence that has been drawn from phase equilibrium

diagrams regarding the constitution of Portland cement has been objected to on the ground that Portland cement does not approach equilibrium during burning under the conditions of commercial production. Much evidence has, however, now been obtained which shows that a Portland cement mix does closely approach equilibrium at the temperature at which it is clinkered commercially [6]. The cumulative results of many investigations on commercial cements by optical and X-ray methods, by analysis of individual minerals constituents separated from the crushed cement by centrifugal methods, and by other lines of attack, have shown fairly conclusively that the compounds present in Portland cement are those predicted by the phase equilibrium data. This may be regarded as a well-merited triumph for those pioneers who some twenty-five years ago amidst the scepticism of many of their fellows first commenced to apply the method of phase equilibrium studies to cements.

Thin sections of Portland cement clinker can be prepared by methods similar to those used by the mineralogists on rocks. In such sections the silicate compounds $3CaO.SiO_2$ and $2CaO.SiO_2$ can be identified, together with an interstitial material containing the remaining crystalline compounds and glass. A photomicrograph of a thin section is reproduced in Plate I.

The cementing qualities of Portland cement are primarily due to the silicate compounds $3CaO.SiO_2$ and $2CaO.SiO_2$. Tricalcium silicate reacts with water and hardens rapidly and the strength developed by it approaches a maximum within seven to twenty-eight days. Dicalcium silicate hydrates and hardens very slowly and in Portland cement it does not contribute appreciably to the strength until some twenty-eight days after the initial setting. It is, however, responsible for most of the further increase in strength of cement at longer ages. There is some doubt as to the role played by the aluminate and iron-containing compounds. Cements which contain only lime and silica can be made in the laboratory and show all the characteristic properties of Portland cement. Such cements could not, however, be produced commercially on account of the high burning temperatures that would be involved. The presence of the fluxing constituents, alumina and ferric oxide, is essential for commercial production. The compounds $3CaO.Al_2O_3$, $5CaO.3Al_2O_3$ and $4CaO.Al_2O_3.Fe_2O_3$ all hydrate rapidly and it seems that they may contribute somewhat to the strength developed in Portland cements during the first few days after setting. The proportion of these compounds present is, however, much less than that of the silicate compounds as is seen from the following values for the compound content of some Portland cements.

Thin section of Portland Cement clinker showing large primary crystals and well-differentiated interstitial material (× 350).

PER CENT. COMPOUND CONTENT OF SOME PORTLAND CEMENTS

	$3CaO.SiO_2$	$2CaO.SiO_2$	$4CaO.Al_2O_3.Fe_2O_3$	$3CaO.Al_2O_3$	$5CaO.3Al_2O_3$
1	31	35	10	10	2
2	54	20	7	9	5
3	45	28	10	7	0
4	43	27	13	11	1

Knowledge of the hydration of Portland cement is on the whole somewhat less advanced than that relating to its constitution. This is due to the marked complexity of the aqueous systems that are involved and the difficulties which attend their investigation. Thus, though many papers have been published in recent years describing investigations on the hydrated calcium silicates and aluminates, the chemistry of these groups of compounds is still obscure at many points.

Various crystalline hydrated calcium silicates are known in nature with $CaO : SiO_2$ ratios varying from 2 : 1 to 1 : 2, though all of them are rare. The compounds which have been prepared in the laboratory at ordinary temperatures are amorphous. They include a compound $CaO.SiO_2$.aq. which is capable of taking up lime from calcium hydroxide solutions up to a $CaO : SiO_2$ ratio of about 1·4 : 1, and a more basic silicate with a $CaO : SiO_2$ ratio which is at least 3 : 2 and may be as high as 2 : 1. It seems probable that it is this latter compound which is formed, together with some free calcium hydroxide, from the hydration of tricalcium and dicalcium silicates at ordinary temperatures. The hydration of the calcium silicates in superheated steam under high pressure leads to the formation of crystalline hydration products [7].

The hydrated calcium aluminates include a hydrated tetracalcium aluminate, $4CaO.Al_2O_3.13H_2O$, and two hydrated tricalcium aluminates, one $3CaO.Al_2O_3.12H_2O$ crystallising in the hexagonal system and the other $3CaO.Al_2O_3.6H_2O$ in the cubic system. It is uncertain which of these hydrated compounds is present in set Portland cement. Hydrated tricalcium aluminate forms a series of complex salts by addition reactions with other calcium salts. One of these complex salts, calcium sulpho-aluminate $3CaO.Al_2O_3.3CaSO_4.31H_2O$, is formed in the hydration of cement by interaction between the tricalcium aluminate and the calcium sulphate which is added to control the time of set.

It is of interest to note that Tilley [8] has recently found in Co. Antrim, Ireland, some small mineral pockets containing hydrated tetracalcium aluminate associated with calcium sulphoaluminate,

hydrated dicalcium silicate, and calcium hydroxide. The occurrence together of these compounds, all of them hydration products of Portland cement, is remarkable.

Two main theories, the so-called crystalline and colloidal theories, have been advanced to explain the setting and hardening of Portland cement. Le Chatelier, the author of the earlier of these theories, considered that hardening was essentially due to the interlocking of crystals produced by hydration. This theory was based on analogy with the setting of Plaster of Paris which, as was originally shown by Lavoisier, is due to the interlocking of crystals of gypsum which deposit from the supersaturated solution formed when water acts on the hemihydrate, $CaSO_4.\frac{1}{2}H_2O$. The action of water on Portland cement also leads to the formation of supersaturated solutions from which the hydrated calcium silicate and aluminate were assumed to crystallise to form an interlocking mass. In the second and later theory of Michaëlis the hardening of cement was attributed to the formation of a gelatinous mass which hardens as it ages and shrinks. According to Michaëlis the supersaturated solution formed initially in the hydration of Portland cement coagulates to form a gel surrounding the cement grains. This gel was supposed to consist mainly of hydrated calcium silicate, with some aluminate, and to contain distributed in it crystals of calcium hydroxide, calcium sulphoaluminate, and some hydrated calcium aluminate. Though the opposing crystalline and colloidal theories have been the subject of much controversy there now seems little doubt of the essential correctness of the view propounded by Michaëlis. The hydration products obtained from the calcium silicates at ordinary temperatures are gelatinous solids and these seem to form the basis of the structure of set Portland cement.

Set cement shows a number of physical properties which are most easily explained by the colloidal theory. Thus when set cement is dried for the first time the curve obtained by plotting the water content of the set cement against its vapour pressure, as the material is progressively desiccated, is irreversible, but on subsequent cycles of wetting and drying the path followed is reversible. In these respects there is a close analogy between the behaviour of set Portland cement and that of silica gel. The volume of a set Portland cement undergoes a similar series of changes. Or allowing the set cement to dry for the first time a shrinkage occurs which is in part irreversible and is greater than the expansion obtained on subsequent rewetting. This drying shrinkage is perhaps the most frequent source of trouble in the use of concrete. The volume changes which occur on cycles of wetting and drying subsequent to the first drying

are approximately reversible. For a typical concrete, when unrestrained, the total magnitude of the first drying shrinkage is about 0·1 per cent. compared with a subsequent reversible movement of about half this value.

Though the colloidal theory appears to describe correctly the structure of set Portland cement hydrated at temperatures up to 100° C., the crystalline theory is probably more closely applicable to cement hydrated in steam under high pressure. Under such conditions the hydration products are crystalline. It seems therefore that, as the temperature and steam pressure under which hydration occurs is raised, the structure of the set mass gradually changes from gel to crystal. Cohesion and strength apparently can be produced by either type of structure.

The reaction of Portland cement with water is exothermic and the heat evolved is sufficient to cause a considerable rise in temperature in the interior of large concrete masses from which loss of heat is relatively slow. The difference in temperature between the interior and surface may attain a maximum value as high as 50° C. within about seven days of first placing the concrete. It is now realised that it is the contraction which occurs during the subsequent slow cooling of the hardened mass which is mainly responsible for the development of cracks in large concrete structures such as dams. In masses of the dimensions of dams the time before the temperature of the interior returns to normal extends to years. Considerable interest is being evinced at the present time in the manufacture of Portland cement with a heat of hydration lower than normal. Such a cement has, for example, been manufactured for the dam, now almost completed, at Black Canyon on the Colorado river in the U.S.A.

The major contributions to the heat evolved in the hydration of Portland cement are derived from the compounds $3CaO.SiO_2$ and $3CaO.Al_2O_3$, while the compounds $2CaO.SiO_2$ and $4CaO.Al_2O_3Fe_2O_3$ only contribute in a much lesser degree. It is possible therefore by suitable modification of the composition of a Portland cement to reduce its heat of hydration considerably ; if the maximum possible reduction is required it is necessary to make some sacrifice in the strengths developed at early ages after setting, though the ultimate strength need not be affected adversely.

Portland cement has so often been regarded as a binding material suitable for almost any purpose that its chemical limitations have sometimes tended to be overlooked. All the hydrated compounds present in the set cement are decomposed or dissolved by water and the ultimate product obtained, when water continuously percolates

through it, is an incoherent mass of gelatinous silica and alumina from which all the lime has been leached out. This action only occurs in practice under very exceptional conditions, for a concrete can be made so impermeable that the decomposition of the set cement is limited to a very thin surface skin and the interior remains permanent indefinitely. When, however, Portland cement concrete is exposed to waters containing salts which react with it, a rapid deterioration can occur. Though, except in very strong solutions, chlorides are almost innocuous, the sulphates of the alkalies and alkaline earths are most destructive. Thus in clays containing gypsum, as in some areas of the London clay, or soils containing alkali sulphates as in the "alkali" soils of the North American Continent, many cases of rapid deterioration of Portland cement concrete have been experienced.

Sulphate salts in solution in general react with the calcium hydroxide present in set cement to form calcium sulphate, and with the hydrated calcium aluminate to form calcium sulphoaluminate. This reaction is accompanied by an expansion which causes progressive disruption of a concrete mass as the attack proceeds inward from the surface. Though there are other cements, such as aluminous cement and pozzolanic cements, which are much more resistant to attack by sulphate-bearing waters, much attention has also been given to the production of Portland cements with a higher resistance than the normal. In general it is found that the substitution in a Portland cement of the compound $4CaO.Al_2O_3.Fe_2O_3$ for $3CaO.Al_2O_3$ increases the resistance to attack by sulphate solutions. This simply involves raising the Fe_2O_3/Al_2O_3 ratio of the cement.

The troubles experienced in France in the latter years of the nineteenth century owing to the disintegration of Portland cement concretes in gypsiferous territories were the cause of the initiation of the investigations of Bied which led eventually to the discovery of aluminous cement.

Aluminous cement is the most recent addition to the list of constructional cementing materials and was first marketed commercially only some fifteen years ago. As its name implies, aluminous cement is characterised by a high content of alumina, and ranges in composition from 35 to 45 per cent. Al_2O_3, 35 to 45 per cent. CaO, 15 to 20 per cent. iron oxides and 4 to 10 per cent. SiO_2.

Though aluminous cement was produced to meet the demand for a cement resistant to attack by sulphate-bearing waters it was found not only to possess the desired properties in this respect, but also to show a rapidity of hardening considerably greater than that of Portland cement. As, however, its cost is about twice that of Portland

cement its use is restricted to specialised purposes where very rapid hardening or a high degree of resistance to chemical attack is required.

The most important constituents of aluminous cement are one or more of the calcium aluminates $CaO.Al_2O_3$, $3CaO.5Al_2O_3$ and $5CaO.3Al_2O_3$, together with one or both of the silica compounds $2CaO.SiO_2$ and $2CaO.Al_2O_3.SiO_2$. The iron oxide is usually present in both the ferrous and ferric conditions, but there is at present no certainty as to the compounds formed. The ferric oxide is most probably combined in solid solutions of $4CaO.Al_2O_3.Fe_2O_3$ and $2CaO.Fe_2O_3$ or of $CaO.Fe_2O_3$ and $CaO.Al_2O_3$.

The reaction of aluminous cement with water is essentially that of the compound $CaO.Al_2O_3$. With an excess of water aluminium hydroxide and crystalline hydrated dicalcium aluminate are formed, but when the cement reacts with a very limited amount of water, as in actual use, it appears that a gelatinous hydrated calcium aluminate may be produced initially. The very high resistance which is shown by aluminous cement to attack by sulphate solutions seems remarkable when it is remembered that the hydrated aluminate compounds in set Portland cement are readily attacked. It seems that the immunity of the hydrated aluminate compounds in set aluminous cement to attack by sulphate solutions is most probably to be attributed to the presence of protective films of gelatinous alumina surrounding the crystals rather than to an inherent stability of the compounds themselves. Much further investigation is required however of this property, as with many other characteristics, of aluminous cement.

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SAP ASCENT IN THE TREE[1]

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1. Some Criticisms of the Cohesion Theory

Some recent observations upon the processes initiated in the woody tree by the recommencement of the growth of the buds in the spring seem to have a direct bearing upon the problem of the movement of water into these buds. Facts of growth and development, however, seem usually to have no place in the statements as to the mechanism of water ascent. The present state of our knowledge of these problems is therefore reviewed very briefly in the following pages. If the conclusions as to the mechanisms operating in the ascent of water seem somewhat unsatisfactory at least a case may have been made for further examination of the problem from new standpoints.

It is generally (though not universally [2]) agreed, on the basis of ringing experiments, that the water ascends the tree in the wood; and so much of the wood consists of empty, lignified elements, particularly the long tracheal elements that seem best adapted to longitudinal transport, that the problem is usually regarded as one of finding the necessary force to drive the water throughout the height of the tree, along such empty conduits. These have lignified cellulose walls, with thin pits through which only the finest perforations are present. The wet wall is readily permeable to water, but gas cannot be driven through the fine pores except under very high pressures, so that the contents of the tracheæ are isolated from any gas content in neighbouring intercellular spaces. Water in these tracheæ must rise to a height of some 350 feet in a redwood tree or a eucalyptus and if the problem is correctly posed two solutions are possible—a force at

[1] A very complete bibliography will be found in E. B. Copeland, *Bot. Gaz.*, 1902, **34**, 161.

[2] W. O. James and H. Baker, *New Phyt.*, 1933, **32**, 317.

least ten times as great as the normal pressure of the atmosphere must either drive it from below or pull it from above.

Some root systems are known to drive water upward at certain times of the year, but there is no evidence, in these manifestations of so-called "root pressure," of a force of ten atmospheres, and, in any case, if a tree bearing foliage is cut down or even if the wood system is simply cut into, the result is usually to demonstrate a tension; the cut surfaces will usually absorb water greedily rather than release it. For this reason, especially, the root system is usually discounted as the source of the force moving water up the tree and attention focussed upon the living cells of the leaf canopy above. Cells which surround the elements of the tracheal system in the leaf are able to withdraw water from this system. If these cells are not fully turgid, so that their suction pressure more nearly approaches their osmotic pressure, they would frequently be able to exert forces exceeding ten atmospheres. There may thus be an adequate force available at this end of the tracheal system, provided that the continuous system of tracheal elements in the wood can remain filled with a continuous column of water under the stress of the tension thus transmitted downward through the system by the continuous removal of water above. The founders of the cohesion theory of the ascent of sap, Dixon and Joly, and Askenasy, provided evidence that a liquid column in a woody trachea might be able to withstand very considerable tensions, far exceeding ten atmospheres, without interruption of the water column by the appearance of bubbles. As root pressure appeared to be inadequate and as no other theory appears adequate to explain the ascent of sap, *faute de mieux* the botanist has fallen back as a rule upon the cohesion theory, but the fact that under certain conditions water columns will remain unbroken under considerable tension is not sufficient evidence that this mechanism is operating to raise the sap in the tree and the supporting experimental evidence for this theory is as yet very meagre.

Since the theory was first advanced evidence has accumulated that, under static conditions, water columns may resist considerable tensions before breaking, in particular in the cells of the fern annulus, either living or dead, Renner [1] and Ursprung [2] have supplied evidence that the water in the cells may withstand tensions exceeding 200 atmospheres before gas enters. But it is still true, as in Askenasy's early experiments, that attempts to raise a moving column of water far above atmospheric height, so that it moves

[1] O. Renner, *Jahrb. für wiss. Bot.*, 1915, **56**, 617.
[2] A. Ursprung, *Ber. der deutsch. Bot. Ges.*, 1915, **33**, 153.

upward under tension, are far from successful and in all such cases when the liquid column is agitated it breaks. In the tree the narrow pipes through which the water ascends are built into shafts that are continually agitated by the wind, but in the light of the fact that we have so little direct evidence as to how ascending columns of water will behave when under tension in such a system, we must leave this question open and examine the evidence for the existence of continuous columns of water, under tension, in the wood of the tree.

Tensions are indubitably present in this tracheal system at most seasons of the year, the only point at issue is whether these tensions are due to stretched liquid columns, when the tensions may be very high, or to the presence of gas at less than atmospheric pressure, possibly only water vapour, when the "tension" is due merely to the excess of the external atmospheric pressure over that of the gas in the tracheal system. Such a system of gas at low pressure is not correctly spoken of as a system in tension and could not be responsible for the rise of water to a greater height than about 34 feet. It is probably because of this difficulty looming ahead that much experimental evidence has been disregarded which points clearly to the conclusion that the tracheæ contain gas at low pressure rather than liquid columns under high tensions.

But let us first look at the few experimental facts which seem to support the view that liquid columns are present under higher tensions.

There are certain experiments which show leafy twigs raising water to heights greater than they can be raised by forces equivalent to the pressure of the atmosphere.

It is true that as a rule the excess above barometric heights is not great, at best about double what the atmosphere would support, but as the systems at work must either be continuous liquid columns or gas at low pressure and as tracheæ containing gas at low pressure could not possibly raise water above barometric height the issue seems already decided. Most of these experiments have been done with Conifer twigs, but Böhm raised water above a mercury column slightly above barometric heights by its absorption by willow shoots of which the root system had been boiled, whilst Nordhausen[1] describes one, apparently precarious, experiment in which Syringa shoots, cut under water, raised water against a mercury column and the pull of a pump so that the water column was actually raised well above barometric height. But neither these occasional experi-

[1] M. Nordhausen, *Jahrb. für wiss. Bot.*, 1917–19, **58**, 295; *Ber. der deutsch. Bot. Ges.*, 1916, **34**, 619.

ments with Dicotyledon shoots nor Ursprung's[1] more successful ones with Conifers can really settle the question at issue.

No woody system, in a living shoot, consists entirely of dead tracheæ; amongst the tracheæ are always living elements with osmotic contents, perfectly capable of absorbing water against outside tensions exceeding the comparatively small tensions existing in the most successful of these experiments. The only condition necessary for the maintenance of these tensions was that the water column should remain in contact with their living cells, and this it would do, so long as it remained in contact with the tracheæ. The difficulty, which leads finally always to the breaking of the column, if cut shoots are used, is that the water is also in contact with the intercellular spaces and that as the tension increases air is drawn through these spaces and out of the cut end. In the Conifer the intercellular spaces in the wood are smaller than in the Dicotyledon so that these twigs will support a greater tension before air is drawn in to break the column. In Nordhausen's experiment the precarious success with Syringa depended upon the precautions he had taken to make the passage of air difficult through the cut end of his twig.

Both Böhm and Nordhausen obviously met the other great difficulty in working with Dicotyledons—the rapid wilting of the leaves of the Dicotyledon when the tracheal system is placed under tension. This difficulty has been frequently met with in experiments with transpiring Dicotyledons; it is mentioned by Vesque, Strasburger, Scheit and Janse and seems very difficult to reconcile with the view that normally the transpiring leaf canopy of the tree is drawing water out of the tracheal elements against quite considerable tensions.

Apart from this evidence that the transpiring shoot can raise water over mercury columns exceeding barometric height the only experimental observations in support of continuous liquid columns in the tracheæ appeared to be those supplied by investigators in Professor Renner's school who have supplied direct evidence for continuous liquid columns and also for their development of tensions. Holle[2] examined under the microscope the tracheal system in living leaves with a sufficiently transparent vein system and the tracheæ could be seen to be full of liquid, even when the leaves were wilting, air only entering them either through cuts opening the tracheæ or through hydathodes at the margin. Bode[3] then made similar

[1] A. Ursprung, *Ber. der deutsch. Bot. Ges.*, 1915, **33**, 253.
[2] H. Holle, *Flora*, 1915, **108**, 73.
[3] H. R. Bode, *Jahrb. für wiss. Bot.*, 1923, **62**, 92.

observations upon the tracheal systems in the young stems of herbaceous plants, which by manipulation could be made sufficiently transparent without damage to the tracheæ or the living cells immediately around them. This evidence is very convincing but does not invalidate the very large amount of experimental evidence that in the tracheal system of older stems and especially of older woody stems, many of the tracheæ demonstrably contain some gas. These experiments, both by injection methods and by samples withdrawn with great precaution for microscopic examination, are rejected by Holle and Bode because, from the nature of the case, the intact tracheæ cannot be examined.

In a later paragraph it will be argued that the injection experiments are strongly in support of the presence of gas at low pressure in many of the tracheæ of the woody stem, but similar injection experiments certainly seem to support the view of the German observers that the tracheal system in the leaf and in young herbaceous internodes is usually full of sap. We must now examine the evidence for the further view that these liquid columns may exert great tensions. This is a development from an early observation by Renner that if the potometer experiment is set up with a shoot which is blocked with wax at the end, the shoot still slowly absorbs water (so slowly that the leaves usually wilt) and the bubble travels along the capillary arm. Now if the shoot is cut off short above the cork the bubble momentarily springs back a short distance in the capillary. Renner explained this as due to the tensions in the tracheal system having caused the butt of the shoot in the potometer to contract slightly. When the top of the shoot is cut off the tension is released and the rapid increase in diameter of the butt of the shoot is responsible for the sudden backward movement of the bubble. Bode confirms this explanation by direct observation of the butt of the shoot in a potometer with plane glass sides; he finds that the butt of a shoot of Syringa or Lycium slowly contracts some 0·3 per cent. as the shoot gradually wilts and then recovers this loss in diameter immediately when the top of the shoot is cut off.

Similarly, continuing his experiments with potted herbaceous plants, he finds that if he pares down the stem until he can get individual tracheæ under microscopic examination, these tracheæ contract slowly as the leaves above wilt and then expand again when the soil is watered with warm water and the leaves recover. The diameter changes in this last type of experiment are small, some 2μ in all during the period of observation. If the experimental difficulties are great, however, it would probably be agreed

that these experiments are evidence of tensions in the tracheal system; it remains to be shown, however, that they are evidence of great tensions, such as could not be represented for instance by the presence of water vapour in the tracheæ; here the evidence definitely breaks down.

Bode uses a method utilised by Renner previously. He cuts off the shoot, in either type of experiment, and attaches the top of the stump to a pump. He then applies suction and as he fails to make any impression, in the one case on the diameter of the stump of the shoot in the potometer, in the other case on the diameter of the trachea, he argues that the tension in the tracheæ which produces the observed contractions must therefore be so much greater than he can apply by the pump. But this argument is inadmissible. The living leaf draws water solely from the liquid contents of the tracheæ, whilst the reduced gas pressure produced by the pump is present both at the cut ends of the tracheæ and in the intercellular system in the tissues around them.

Under these conditions there is no guarantee that there will be any pressure difference at all between the interior of the trachea and the intercellular space system outside it. In fact, in Bode's two experiments, where the requirement is to bring about a difference of tension between the inside of the tracheal system and the intercellular space system around, it can safely be said that no such difference can be produced and no contraction is to be expected as the result of the manipulation with the pump. We are left then with a demonstration of low pressures in the tracheal system by Bode's experiments but no evidence for continuous liquid columns under great tension.

Other observers using potometers have occasionally noted facts that would suggest that the tracheæ in the stems of plants used in these experiments actually contain gas under low pressure. Thus Vines noted that the leafy shoots in the potometer continued to absorb water when most of the leaves were removed and Nordhausen draws attention to the fact that even when most of the leafy shoot is removed, as in Renner's and Bode's experiments, the stump may continue to suck up water. He then proceeds to carry out experiments using his own modifications of the potometer in which he shows that the stem, with leaves removed, still pulls up water with as much force as if the leaves were still present. Nordhausen is thus led to conclude that the living forces in the stem are effective in the ascent of the sap. He gauges the force at work by the rate at which the plant can pull water through a clay resistance at the base of the cut shoot as compared with the rate at

which the suction pump can pull water through the same resistance. He thus arrives at values of up to 7 atmospheres, though it seems open to serious question whether an increase in the force applied above the clay resistance would produce a proportional increase in the flow of water through the resistance. However, even if these estimates be accepted, as pointed out previously (p. 44), they are not evidence for the existence of such tensions in the tracheæ; the tensions may well be due to the suction pressures of the living cells in the twig.

This very brief review of the literature has thus not produced any very striking evidence of the existence of high tensions in continuous liquid columns in the tracheæ of the tree—nor is it at all clear to the writer how such tensions, if present, would provide a system moving water up the tree. They would tend surely, as pointed out by Nägeli and Schwendener many years previously, to immobilise the water within the series of completely closed elements that form the continuous chain from root to leaf, in which all walls would tend gradually to dry as the concavity of the liquid meniscus increased and its surface withdrew further into the minute pores in the walls. However, its theoretical adequacy may wait upon a more convincing demonstration of its existence as a mechanism. In the next section the observations are described which had awakened in the writer doubt as to the adequacy of existing theories to account for the movement of water in the tree.

2. Vessel Differentiation and Water Movement into the Bud

During the last few years the writer has convinced himself that radial growth in the woody shoot of both softwood or Conifer and hardwood or Dicotyledon is indissolubly linked with the extension growth of the leafy shoots.[1] Only when leaves are produced and extended and their vein system develops is radial growth resumed throughout the woody axis beneath and every longitudinal file of tracheal elements produced in the new wood of trunk and branches, if it could be traced upwards, would be found to end in the lateral emergences upon the growing shoot. Then in winter, in the deciduous tree, all those longitudinal files of tracheæ in the trunk and branches, which are gradually filling with water, end upwards either in the leaf scars on the older wood or in the young primordia within the buds. Then as the temperature rises in spring and the water in the woody system expands it moves along the wood longitudinally—the direction in which water moves most easily—

[1] J. H. Priestley, *The Growing Tree*, Report Brit. Assoc., York, 1932.

and as all other tracheæ are blocked it must ultimately be driven into the buds where increased water supply, plus rising temperature, causes the bud to resume growth.

This renewal of growth in the bud is followed, immediately in ring porous Dicotyledons, very rapidly in Conifers, more slowly in many diffuse porous Dicotyledons, by a renewal of growth basipetally over the surface of the old wood down twigs, branches and trunk. A method has recently been described [1] by which this newly formed tissue can readily be removed for microscopic examination. Thus we have confirmed Bailey's generalisations [2] as to the differences between cambial activity in softwoods and the bulk of the Dicotyledons. In the softwood the cambium initial, which cuts off to the

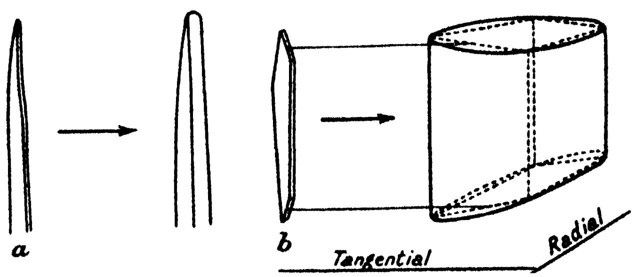

FIG. 1.

Cambium cells of spruce (*a*) and elm (*b*), showing the alteration in slope of the walls, resulting from the expansion of (*a*) to a tracheid and (*b*) to a vessel segment. (× 225 approx.)

inside of the cambium ring a series of replicas of itself by tangential longitudinal divisions, is a very long, pointed element. In Fig. 1 the greatly enlarged end of this cambial element is figured as seen in tangential longitudinal view as it thus increases in thickness at right angles to the plane of the paper and then is cut in two by a wall parallel to the paper surface. Similarly the end of the shorter Dicotyledon initial is represented engaged in the same process of expansion, and it will be seen that one result is that, whilst in the softwood no walls can be spoken of as relatively transverse, in the tissue thus derived from the cambium initial, in the hardwood such walls are present. The next stage in the differentiation of this newly formed tissue producing wood is always its vacuolation and transverse expansion through the intake of water. It now seems clear why in the Dicotyledon this process may be associated with the perforation of some of these relatively transverse cross walls whilst no such

[1] J. H. Priestley, L. I. Scott and M. E. Malins, *Proc. Leeds Phil. Soc.*, 1933, **2**, 365.

[2] I. W. Bailey, *Amer. Journ. of Bot.*, 1920, **7**, 355.

walls are present to be perforated in the softwood. Perforation seems indubitably to be associated with the transverse expansion of these elements. Naturally it usually occurs in the contiguous transverse walls of two elements which are approximately in the same vertical line so that one common perforation is thus obtained, and such a process of perforation, when once a hole appears in the thin "three-ply" wall of cellulose, pectin middle lamella, cellulose, will spread very rapidly whilst the girth of the cells increases.

We have been forced to regard the process in this light because the "strip" method of removing these new tissues from the old wood enables us to examine these vessel systems in all stages and to follow the same vessels under the microscope for long distances.[1] In large quantities of this material no stage has ever been seen between that in which files of elements begin to vacuolate and that with the expanded vessel with a series of perforated vessel segments, all still with exceedingly thin walls but showing absolutely no stages in the process of perforation. The same technique has provided convincing evidence that the vessel expansion proceeds with considerable displacement of surrounding elements and there can be little doubt that the fibres, for instance, which invariably accompany vessels in hardwoods but are absent from the wood of softwoods, are formed as the result of the compression of other relatively undifferentiated cells cut off from the cambium which thus become longer than the cambium initials from which they are derived. Possibly as the result of the expansion that the vessel segments are able to undergo, by compressing and displacing neighbouring elements and as a result of the tearing of their own transverse walls, the liquid contents of the differentiating vessels in hardwoods seem to be contained under less pressure than the contents of the differentiating tracheids of the softwoods. This is clearly indicated by a simple test.

If the bark is stripped from the new wood in May or June and a knife blade passed rapidly across the differentiating tissues, in the softwood the liquid contents can be seen to spurt out into the air. It is thus possible on scratching the surface to feel a fine spray on one's face at a distance of several inches from the surface of the wood. On the other hand when the hardwood surface is similarly cut the liquid oozes out gently from the vessels, there is no spray. The only exception to this noted by the writer is a very suggestive one; the differentiating wood of the Dicotyledon, Drimys, was found to spray like a softwood, and this tree of course has long

[1] J. H. Priestley, L. I. Scott and M. E. Malins, *Proc. Leeds Phil. Soc.*, 1935, 3, 42.

cambium initials like the softwood and no vessels are formed in the wood. On the whole the general connection between long fusiform initials and the absence of vessels seems to be definitely significant in the light of the facts that are accumulating as to the process of xylem differentiation, though the writer is grateful to Dr. L. Chalk for drawing his attention to one exception. The hardwood species, *Millingtonia hortensis*, has cambium initials equal in length to those of the softwood and with very sloping end walls, but one of these sloping walls is perforated during differentiation to give an exceptionally long and narrow, scalariform perforated, end wall. In view of the distinctions that are emerging as to the differences between vessel differentiation in diffuse porous and ring porous types, it would seem that transitional types like *Millingtonia hortensis* and the vessel-less Dicotyledonous genera are to be expected.

Up till now most observations upon the process of vessel differentiation by the new technique have been made upon the ring porous hardwoods. Here the vessels in the trunk are remarkable for their large diameter and great length so that their differentiation raises problems of great interest. We have recently described how the vessel segments here expand rapidly whilst their walls are quite thin.[1] They perforate before any signs of pitting can be detected upon their very thin containing walls. Each vessel segment then contains a living protoplast, with a comparatively concentrated vacuolar sap to judge from the concentrations required to plasmolyse it. Between adjacent protoplasts in the vessel, although the cellulose walls have become perforated, the mucilaginous material of the middle lamellæ remains for some time so that evidently there can be no rapid longitudinal movement of water throughout the vessel whilst its contents are thus constituted. The pectin layers seem to disappear in time but the protoplasts remain throughout the process of thickening and lignification of the vessel walls, for which they are doubtless responsible. They then disappear comparatively suddenly as is evidenced by the fact that these vessels which, until then, released a drop of sap when opened, now, if opened under such a liquid as indian ink, inject with great rapidity and for great lengths. But before following up this observation we must examine the liquid contents of this vessel in relation to the bud into which this particular tracheal file could certainly be traced if it were followed upwards.

In ring porous trees, as, for example, ash, elm, wych elm, oak, laburnum and sweet chestnut, when bud development begins cambial

[1] J. H. Priestley, L. I. Scott and M. E. Malins, *Proc. Leeds Phil. Soc.*, 1935, **3**, 42.

activity resumes with extraordinary rapidity all over the tree right to the base of the trunk but the buds are very slow in opening. The result is that when vessel differentiation begins over the surface of the old wood, an expanding cell finds a cell below it at the same stage throughout the whole length of the tree, the result seems to be an almost simultaneous extension of such vessel segments along a longitudinal path practically from twig to base of trunk. Up to date we have quite failed to trace the spread of the process of differentiation throughout the length of the tree. Amazing as it seems the process takes place so rapidly that when vessels are found in the branch they will be found in the base of the trunk. Another result is that vessel length in these trees is quite exceptional. So far as it has proved possible to ascertain, the vessel seems to run nearly from base of the new shoot to the base of the trunk without a cross wall. Evidence is always found that a wall cuts off the vessel in the leaf trace from the vessel in the branch below, but we now have a picture of every tracheal element in the leaf, still hidden in the bud, being continued downward in longitudinal continuity with an expanded vessel which contains a long file of protoplasts all fully expanded with sap, expanded against one another and responsible for the expansion of the vessels.

Incidentally this vessel, very narrow in the slender distal branch, may reach a diameter of about one-third of a millimetre in the main trunk. Where has this vessel obtained its water from? On this point there can be no doubt. The osmotically active expanding protoplasts have drawn water from the nearest available source, the old wood over the surface of which they lie. In this country these ring porous vessels are differentiating in early May and if the bark is removed from these trees at this time with care, so as not to damage these delicate tissues, it is then possible to cut through them into the wood just beneath and thus to demonstrate that vessels lying in the old wood just beneath can be injected with a liquid like indian ink for distances of many feet. This injection was not possible until these new elements differentiated, and if the new elements are scraped off in a few days it will no longer be possible to inject the old wood underneath. It is difficult to reconcile this injection with the idea that these elements lying beneath the old wood contain liquid columns under high tensions. Such high tensions would presumably have prevented the vessels outside from drawing from them the water which they have undoubtedly taken from this source. There can really be little doubt but that these elements have parted with their liquid content by its radial movement to these expanding vessels outside them and are now

simply occupied by gas at low pressures, mainly water vapour. A more difficult problem is the relation of the water in these expanding vessels to the new leaves above. No rapid flow of liquid can take place through this file of protoplasts, but as they expand against the resistance of the surrounding tissues they must certainly reach the state of a fully turgid tissue with no suction pressure from which water may be withdrawn by living cells with a definite suction pressure. But then their protoplasts must gradually become more permeable. In time the protoplasts disappear altogether as entities. Presumably then some of their contents may be forced out of them, but in that case its natural course, as always, will be longitudinal, where there are few cross walls, and thus into the leaves; the process may well be expedited if the upper protoplasts were differentiated first and then become more permeable first, a condition which may perhaps be expected in view of the basipetal resumption of cambial activity.

Later all the liquid contents of these tracheæ will have disappeared after the protoplasts have broken down, and when these newly formed tracheæ can also be injected to great length and very rapidly. This may mean that much of the liquid has moved forward into the transpiring leaves because the tracheæ certainly connect with the oldest leaves, but outside these vessels are now others differentiating in their turn and some of the liquid has probably thus been drawn off radially. In the meantime, however, as von Höhnel pointed out years ago, the drain upon the water supplies in the older wood has continued and it will be found possible to inject the tracheæ of inner rings of wood if they are cut open under a suitable liquid. But von Höhnel showed that the injection was always most marked in the outermost tracheæ and gradually fell off inwards although it might be seen in the wood of five or six of the outer rings in many trees.

Presumably much of the water from this long vessel has found its way into the leaf with which it stands in such close connection and we now have a picture of the movement of water into the expanding leaf which is very different to that given by the cohesion theory. The old wood is acting as a reservoir of water and instead of water moving continuously along a lifeless trachea from living root to living leaf it is drawn off laterally from the old wood into the differentiating vessels of the new wood and from thence it is driven, by processes still requiring further elucidation, into the expanding tissues of the young leaves. This system involves the interplay of living tissues, engaged in the processes of growth and differentiation, and the most superficial observation of growing

trees will show that only those twigs on which buds are growing and in which, as a result, radial growth is proceeding, are able to share in the water supplies of the tree. When trees are grown so that their lower branches are in deep shade many of the branches fail to grow and it is a fact of common experience that such branches not only remain dormant but gradually dry out—their water supplies are lost either by evaporation or by being drawn off by the osmotic systems developing on the surfaces where the wood is still growing.

It may be true that Strasburger, under special experimental conditions, found that one poison in succession to another, could slowly creep along a presumably dead tracheal system into leaves at great height from the ground, but such observations do not invalidate the conclusion that the movement of water into growing buds is a process which is only effected in a growing tree and by a system which requires the collaboration of living differentiating systems provided by the processes of radial growth, initiated in the bud and now proceeding basipetally down the tree.

But these observations are as yet an isolated contribution to the movement of water in the tree. The fact that the older lignified tracheæ soon inject with liquids with great speeds and to great heights has made the present writer sceptical of the existence of the liquid columns under high tensions postulated by the advocates of the cohesion theory, but the fact itself does not provide an explanation as to how the water moves in the tree. There seems little doubt from a trial of injection methods at different times of day and different seasons that these tracheæ fill up again particularly at night or during rainy weather, but there is as yet no explanation of the mechanism involved and, in particular, we lack an explanation of the machinery by which the wood of the tree regains its water content during the winter months. It is true that, if the supply from the root is in excess of the loss from the leafy crown, sap may again be present in excess at the lower ends of the tracheæ in the trunk and that these tracheæ would then rapidly fill again as they do when injected with aqueous fluid experimentally. But this does not explain how water can once more fill these astonishing vessels in the trunk of ring porous trees which appear to be well over 34 feet in length, nor does it explain how, above this height, water can move from one closed trachea of a diffuse porous system into another, without draining away down walls and intercellular spaces in the process.

In diffuse porous woods, in which radial growth has been comparatively slow in its basipetal progress down the surface of the

old wood, the vessels as they differentiated probably only found comparatively short longitudinal files of cells at the same stage of differentiation at the same moment. In any case in these woods, vessels are comparatively short, a few inches to a few feet in length, and vessel ends are readily recognisable in macerated wood from any regions of such trees. In such trees it is clear that the problem of sap ascent raises the question of the rapid refilling of longitudinal files of such vessels. At the same time these diffuse porous woods enable injection experiments to be carried out which seem to give indubitable proof of the presence of numerous vessels with gaseous contents at very low pressure. In the ring porous types, when a vessel is injected, the liquid rushes up 20 feet in a few seconds. It may be possible to argue that this vessel originally contained liquid at high tension and that this liquid has now been displaced into the leaves and roots, though the explanation will hardly survive an extensive experience of this type of injection experiment. But in the diffuse porous types a piece of branch may be isolated from all roots and leaf systems without a number of the closed vessel systems being opened in the process. If this is done in early summer and the bark then removed from this piece of branch it is possible to inject these tracheæ all round the branch, the injections proceeding as rapidly as the circuit is completed as when the liquid shot into the first tracheæ to be cut open. In this case it is quite impossible to account for the whereabouts of the displaced liquid if the injected elements are assumed to have originally contained liquid under tension. The tracheæ must evidently have contained gas at low pressure.

The structure of the wall of the trachea explains fully how that, once full, it can retain its liquid contents and that the temporary formation of a continuous liquid column hanging from the highest trachea, even of a 350-foot tree, would not be likely to develop tensions dragging the water out of the trachea, but it is not yet clear what is the force that moves the water continuously upward or retains it in the wood above the barometric height under such conditions that it is free to be sucked into tracheal elements at this height that previously contained nothing but water vapour.

One point must certainly be remembered. Other elements in the sap wood, in which water movement takes place, are always the living cell of ray and wood parenchyma. These cells contain starch, which is a potential source of sugar, or sugar itself, which, if released into the water in the tracheæ, will undoubtedly tend to draw water into the tracheal elements by osmotic action. During the winter of 1933–34, Drs. R. D. Preston and W. E. Berry made

observations for me upon the sugar content of comparable quantities of water sucked through twigs of the poplar which were cut from the tree every few weeks. These observations, though preliminary in nature, showed definite evidence of the presence of increasing quantities of sugar in the tracheal elements just at the time, before radial growth starts, when the water table in the tree appears to be rising most rapidly. Dr. R. D. Preston has also carried out a considerable amount of experimental work which shows clearly that the lignified walls of the wood may behave as an imperfect semi-permeable membrane with respect to sugar solutions.

These are only hints, we can offer no satisfactory solution of the old problem as to how the sap is carried from root to leaf throughout the season in trees of which the canopy is spread at heights well above that to which water can be carried by the lifting pressure of the atmosphere. Our main thesis has been twofold: (1) that the cohesion hypothesis does not rest upon a satisfactory experimental basis, and that (2) the movement of water into the expanding leaves in spring is associated with processes of growth and differentiation both in leaf and woody axis, which are entirely neglected at present in the orthodox treatment of the problems of sap ascent.

INDUSTRIAL PHYSICS

THE INSTITUTE OF PHYSICS CONFERENCE

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THE Institute of Physics, since its foundation in 1918, has consistently endeavoured, among other things, to co-ordinate the activities of physicists engaged in pure research and in industry. The numerous lectures on *Physics in Industry* which it has arranged and published from time to time have been universally regarded as providing valuable surveys of the methods of investigation and the achievements in the various special departments of commercial work to which they refer. In order the more to emphasise, both to experts and laymen, the important part which Physics plays in modern industrial life and to provide a common meeting ground for all those whose activities involve Physics, the Institute organised a Conference in Manchester at the end of March, 1935, the special subject of which was *Vacuum Devices in Research and Industry.* Accommodation and laboratory facilities were provided by the University of Manchester, and the local arrangements were in the hands of the Manchester and District Local Section of the Institute of Physics which is at present the only local section of the Institute in this country. The Chairman of the Local Section, Professor W. L. Bragg, F.R.S., presided over the Conference.

After the formal opening on March 28, by Sir William Clare Lees and the Vice-Chancellor of the University, lectures, demonstrations and discussions were held, during the course of which the applications of vacuum devices specially adapted for specific industrial purposes were described by experts from leading industrial concerns. An important feature of the Conference was the holding of an exhibition of apparatus, instruments and books showing how vacuum devices such as the photo-cell and cathode ray tube have now developed into reliable industrial tools. In connection with the Conference, visits were paid to the research laboratories and works of Messrs. Metropolitan-Vickers Electrical Company, Limited,

also to the Shirley Institute of the Cotton Industry Research Association, Messrs. Tootal, Broadhurst Lee Co., Ltd., and the Post Office Telephones.

Public interest in the Conference was aroused through a broadcast talk by Professor W. L. Bragg from the B.B.C. North Regional Station, and a Public Lecture on Cathode Ray Tubes in Industry by Mr. R. A. Watson Watt, Superintendent of the Radio Department, National Physical Laboratory. Members of the public and pupils from the local schools were admitted to the exhibition on the last day.

The social events included a Conference dinner in the College of Technology, presided over by the President of the Institute, Sir Henry G. Lyons. The guests included the Lord Mayor and Lady Mayoress of Manchester and other distinguished persons.

Altogether, about 600 members attended the Conference, and nearly 3,500 persons visited the Exhibition.

As the lectures given at the Conference are not being published, a summary of the chief points raised is given below.

The subject of "Modern Electrical Illuminating Devices" was introduced by Mr. J. W. Ryde of the General Electric Company, Limited, who pointed out that, until the beginning of the present century, practical sources of illumination depended on temperature radiation, a characteristic of which is the emission of radiation over a large range of wave-lengths accompanied by a distribution of energy with a maximum value at a wave-length depending on the temperature. After remarking on the recent improvement of 15 per cent. to 20 per cent. effected in the efficiency of tungsten filament gas lamps by respiralling the spiral filament, he proceeded to discuss the production of light by electric discharges through gases. Here the radiation is restricted to a few isolated wave-lengths only which are independent of temperature but which may, of course, occur in an unwanted region of the spectrum. Discharge lamps may be classified according as they require high tension or ordinary supply voltages. The former include carbon dioxide tubes giving light similar to daylight which may be used for colour matching, and the well-known tubes for advertising and display. The light output per unit length is, however, not sufficient for use in optical systems. Hot cathode tubes, in which the electrons consist of heated oxides, enable large currents to be passed and may be run directly off the mains. They are from 50 to 100 times brighter than the earlier discharge tubes and are particularly suitable for floodlighting. High pressure mercury vapour lamps give light deficient only in red. Their efficiency is about three times

that of the filament lamp and over 5,000 are already in use for street lighting.

"The Applications of Photo-Electric Cells" were dealt with by Mr. T. M. C. Lance, of Messrs. Baird Television, Limited, and Mr. R. C. Walker of the General Electric Company, Limited. The difficulties of amplifying faithfully the current due to light changes from 10 to 200,000 times per sec. were discussed and some details given regarding the experimental technique involved. Reference was made to the use of photo-cells in special cases of timing and counting, in smoke detection for factory chimneys and in alarms and safety controls. Their use and limitations in colour matching problems were also considered.

Among the many impressive features of interest at the Conference, pride of place must undoubtedly be given to the cathode ray tube which, while having served the pure physicist faithfully in one form or another as an accurate measuring instrument for at least 40 years, has now entered into a new sphere of utility as an industrial tool. The properties of the cathode ray tube rendering it particularly suitable for industrial purposes were discussed by Mr. L. H. Bedford of the firm of Messrs. A. C. Cossor, Limited, while some applications of the tube were described at the public lecture by Mr. Watson Watt. During his remarks, Mr. Bedford emphasised that the cathode ray tube is merely a two-dimensional recording instrument with an extraordinarily high frequency range, and its purpose is to record oscillatory phenomena of every type such as the wave forms of electric power supply, speech, music, atmospherics, electro-cardiograms and engine indicator diagrams. The recent application to television has resulted in the development of a new technique involving the use of special high vacuum tubes together with a new form of time base in which hard valves are employed, thus dispensing entirely with gas discharge devices. Numerous exhibits at the exhibition served to enforce the special points brought forward by Mr. Bedford.

A very important vacuum device developed only during the last two or three years is the thyratron or grid-controlled mercury vapour rectifier, already an appliance which has proved of immense value in the precise control of the speed of electric motors, voltage control of electric generators, dimming of lighting systems and resistance control of electric welding.

In discussing the thyratron, Messrs. L. J. Davies and A. L. Whiteley of the British Thomson-Houston Company, Limited, drew attention to the fact that until the advent of this device, the method of interrupting an electric circuit was fundamentally the same as

Faraday might have used a hundred years ago, that is, by the mechanical separation of conductors. Definite limitation in the frequency and accuracy with which a current can be started and stopped are inherent in this mechanical method, the maximum number of interruptions per minute being only of the order 200–300. With the thyratron, 1,500 interruptions per minute are easily attainable, while much larger currents can be handled than was the case previously.

The spectacular development of the applications of high voltages to research purposes was described by Dr. J. D. Cockcroft of the Cavendish Laboratory, Cambridge. He pointed out that while in 1928 voltages greater than 200 kilo-volts had not been applied to vacuum tubes, in 1934 success had been achieved with tubes working under a million volts; further, prospects are favourable for an immediate increase to 2,000,000 volts. Large power tubes depend mainly on the production of high vacua on an engineering scale, the metal pumps of to-day having a hundred times the pumping capacity of the glass pumps formerly in use in physical laboratories. Apparatus is now connected by large-diameter welded steel pipes. The general principles involved in the construction of high voltage tubes indicate the necessity of subdividing the vacuum units so that no section has to withstand more than 500 kilo-volts; insulating surfaces must be outside the strongest parts of the electric fields and the electrodes must be designed so that electrons are prevented from leaving them under the action of the fields, otherwise destructive discharges may be initiated. To prevent the passage of sparks down the outside, the apparatus has to be of large dimensions, some tubes being 15 feet in length. The laboratory has also to be spacious and in one case will be 100 feet high. In the future, it may be necessary to immerse the apparatus in oil or build it entirely in the open air.

The construction of such high voltage plant immediately opens up new fields of investigation, some of great industrial importance. Among these may be mentioned the production of high voltage X-rays for medical purposes. The most useful radiations from radioactive sources are gamma rays of about 2,000,000 volts energy. With high voltage X-ray tubes a steady increase towards this energy has recently been made, tubes operating at a million volts being already in use. Higher voltages imply an increase in the penetrating power of the rays, and since the beams of X-rays of higher energy strike their targets with less scatter to surrounding tissues, the recent increases in energy enable larger "doses" of radiation to be applied without harm.

Another valuable industrial application of high voltage tubes lies in the production of rectifiers containing mercury vapour which seem likely to enable D.C. voltages to be stepped up and down comparatively easily and thereby render D.C. transmission practicable.

The popular press quite naturally seized upon Dr. Cockcroft's remarks concerning the use of high voltages in producing transmutations of matter, particularly in regard to the generation of "artificial radioactivity." While work in this field is likely to develop rapidly, it is too early yet to predict possible industrial consequences.

Dr. G. Shearer of the National Physical Laboratory gave an account of the utility of X-rays in industry, referring to both radiography and X-ray spectrometry. He pointed out among other things that X-rays have revealed that nearly all solids are crystalline in character. Moreover, X-rays can give fundamental information about the structure of metals, chemicals, bricks and clay, paints, vegetable and animal fibres, textiles, rubber, mineral ores and the like.

In conclusion, it may be remarked that the Conference was so successful that many individuals and firms have expressed a desire that further Conferences on similar lines should be held in the near future.

AN HISTORICAL ACCOUNT OF PHARAOH'S SERPENTS

By H. IRVING, M.A., D.Phil., A.I.C., L.R.A.M.
Demonstrator in the Old Chemistry Department, Oxford

ALMOST a hundred and twenty years ago, Porret [1] described the preparation of a new "red tinging acid" which formed with protoxide of mercury an insoluble white salt which he called "sulphuretted chyazate of mercury."

An aqueous solution of this acid—thiocyanic acid—was later prepared by Berzelius [2] by distilling ammonium thiocyanate with concentrated sulphuric acid. Since cyanogen $(CN)_2$ had recently been obtained by Gay Lussac [3] by heating mercuric cyanide, Berzelius naturally hoped to obtain thiocyanogen $(CNS)_2$ by the action of heat upon the mercury thiocyanate prepared by neutralising the parent acid with mercuric oxide. His expectations were not, however, realised,[1] for carbon disulphide and nitrogen were evolved and cinnabar sublimed. Distilled with one-third of its weight of sulphur, however, the salt decomposed violently, forming carbon disulphide, cyanogen and nitrogen, and a black, porous, pumice-like mass was formed in such abundance that the apparatus was broken open.

In the same year, Wöhler [4] attempted to prepare anhydrous thiocyanic acid by the action of sulphuretted hydrogen upon mercury thiocyanate, and was actually the first to record the very characteristic snake-like development of the residue [2] formed when this salt is allowed to burn in the air. He writes: "Erhitz man es gelinde, so schwillt es plötzlich, sich gleichsam aus sich selbst in wurmartigen Gestalten windend, um das Vielfach seines vorigen Umfangs auf. . . ."

[1] Thiocyanogen was not obtained until 1919 when Söderbäck prepared it by the action of bromine upon lead thiocyanate. It forms colourless crystals m.p. − 2 to − 3° C.

[2] This amorphous residue "mellon" formed the subject of numerous researches by Liebig, Gerhardt, etc. The originally proposed formula C_3N_4 was later amended to $C_6H_3N_9$ but its constitution has not yet been thoroughly established. Cf. Franklin, *J.A.C.S.*, 1922, **44**, 506.

Wöhler's mercury thiocyanate was certainly not a pure compound for his method of preparing it " durch Vermischung einer Auflösung von Schwefel-Cyankalium mit der Auflösung von saltpetersaurem Queeksilber Oxydul" was shown by Hermes [5] to yield not mercurous thiocyanate, but a mixture of mercuric thiocyanate and metallic mercury which is much less combustible than the pure mercuric salt which is readily ignited by a match.

The existence of mercurous thiocyanate was, indeed, for a long time a matter of controversy (cf. Hermes [6]) until Phillips [7] described the conditions essential to its formation. The latter points out that although the mercurous salt swells up like the mercuric salt on heating, the residue does not develop the same snake-like convolutions. It is of interest to note that the compounds HgCl(SCN) and HgBr(SCN) also intumesce on heating [8], whilst the selenium compounds $Hg_2(SeCN)_2$ and $Hg(SeCN)_2$ behave exactly like their sulphur analogues [9].

More than forty years elapsed before the commercial possibilities of Wöhler's interesting observation were realised by one Barnett and we then read of a new conjuring trick exhibited in Paris in 1865 in which the magical materialisation of " serpents de Pharaon " was undoubtedly to be attributed to small pellets of mercuric thiocyanate which thus appeared for the first time under this now familiar synonym [10].

On the Continent the new playthings rapidly became popular and they appeared simultaneously in Germany as " Pharao-schlangen " or " Riesenschlangen " and in Italy as " serpenti Indiani." In England, Fred Tolhausen [11] sealed a Patent in March 1866 for the manufacture of a " firework composed . . . of sulphocyanate of mercury."

However much one may admire the commercial acumen which so ingeniously described the tiny pellets of mercury thiocyanate as " Pharaoh's Serpents " eggs, one feels bound to deplore that felicity of description is not here wedded to an equally exact display of erudition. For though in Exodus [12] we read that Aaron " cast down his rod before Pharaoh . . . and it became a serpent," and that the magicians of Egypt " also did in like manner with their enchantments. For they cast down every man his rod, and they became serpents : but Aaron's rod swallowed up their rods," it is clear that these serpents had nothing pyrogenic in their origin or behaviour.

Furthermore, the " fiery serpents " of Numbers [13] which " bit the people : and much people of Israel died " had equally no relation to Pharaoh, and indeed harassed the Israelites at a

time when their vicissitudes under his rule could have been little more than a memory.

In view perhaps of such inconsistencies, Eulenberg [14] adopts the description "Phanarschlangen," deriving this from the Greek φαναριον (*i.e.* little flames, or lights): this derivation, however, if not due solely to some typographical error or another, can scarcely be regarded as anything more than a philological *jeu d'esprit.*

In 1882, H. Fleck [15] designated tiny pellets of mercuric thiocyanate as "Hinterladen" (*i.e.* breech-loaders), referring doubtless to the contemporary scatological humour which applauded their insertion, prior to their ignition, in the manner of a suppository [1] into the anus of a grotesque porcelain miniature (*choleramännchen*).

A recrudescence of this essentially Teutonic humour appears at intervals in this country in the guise of porcelain Chinamen which develop abundant black pig-tails, or, less delicately, of china pigs from which sprout phenomenally long and curly tails.

The poisonous properties of Pharaoh's Serpents were soon discovered, for the manufactured articles which consisted of cone-shaped fragments of the mercury salt wrapped in silver paper (tin-foil) were easily confused with a sweetmeat or bon-bon. "Prince O." is said to have very narrowly escaped death as a result of such a mistake [16], and his indiscriminate gourmandising certainly had a fatal effect upon an intestinal tape-worm which he harboured.

Since up to 1904 no actual deaths had resulted in Germany from the internal administration of mercuric thiocyanate, and but few cases of poisoning with serious or fatal consequences had been reported from elsewhere, the practical efficiency of the antidotes proposed by Helbig [17] and Th. Husemann [18] has not been adequately tested.

In order to promote their steady combustion it became customary for the manufacturer of Pharaoh's Serpents to add some 2 per cent. of potassium chlorate or nitrate. This certainly tended to increase the amount of free mercury vapour and was therefore discountenanced by many authorities [19].

On the other hand, Fleck [15] showed experimentally that during the combustion of one serpent's egg in a room, a process which occupied a minute, a negligible quantity only (0·00038 g.) of mercury

[1] *Suppository:* a medicated capsule of gelatine, etc., which when introduced into the anus or vagina melts therein and releases the medicaments (*e.g.* oil of theobroma, phenols, morphine, quinine) contained in it. Suppositories are used nowadays in cases of acute constipation, for treatment of vaginal and uterine disorders and in contraceptive practice.

vapour was inhaled. Husemann [20] confirms this opinion and avers that, in such concentrations, the gaseous products of the combustion become dangerous only to those whose personal idiosyncrasies render them peculiarly sensitive to mercurial poisoning.[1]

On account of the more imagined than real dangers of poisoning arising from the combustion of Pharaoh's Serpents, substitutes have from time to time been proposed.

Thus a mixture of potassium bichromate (2 parts), potassium nitrate (1 part), and white sugar (3 parts) was described in 1871 by Dr. Puscher [21], and Dr. Bersch recommends a mixture of ammonium bichromate (1 part), sugar (2 parts), potassium nitrate (1 part) and enough Peru balsam to yield a plastic mass.

Quite an elaborate substitute is described by Donath [22]. Cigar ash heaped up in the form of a cone is covered with three or four Emser Pastilles,[2] soaked in spirit, and ignited. The mound burns like a wick and just as the flame is about to go out "an adventurous serpent, corpulent and revolting, crawls out from the ash . . ." According to Helbig [17], the expensive Emser Pastilles may be replaced by a mixture of sodium bicarbonate (0·1 g.), sugar (0·9 g.), and tragacanth (0·03 g.).

The most, and perhaps the only effective substitute, and one which has been extensively adopted by the Trade, was first described by Vorbringer [23] so long ago as 1867. It consists of a resinous material obtained by the action of fuming nitric acid upon certain by-products of coal-tar distillation which, when ignited, exhibits "in hohem Grade die Eigenschaft des Rhodanquecksilbers, sich in seinem Volumen bedeutend zu vergroßern. Ein Kegel von 1″ Länge eine Schlange von 4′ lieferte." The preparation of this material is discussed by Vanino [24] and Musprat [25].

Pharaoh's Serpents appear to be perennially popular and a comparison of the earliest recorded analyses [10] with those of Dr. Vanino in 1904 [24] and with some analyses carried out by the author upon various contemporary specimens of "Snakes in the Grass," etc., reveal but little evidence of evolutionary tendencies among these Reptilia.

The modern "eggs" exhibit perhaps a more elaborate structure. In some, a foundation of sodium bicarbonate and sugar is overlaid with a resinous material which has the properties described by

[1] Cf. Stock, *The Hydrides of Boron and Silicon*, Cornell University Press, 1933, pp. 19, 203 f.

[2] Emser Salz, consisting essentially of sodium chlorate (1 part) and sodium bicarbonate (2 parts), takes its name from the hot alkaline springs of Ems, a German Spa near Coblenz.

Vorbringer, and the top of the cone is furnished with a mixture of picric acid and aluminium powder. In others, an inner cone of mercuric thiocyanate, admixed with a small amount of potassium chlorate, is overlaid with a layer of ammonium bichromate which furnishes the " grass " from which the " snake " subsequently creeps. This combination is by far the most effective. Following the prescription of Böttger [26], a spotted or a jet-black snake may be made by impregnating the mercury salt with an alcoholic solution of shellac, or dammar-resin respectively.

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DATING THE ICE AGE IN BRITAIN

By WILLIAM J. McCALLIEN, D.Sc., F.R.S.E.

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In the study of the rocks which make up the crust of the Earth the geologist has arrived at many ways of estimating time. On the whole, however, the methods allow of only approximate dates being given, but for all practical purposes the results are sufficiently accurate. There has been discovered one method of estimating the ages of certain sediments which is so exact that it enables us to tell the actual year in which the material was deposited. This is the method of the Swedish geologist Baron Gerard de Geer, and it has been extended by de Geer and his active pupils to most parts of the world. Baron de Geer came to Scotland in the autumn of 1934 in connection with the Centenary of the Geological Society of Edinburgh and since then he has published a paper [1] on the date of the Ice Age in this country. This paper, which is the first attempt to apply his method to the glaciation of Britain, opens up problems of great importance to geologists, biologists and archæologists. The present writer doubts if de Geer will find many supporters among British geologists for the views which he has expressed.

Before discussing the problem of the correlation of British and Scandinavian ice sheets which forms the basis of de Geer's dating, it will be pertinent to consider briefly first of all the method by which de Geer calculates glacial and post-glacial time. The type of sediment used in his studies is the time-recording clay called "varve," a term of Swedish origin meaning a turn or revolution, such as the complete round of the hands of a clock. Varved deposits are those in which the deposit of one year is distinctly marked off from the deposits of the preceding and following years. Although not restricted to glacial deposits, the finest and most abundant varves are undoubtedly of glacial origin.

The necessary condition for the formation of varved clays is a periodic supply of material, a large supply in summer and a

[1] "Dating of Late-Glacial Clay Varves in Scotland," by Gerard de Geer, *Proc. Roy. Soc. Edinburgh*, LV, Pt. I,—No. 2, 1935, p. 23.

small or no supply in winter. In varved clays of glacial origin, each layer during deposition in a lake stood in close relationship to the border of the ice. The material was carried to the lake by rivers of melt water. On entering the lake the coarse material was deposited as a delta, the finer material, consisting of sand and mud, being spread out fanwise in the waters of the lake. Of this fine material the sand was deposited first, the clay remaining longer in suspension. During the summer when the amount of ice melting was at its maximum, a great quantity of sand and mud reached the lake. Most of the clay, however, remained in suspension until the winter, and when there was no melting and the surface of the lake was frozen over this clay was deposited.

As the edge of the ice melted back a new part of the area became ice-free and so each subsequent varve was laid down over the ends of the older varves. As the distance of deposition of the material from the mouths of the sub-glacial rivers increased so the varves decreased in thickness and in size of grain, their relative thicknesses, however, remaining constant.

Varve clays such as we have just described are known to be forming at the present day in Lake Louise in the Canadian Rockies, where they have been studied by W. A. Johnston.

According to de Geer, since the melting of the ice and, therefore, the thicknesses of the varves are controlled by the summer temperature, the deposit of any year in one locality can be matched with the deposit of the same year in another locality. In this way correlation can be made from one place to another, and gaps in the sequence can be bridged by finding the corresponding section in another locality. De Geer actually believes the varves are capable of identification all over the world, and already he has made correlations between his type sections in Sweden and sections from across the Atlantic and from the Himalaya. He began his life work as long ago as 1878, but it was not till 1904 that he succeeded in getting a successful correlation between two sections one kilometre apart.

In order to correlate in a convenient form the varves in one place with those at another, the record of each station is translated into a curve with the succession of layers entered on the abscissa and the thickness of each layer indicated on the ordinate (Fig. 1). On such a curve thick layers have high points, and thin layers have low points. For the purpose of his Scottish correlation which he has just published in the paper referred to, de Geer was furnished with a varve-measurement from near Dunning in the valley of the River Earn, by Dr. J. B. Simpson of the Geological Survey of

Scotland. This section contained 59 annual layers and its curve is shown in Fig. 1. De Geer considers that the agreement between this curve and another from Lyngby, about 6 kilometres north-north-west of Copenhagen, is sufficient for us to say that the varves in these two localities were deposited in the same years. In this

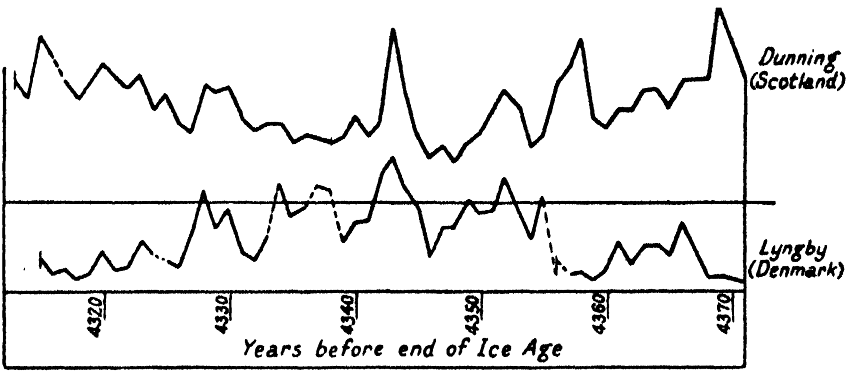


FIG. 1.—The varve sections at Dunning, Scotland, and at Lyngby, Denmark, compared with one another (de Geer).

way the Scottish clays from Dunning are dated by de Geer at from 4,313 to 4,371 years before the end of the Ice Age in Sweden. The end of the Ice Age in Sweden is dated at 8,700 years before the present century. That is to say, the Dunning clays were deposited at from 13,048 to 13,106 years ago. It will be seen from Fig. 2 that de Geer correlates clays deposited during the District or Moraine

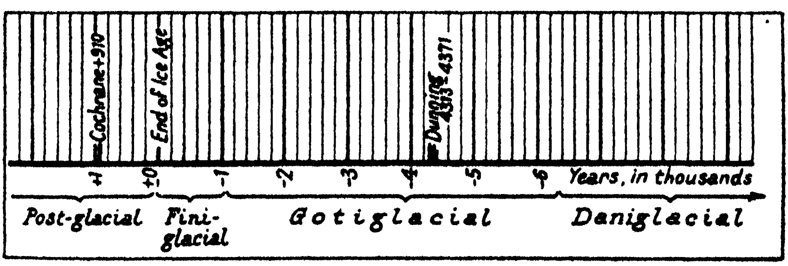


FIG. 2.—The Swedish Time-Scale with de Geer's interpretation of the age of the Dunning varves.

Glaciation of the Highlands with the Gotiglacial phase of the retreat of the Fourth Glaciation in the Baltic region. So confident is de Geer that his conclusion is correct that he states in the concluding paragraph of his paper that, "Now that it has been possible to obtain accurate teleconnection between Late Quaternary varves in Scotland and the Swedish time-scale, it seems highly desirable by continued varve-measurements to elaborate a closer parallelisa-

tion concerning other stages of the last deglaciation of Scotland and other countries."

The correlation of the British and Scandinavian and Alpine glaciations has been attempted by a great number of people not all of whom were geologists. The correlations have not always been very happy and no useful purpose would be served by attempting to review them here. Suffice it to say that the present writer's views are based primarily on the work of Professor J. Kaye Charlesworth of Belfast University, who has done more than any other glacialist to bring order out of chaos in this connection.

Without dealing with the Older Drift glaciation of the British Isles, the stages in the retreat of the Fourth Glaciation are those which we shall correlate with the phases in the retreat of the Fourth Glaciation in the Baltic region. In the British Isles these stages are the Newer Drift itself, the N.E. Antrim–Isle of Man–Cumberland stage, the Lammermuir–Stranraer stage and the District or Moraine stage. In the Baltic region, as indicated in Fig. 2, the Fourth Glaciation consisted of at least three stages, the Daniglacial, Gotiglacial, and Finiglacial stages, followed by the Post-Glacial phase. Without going into detail it would seem that (1) the District Glaciation of Britain corresponds to the Post-Glacial of Scandinavia, (2) that the Lammermuir–Stranraer stage corresponds to the Finiglacial phase and (3) the N.E. Antrim–Isle of Man–Cumberland stage corresponds to the Gotiglacial phase.

The varve-section on which de Geer has based his Scottish-Baltic correlation is exposed on the right bank of the River Earn, Perthshire, three-quarters of a mile downstream from Dalreoch Bridge, near Dunning, and according to Dr. Simpson when the Dalreoch varves were formed the glacier occupying the valley of the Earn probably had its front in the vicinity of Auchterarder, or it may have retired as far as Crieff. According to the correlations suggested above the present writer would expect the Dalreoch clays to be matched by clays of younger date than the Finiglacial phase, actually in the Post-Glacial stage of the Swedish geologists. This agrees with the suggestion of Dr. Simpson himself that the oscillation of the glacier responsible for the deposition of moraine over the varve clays may be correlated with an oscillation of the ice at Cochrane in Canada which de Geer has placed in the Post-Glacial stage, nearly 1,000 years after the end of the Ice Age (7,735 years ago).

De Geer's pupil Antevs has already pointed out several reasons for not believing that varve-agreements, such as are indicated in Fig. 1, necessarily mean the contemporaneity of the deposits.

The conclusions expressed here that the District Glaciation began some 9,000 years ago, and that the Dunning clays are younger than this, receives a certain amount of support from the study of the Late-Glacial sea level. The 100-foot beach in Scotland is generally believed to be synchronous with the Yoldia Sea in the Baltic, and the latter existed when the Scandinavian ice stood at the position of the Ra and Salpausselka moraines at the beginning of the Finiglacial phase. This, however, was the final stage in the 100-foot sea in Scotland, for we know that before the end of the District Glaciation the sea level had fallen from 100 feet to about 30 feet above present sea level.

In Scandinavia the Yoldia Sea was brought to an end by an uplift of the land, and a new phase, the Ancylus Lake, of rather long duration was ushered in. This came to an end about 5000 B.C. by the opening up of direct connection between the Baltic and the North Sea. Then began the Littorina phase which we correlate with the Mesolithic 25-foot raised beach of Scotland. The maximum of this transgression is believed to have existed about 4000 B.C., and this point is of considerable importance, for it was at this time, or shortly before it, that man appears to have arrived in Scotland.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By E. Maitland Wright, M.A., D.Phil., Christ Church, Oxford.

Van der Corput's Method.—A large number of problems in the theory of numbers are concerned with the order of certain functions as the argument tends to infinity. Many of these problems can be reduced to the problem of finding an upper bound for a sum of the form

$$\sum_{a \leq n \leq b} e^{2\pi i f(n)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

where $f(n)$ is a real function. In the last resort we can only say that

$$\Big| \sum_{a \leq n \leq b} e^{2\pi i f(n)} \Big| \leq b - a, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

is the crude approximation. But van der Corput has shown that under suitable conditions a sum of the form (1) can be made to depend on another sum of the same form but with different elements, such that the crude approximation (2) applied to the second sum gives an upper bound for the first sum which is far from crude. Van der Corput used the following two theorems :

A. *If $f(x)$ is a real function, and ρ is a positive integer not exceeding $b - a$, then*

$$\Big| \sum_{a \leq n \leq b} e^{2\pi i f(n)} \Big| = O\left(\frac{b-a}{\sqrt{\rho}}\right) + O\left\{\frac{b-a}{\rho} \sum_{s=1}^{\rho-1} \Big| \sum_{a \leq n \leq b-s} e^{2\pi i (f(n+s) - f(n))} \Big| \right\}^{\frac{1}{2}}$$

B. *Let $f(x)$ be a real function with continuous differential coefficients of the first three orders which satisfy the following conditions :*

(i) *$f'(x)$ is steadily decreasing in the interval (a, b),*

(ii) $m_2 \leq | f''(x) | < Am_2$,

(iii) $| f'''(x) | < Am_3$.

Let $f'(b) = \alpha$, $f'(a) = \beta$, and n_ν be such that $f'(n_\nu) = \nu$ (an integer) for $\alpha \leq \nu \leq \beta$. Then

$$\sum_{a \leq n \leq b} e^{2\pi i f(n)} = e^{-\frac{1}{4}\pi i} \sum_{\alpha \leq \nu \leq \beta} \frac{e^{2\pi i (f(n_\nu) - \nu n_\nu)}}{| f''(n_\nu) |^{\frac{1}{2}}} + O(m_2^{-\frac{1}{2}})$$

$$+ O(\log \{2 + (b - a)m_2\}) + O((b - a)m_2^{\frac{1}{5}}m_3^{\frac{1}{5}}).$$

It is easily seen that A by itself is of no use, but if we use the crude approximation in the sum on the right of the equation in B we shall obtain an inequality of the form

$$\sum_{a \leq m \leq b} e^{2\pi i f(n)} = O\left(\frac{\beta - \alpha}{m_2^{\frac{1}{2}}}\right) + \ldots$$
$$= O\left(\frac{f'(a) - f'(b)}{m_2^{\frac{1}{2}}}\right) + .$$

The best form of this result is the following theorem, which owes its final shape to Titchmarsh and which can be proved independently of B :

B′. *If $f(x)$ is real and has continuous first and second differential coefficients and $f''(x) \geq r$ (or $\leq -r$) throughout the interval (a, b) then*

$$\sum_{a \leq n \leq b} e^{2\pi i f(n)} = O\left(\frac{f'(b) - f'(a)}{\sqrt{r}}\right) + O\left(\frac{1}{\sqrt{r}}\right).$$

By repeated use of A followed by an application of B′ we obtain the following useful theorem :

Theorem. Let k be a positive integer, $K = 2^{k-1}$, $f(x)$ a real function with continuous differential coefficients of the first $k + 1$ orders and let

$$r_k \leq f^{(k+1)}(x) \leq R_k \text{ (or } - R_k \leq f^{(k+1)}(x) \leq -r_k)$$

throughout the interval (a, b). Then

$$\sum_{a \leq n \leq b} e^{2\pi i f(n)} = O\left(\frac{(b-a)R_k^{1/(2K-1)}}{r_k^{1/(4K-2)}}\right) + O\left(\frac{(b-a)^{1-\frac{1}{K}} R_k^{\frac{1}{K}} r_k^{\frac{1}{4K-2}}}{r_k^{1/K} R_k^{1/(2K-1)}}\right).$$

If, in addition, $R_k < Ar_k$, this becomes

$$\sum_{a \leq n \leq b} e^{2\pi i f(n)} = O((b-a)r_k^{1/(4K-2)}) + O((b-a)^{1-\frac{1}{K}} r_k^{-1/(K+2)}).$$

This is the machinery of van der Corput's method. It has been used by van der Corput himself to prove the following result in the divisor problem :

$$\sum_{n \leq x} d(n) = x \log x + (2C - 1)x + o(x^{33/100}).$$

Nuland used it to prove the following result for the circle problem, *viz.* that if $R(x)$ is the number of solutions in integers of the inequality $u^2 + v^2 \leq x$ then

$$R(x) = \pi x + O(x^{27/82}).$$

For a simple proof of this result we may refer the reader to papers by Professor Titchmarsh (*Quarterly Journal of Mathematics*, **3**,

5, 1932 and 1934). Titchmarsh has also used this method to get improved results in the error term in certain mean-value theorems of the zeta-function and to investigate the zeros of $\zeta(\frac{1}{2} + it)$. Eric Phillips in Vol. 4 of the same periodical has used the method to prove that

$$\zeta(\tfrac{1}{2} + it) = O(t^{229/1392}).$$

In some cases, *e.g.* the circle problem, the sum to be bounded is really a double sum of the form

$$\Sigma\Sigma e^{2\pi i f(m,\, n)}.$$

Titchmarsh has extended the old van der Corput method to a two-dimensional form which finds upper bounds for these sums directly. For example, in place of Theorem A, he uses the following theorem :

Let $f(x,y)$ be a real function of x and y, defined for $a \leq x \leq b$, $\alpha \leq y \leq \beta$; let

$$S = \sum_{m=a}^{b} \sum_{n=\alpha}^{\beta} e^{if(m,\, n)},$$

$$S_1 = \sum_{m=a}^{b-\mu} \sum_{n=\alpha}^{\beta-\nu} e^{i\{f(m+\mu,\, n+\nu) - f(m,\, n)\}}$$

$$S_2 = \sum_{m=a}^{b-\mu} \sum_{n=\alpha+\nu}^{\beta} e^{i\{f(m+\mu,\, n-\nu) - f(m,\, n)\}}$$

Let ρ be a positive integer not exceeding $b - a$, and ρ' a positive integer not exceeding $\beta - \alpha$. Then

$$S = O\left\{\frac{(b-a)(\beta-\alpha)}{(\rho\rho')^{\frac{1}{2}}}\right\} + O\left\{\frac{(b-a)(\beta-\alpha)}{\rho\rho'} \sum_{\mu=1}^{\rho-1} \sum_{\nu=0}^{\rho'-1} |\, S_1 \,|\right\}^{\frac{1}{2}}$$
$$+ O\left\{\frac{(b-a)(\beta-\alpha)}{\rho\rho'} \sum_{\mu=0}^{\rho-1} \sum_{\nu=1}^{\rho'-1} |\, S_2 \,|\right\}^{\frac{1}{2}}.$$

In place of Theorem B′ the following lemma is used :

Let $f(x,y)$ be a real differentiable function of x and y. Let $f_x(x,y)$ have $O(1)$ maxima and minima, considered as a function of x, for each value of y considered, and let $f_y(x,y)$ satisfy a similar condition as a function of y, for each value of x considered. Let

$$|\, f_x(x,y) \,| \leq \tfrac{3}{2}\pi, \quad |\, f_y(x,y) \,| \leq \tfrac{3}{2}\pi$$

for $a \leq x \leq b$, $\alpha \leq y \leq \beta$, *where* $b - a \leq l$, $\beta - \alpha \leq l (l \geq 1)$. *Then*

$$\sum_{m=a}^{b} \sum_{n=\alpha}^{\beta} e^{if(m,\, n)} = \int_a^b \int_\alpha^\beta e^{if(x,\, y)}\, dx\, dy + O(l).$$

A further theorem provides an upper bound for the double integral. There are a variety of such theorems of which the following is an example:

Let $f(x,y)$ be a real function with continuous partial derivatives up to the third order, and let

$$r \lesssim |f_{xx}| < Ar,\ r \lesssim |f_{yy}| < Ar,\ |f_{xy}| < Ar,$$
$$|f_{xx}f_{yy} - f_{xy}^2| \geq r^2$$

throughout the rectangle $a \leq x \leq b$, $\alpha \leq y \leq \beta$, where $b - a \leq l$, $\beta - \alpha \leq l$. Then

$$\int_a^b \int_\alpha^\beta e^{if(x,y)} dx\, dy = O\left(\frac{1 + |\log l| + |\log r|}{r}\right).$$

By this two-dimensional method Titchmarsh has proved the following results:

$$Z(\tfrac{1}{2} + it) = O(t^{\frac{1}{4}} \log^3 t),$$

where $Z(s) = \sum_{m=-\infty}^{\infty} \sum_{n=-\infty}^{\infty} (m^2 + an^2)^s$, a particular case of the Epstein zeta-function:

$$R(x) = \pi x + O(x^{15/46} \log^{77/72} x),$$

where $R(x)$ is the function mentioned above.

THE METRIC THEORY OF CONTINUED FRACTIONS.—In a recent paper in *Compositio Mathematica* (**1**, 1935, 361–82), A. Khintchine proves several attractive theorems in this theory. Let α denote a number between 0 and 1, whose development as a simple continued fraction is denoted by

$$a_1 + \frac{1}{a_2 + \ldots} + \frac{1}{a_n + \ldots} = [a_1, a_2, a_3, \ldots, a_n, \ldots].$$

In 1928 Kuzmin proved the theorem (conjectured by Gauss) that, if $z_n(\alpha) = [a_{n+1}, a_{n+2}, \ldots]$ and if $m_n(x)$ is the measure of the set of numbers α such that $z_n(\alpha) < x$, $(0 \leq x \leq 1)$, then

$$\lim_{n \to \infty} m_n(x) = \frac{\log (1 + x)}{\log 2}.$$

Let $E\begin{pmatrix} n_1, n_2, \ldots, n_k \\ r_1, r_2, \ldots, r_k \end{pmatrix}$ be the set of those numbers whose development as a simple continued fraction satisfy

$$a_{n_1} = r_1, a_{n_2} = r_2, \ldots, a_{n_k} = r_k,$$

and let ME be the measure of the set E. Khintchine proves two lemmas :

$$1.\quad \frac{\mathrm{ME}\begin{pmatrix} n_1, \ldots, n_k, n_{k+1} \\ r_1, \ldots, r_k, r_{k+1} \end{pmatrix}}{\mathrm{ME}\begin{pmatrix} n_1, \ldots, n_k \\ r_1, \ldots, r_k \end{pmatrix}} < {}_{r_{k+1}^{-}}$$

where C is a constant and $n_1, \ldots, n_{k+1}$ *are all different.* (This is a generalisation of a result due to Borel.)

$$2.\quad \frac{\mathrm{ME}\begin{pmatrix} n_1, \ldots, n_k, n_{k+1} \\ r_1, \ldots, r_k, r \end{pmatrix}}{\mathrm{ME}\begin{pmatrix} n_1, \ldots, n_k \\ r_1, \ldots, r_k \end{pmatrix}} + \frac{\log\left(1 - \dfrac{1}{r(r+2)}\right)}{\log 2}$$

$$< \mathrm{B} \exp\{-\beta(n_{k+1} - n_k)^{\frac{1}{2}}\},$$

where $n_1 < n_2 < \ldots < n_k < n_{k+1}$, and B and β are absolute constants. Khintchine then proves the following generalisation of Kuzmin's theorem : *If $f(r)$ is a non-negative function such that $f(r) = O(r^{1-\delta})$ for large r and some $\delta > O$, then for " almost all " α (that is, for all α except those of a set of measure zero)*

$$\lim_{n \to \infty} \frac{1}{n} \sum_{k=1}^{n} f(a_k) = \sum_{r=1}^{\infty} f(r) \frac{\log\left(1 + \dfrac{1}{r(r+2)}\right)}{\log 2}.$$

If we take $f(r) = \log r$, this becomes

$$\lim_{n \to \infty} \sqrt[n]{a_1 \ldots a_n} = \prod_{r=1}^{n} \left(1 + \frac{1}{r(r+2)}\right)^{\log r/\log 2} = 2{\cdot}6 \ldots$$

A theorem on the arithmetic mean of the partial quotients a_n is as follows : If $s_n = a_1 + \ldots + a_n$ and if E_n is the set of those α for which

$$\frac{s_n/n}{\log n/\log 2} - \quad > \varepsilon$$

then the measure of E_n tends to zero as $n \longrightarrow \infty$. We have as an immediate corollary of this last result the theorem that the series

$$\frac{1}{a_1} + \frac{1}{a_1 a_2} + \frac{1}{a_1 a_2 a_3} + \ldots$$

is divergent for " almost all " α.

ASTRONOMY. By R. W. Wrigley, M.A., F.R.S.E., Royal Observatory, Edinburgh.

The Evolution of the Solar System.—According to the Planetesimal Hypothesis of Chamberlin and Moulton the planets owe their birth to another star which approached the sun probably within the limits of the present solar system. Through tidal action masses of varying dimensions were ejected from both bodies while they remained in each other's neighbourhood, and, after the visiting star had departed, much of the ejected material was left revolving round the sun in orbits of varying sizes, eccentricities and orientations. Some of these masses were large enough to remain in a gaseous or liquid condition, but the majority (the planetesimals) were small and speedily cooled to a solid state. The larger nuclei grew in size by gradually sweeping up the planetesimals, with the result that their orbits became progressively more circular and more nearly in one plane. The earth, therefore, grew from a nucleus of unknown size by a gradual acquisition of planetesimals just as it now sweeps up some 20,000,000 meteors each day, and its outer layers were always solid. The hypothesis is supported by the fact that the larger planets have orbits with the smaller eccentricities, and their planes are almost coincident.

Dr. H. Jeffreys in his book *The Earth* has pointed out that the planetesimals would be more likely to volatilise completely before they could be swept up and formed into planets. This objection does not, however, apply to a new condensation theory of meteoric matter propounded by Professor Bertil Lindblad in *Meddelande*, No. 18, Stockholm Observatory (see also *Nature*, 1935, Jan. 26). This theory is founded on the great difference in temperature supposed to exist between interstellar gas and solid interstellar particles, the former having a temperature approaching 10,000°, and the latter one of but little above zero absolute. The solid particles will therefore presumably grow in size by the condensation on their surfaces of sublimal matter from the surrounding gas which may be assumed to contain all the elements found in the sun. Adopting Gerasimovic and Struve's value of 10^{-26} gm. per cu. cm. for the density of the interstellar gas, Lindblad calculates that particles of mass 10^{-15} gm. will form in a period of 10^{9} years. According to C. Schalen (*Upsala Meddelande*, No. 58) this is the order of magnitude of the particles in the clouds of obscuring matter in the Milky Way, while the period of 10^{9} years agrees with the average age of meteorites as derived from an analysis of their radioactive material.

Assuming that the solar system originated from a gaseous nebula with a concentration towards the central nucleus and extending

beyond the orbits of Neptune and Pluto, Lindblad estimates the density in the outer regions as 10^{-15} gm. per cu. cm. and deduces that in 10^{9} years there would be developed bodies of 10^{18} gms., equal in mass to the smallest of the asteroids. A certain number of much larger bodies would be formed by the occasional combination of smaller ones, while, on the other hand, many of the planetesimals in the denser regions near the central nucleus would be volatilised by violent encounters. The final result would be the growth of a comparatively small number of large bodies which we know as the planets and their satellites. The earth is imagined as being formed round a heavy core by the accumulation of a great number of smaller bodies of varying size; at times when this accumulation was specially rapid the surface layers may have become heated to a fluid or even incandescent state, but solidification would follow through loss of heat by radiation. The formation of the systems of satellites is explained in a similar way, the planet in its early state consisting of an incandescent nucleus with an extended gaseous atmosphere. It is pointed out that this conception leads to the same direction of rotation of the planets in their orbits and around their axes, and of the satellites about the planets, the few known exceptions being readily explained as due either to capture in recent times as in the cases of Jupiter's satellites VIII and IX, or to local conditions as in the Uranus and Neptune systems. It is further suggested that the particles forming the Zodiacal Light represent the last remains of the nebulous cloud which once surrounded the sun. Meteorites are regarded as remnants of larger bodies shattered in the past by violent encounters.

In *Monthly Notices*, R.A.S., **94**, No. 9, Dr. Jeffreys, in a note on the constitution of the inner planets, draws attention to the fact that for each of these four bodies, and also for the moon, the density increases with the size, that of the moon being less than two-thirds that of the earth, the largest of the group. This he attributes to a difference in their composition, and not to the extra compression in the larger bodies. Geophysical evidence indicates that the earth consists of a silicate shell resting on a core of liquid heavy metals, the density of the shell being nearly equal to the average density of the moon. Jeffreys infers that the moon is probably devoid of a metallic core, and that the variation of density among the four inner planets is caused in each case by the varying size of this core. According to the tidal theory developed by Jeans the filament which gave rise to the planets broke up as fast as it emerged from the sun, and the inner planets, while losing their external gases, should have retained the same constitution as the

sun as regards materials of high boiling-point. We would therefore expect even such small bodies as Mercury and Mars to have retained their due proportion of iron and other heavy metals. The smaller densities of these bodies suggest, according to Jeffreys, that the materials must have had time to separate after leaving the sun. If the terrestrial planets were originally formed as only one or two bodies in which the heavy metals had time to settle to the centre before a fresh rupture occurred, the present observed densities could be explained, but how and why this latter rupture should take place remains an insoluble problem.

The explanation may, however, be more simple. The original Planetesimal Hypothesis of Chamberlin and Moulton postulates the existence of nuclei of varying sizes which gradually developed into the planets by sweeping up the small planetesimals. It is possible that the size of the original nucleus was in each case largely responsible for the density of the resultant planet. Further, if the moon was born out of the earth after the heavy metals had sunk towards the centre, then the similarity between the lunar density and that of the earth's crust needs no further explanation.

Meteorites and the Age of the Universe.—A summary in *Nature*, **125**, 490, describes how Professor Paneth of Königsberg has recently determined the age of various meteorites by methods similar to those already used in dealing with terrestrial rocks, consideration being given to their relative content of helium and radium. The investigation was mainly directed to iron meteorites, which were assumed to retain their helium firmly up to a temperature of 1,000° C. It was therefore possible to estimate an upper limit for the time elapsed since the meteorite possessed a temperature high enough to permit the escape of its helium. In every case which he examined Paneth found that the age could not exceed 3,000,000,000 years, a limit close to the generally accepted estimate of the age of the earth and the solar system. He therefore concludes that all meteorites must be of solar origin, and that the hyperbolic velocities which have been found for many fireballs must be due to erroneous observations. In *Popular Astronomy*, XLI, No. 2 (*Harvard Reprint*, **84**), Ernst Öpik disputes this conclusion on the ground that all the observations of hyperbolic velocity in meteorites cannot thus be lightly swept aside as inaccurate, and he cites in particular the Poltusk stone shower of January 1868, when the observations were practically beyond question and the interstellar origin of the stones was proved almost without shadow of doubt. On the other hand, Paneth has made a special examination of a sample from one of these very meteorites and finds the age to be

only 500,000,000 years. Öpik accepts both this age estimate and the observation of hyperbolic velocity to be substantially correct, and reconciles them by the assumption that the stellar universe whence all meteorites with hyperbolic velocity must come, is of the same order of age as the solar system. Several arguments are put forward for thus drastically reducing the usual estimate of the age of the universe. Undoubtedly a much longer interval of time must be conceded if a star, losing its mass in the form of quanta of light, is to decrease gradually in luminosity and pass through the consecutive stages of evolution represented by the different classes of spectrum, size and luminosity now observable in the sky, but Öpik points out that no positive evidence has yet been found that stars have really changed from one class to another. It is quite possible that the observed variety in luminosity, mass and spectrum is not the result of evolutionary change, but is merely due to initial conditions, size and chemical constitution. Certain other evidence is not inconsistent with a comparatively low estimate of the age of the universe. The distribution of stellar luminosities in globular clusters, statistical data relating to wide double stars, and the observed rate of recession of the spiral nebulæ resulting in a general rapid expansion of the universe are all put forward by Öpik as supporting the view that the universe, though the parent, is not much older than its child, the earth, and has existed in its present form only about 3,000,000,000 years.

The study of the cosmical origin of meteors was the main object of the Arizona expedition organised by Harvard College Observatory in 1931–33. The distribution of their heliocentric velocities with regard to size and direction; the relative proportion of solar and hyperbolic meteors; the distribution of velocities of the hyperbolic meteors outside the solar system; the frequency function of the relative masses of meteors; the absolute density of meteoric matter in space;—these were the general questions to the solution of which the observations were directed. The plan of the latter is described in *Proc. Nat. Acad. Sci.*, **18,** No. 1 (*Harvard Reprint*, **74**) by Shapley, Öpik, and Boothroyd. Provision was made for both visual and photographic observations, and the instruments included ingenious devices for measuring the angular velocities of the meteors. The results are gradually being published in a series of Harvard Circulars. In *Circular 388* E. Öpik makes a statistical investigation of the general distribution of radiants, making use of about 26,000 naked eye observations of 22,000 individual objects obtained on 366 nights. Following a description of his method of testing the reality of radiants, he gives a list of 279 of which 223 are considered to be real radiants

of sufficient concentration. There is apparently an interesting seasonal variation, for in autumn the number of meteors belonging to real radiants equals 26 per cent. of the total, while during the rest of the year the percentage is only 15. In *Harvard Circular 389* Öpik discusses the velocities of the meteors observed visually, and finds a number of high heliocentric velocities whose appearance cannot be accounted for by possible observational errors. As the observed values are those of the projected components, and as a large proportion of these exceed the parabolic limit of 42 km./sec., the real space velocities must be still higher. A similar conclusion regarding the frequency of hyperbolic velocities is reached by S. L. Boothroyd (*Harvard Circular 390*) from his examination of the telescopic observations. He finds that the frequency of high velocities is greater for faint than for bright meteors. In *Circular 391* Öpik reaches the same result from a more detailed statistical analysis. He finds that solar meteors, with a velocity less than 42·4 km./sec., represent the minority, the percentage of high velocities increases with decreasing brightness, and the largest velocities reach an order of 200 km./sec. The observations indicate that all solar naked eye meteors belong to real radiants, while all "sporadic" meteors come from outer space, but possibly belong to our own galactic system. Telescopic meteors (but not the naked eye observations) show a gap in the distribution of velocities corresponding to the stellar velocities of our "Local Cluster" of stars, but have a strong maximum of frequency round the velocity 143 km./sec.

Meteor spectra are difficult to observe, either visually or by photography. Prior to 1885 all the observations were necessarily visual, and were generally made with a binocular arrangement of two prisms, giving a field of view about 30° in diameter. They indicated a continuous spectrum crossed by certain bright lines considered to be due to sodium and magnesium, a predominance of the latter giving the meteor a definitely green colour. After the introduction of objective prism photography meteor spectra were occasionally secured, generally by chance, when a bright meteor happened to cross the field, but naturally such occasions were few. In *Harvard Annals*, **82**, 6, P. M. Millman has made an examination of all such spectra readily obtainable. They were nine in number, photographed between 1897 and 1931, two being secured at Arequipa, three at Moscow, two at Mount Wilson, and one at Bergedorf. The ninth of the series was taken at Blue Hill Meteorological Observatory, Massachusetts, in 1931, when four cameras were erected and 120 plates were exposed with average exposures

of one hour each. As only one serviceable meteor spectrum was secured over the whole series, it is unnecessary to emphasise the difficulties. All the nine spectra show well-defined bright lines, varying in number from 6 to 53, with but faint continuous spectrum. All the lines have been identified with elements common in meteorites, iron being found in every case, neutral calcium in 4 and ionised calcium in 6, while other elements identified are magnesium, manganese, aluminium, chromium and silicon. The line intensities were determined with a microphotometer in five cases and by eye estimates for the remainder, the results being exhibited in a graphical plot. Three of the meteors belonged definitely to the Perseid shower, but their spectra are not noticeably more alike than the other six. The continuous spectrum seems to be due almost entirely to unresolved weak emission lines. The excitation potential of the upper level of the multiplets identified was in every case under six volts. In the four cases where microphotometric intensity measures were made, the iron vapour was found to be at states of excitation corresponding to furnace temperatures ranging from 1,680° to 2,800° Absolute. In short, the results derived from these nine meteor spectra are very much as would be expected from consideration of atomic physics, and the known constitution of meteorites.

PHYSICS. By L. F. Bates, D.Sc., Ph.D., F.Inst.P., University College, London.

The Problem of the Semi-Conductor.—The recent appearance of several papers on the electrical properties of semi-conductors draws attention to the theoretical and technical importance of these materials. It cannot be said that there exists at present a theory which completely accounts for all their electrical properties, but during the last four or five years some kind of order has been introduced into the theoretical statement of the problems concerned, mainly owing to the work of A. H. Wilson (*Proc. Roy. Soc.*, A **133**, 458 and A **134**, 277, 1931), Bornstein (*Phys. Zeit. der Sowjet Union*, **2**, 28, 1932), Frenkel and Joffé (*ibid.*, **1**, 60, 1932), while R. H. Fowler (*Proc. Roy. Soc.*, A **140**, 505, 1933) has given an elementary mathematical treatment of the modern ideas, and Gudden (*Ergeb. der exakt. Wissenschaften*, XIII, 223, 1934) has given an interesting survey of the experimental facts.

These facts are complex and not easy to interpret, for in the first place the measurement of the resistance of a semi-conductor, particularly if it happens to be in the form of powder, often does not lead to definite results. In attempting to find the true resist-

ance which the material would possess when in an ideal form for resistance measurements, there are to be avoided the enormous effects of surface contacts and the effects of gases, of which rectification and "Sperrschicht" phenomena all serve to remind us. In the second place, the conductivity may be of electronic or of electrolytic nature, and the corresponding phenomena may not be easy to disentangle when both exist side by side. However, it is mainly the electronic type of conduction which will be dealt with in this article.

Now, while we may attribute a definite resistivity to a pure metal, we find that it is impossible to state what is the true resistivity of a semi-conductor. In the latter case the effect of impurities is enormous; indeed, we are practically forced to take the view that the entire conductivity of an electronic semi-conductor is due to the presence of minute quantities of impurities contained in what would otherwise be a perfect insulator. Consequently, the resistivities of different samples of a particular material may vary within wide limits even though they are prepared under identical conditions.

Semi-conductors usually exhibit a well-marked variation of electrical conductivity with temperature, the conductivity following the equation

$$\sigma = \mathrm{A} \,.\, e^{-\mathrm{E}/k\mathrm{T}},$$

where A and E are constants. In other words, the logarithm of the conductivity is a linear function of the reciprocal of the absolute temperature, the slope of the line being the greater, the smaller the conductivity of the specimen at room temperature; Fritsch's results with zinc oxide (*Ann. der. Phys.*, **22**, 375, 1935) provide a very nice illustration of this statement. The magnitude and sign of the Hall coefficient R of a semi-conductor is of great importance, first, because the sign tells us a good deal about the nature of the carriers responsible for conduction, and second, because the product of the conductivity and Hall coefficient gives us valuable information concerning the mobilities of the carriers; its temperature variation will later be shown to be of great importance, because it shows the extent to which oppositely charged carriers take part in conduction. Thus for zinc oxide Fritsch deduces from the smallness of the product R . σ and its small variation with temperature, that two oppositely directed Hall effects are present.

An attempt will now be made to give an insight into the present state of the theoretical aspects of the problem. The conduction

electrons in a crystal lattice may be supposed to occupy a band or series of bands of energy levels. For simplicity it may be considered that at most there are two bands, one, called the lower, corresponding to the energy levels when the metal is at absolute zero. If the lower band is incomplete, *i.e.* according to the Fermi-Dirac statistics, some of the higher energy levels are unoccupied, then, by rise in temperature or under the influence of an electric field, electrons may pass from lower to higher levels, and so be able to travel from atom to atom as Sommerfeld and others have assumed in their electron theories of metallic conduction. Such a band may reasonably be assumed in the case of the alkali metals.

If, however, the lower band is filled, the possibility of metallic conduction depends on the nearness of the other band. The latter may actually overlap the lower band to some extent, and this, it has been suggested, explains the conductivity of the alkaline earths. Suppose, now, that the lower band is completely filled, and the other, empty, band is so far removed, *i.e.* there exists so large a difference in the average energy level between the two bands, that electron transitions become much more difficult or unlikely. Then the electrons cannot be handed on from atom to atom, and we have a perfect insulator. There must, of course, be cases in which the electrons may pass from one band to the other by thermal agitation, and such substances Fowler (loc. cit.) calls *intrinsic* semi-conductors. The conductivity may then be computed by classical methods as the number of electrons involved is small. Let us note that, on this theory, when an electron leaves the lower band it leaves a hole therein ; this is theoretically equivalent to a positive electron.

In general, a semi-conductor is formed when a new distinct band or set of levels of intermediate energy is supplied by the impurities present. If this impurity band is full of electrons at low temperatures, then electrons may pass either from the impurity band or from the lower band in the substance to the upper, the first transition being the more common at ordinary temperatures, and such a semi-conductor is styled *normal extrinsic*. On the other hand, if the impurity band is empty at low temperatures, then electrons may pass from the lower band to the impurity band, and thence to the upper band ; such semi-conductors are termed *abnormal extrinsic*. The terms *normal* and *abnormal* are used because the corresponding semi-conductors usually possess Hall coefficients of normal and abnormal sign, respectively.

Electrical conduction may therefore be pictured as the migration of the electrons, which have undergone transitions, towards the positive terminal, and as a migration of the positive holes in

the lower band towards the negative terminal. It is the latter migration which may give rise to an abnormal Hall coefficient. This is the basis of Fowler's elementary treatment. He shows that the temperature variation of σ with T, mentioned above, is satisfactorily explained on these lines. He shows too that an *intrinsic* semi-conductor and a normal metal must always form a thermocouple with an E.M.F. from metal to semi-conductor at the low-temperature junction. The same is true for a *normal extrinsic* semi-conductor, but the E.M.F. of an *abnormal extrinsic* may vanish and change sign as the temperature difference between the junctions increases. On the old classical electron theory semi-conductors were all expected to behave in the same way, the thermoelectric current passing from a good to a semi-conductor at the cold junction. The discussion of the Hall effect shows that *intrinsic* and *normal extrinsic* semi-conductors must exhibit a normal Hall coefficient, corresponding to the deflection of electrons, but that the *abnormal extrinsic* will give an abnormal Hall coefficient at low temperatures, which may vanish and become normal as the temperature rises.

Some very interesting results have recently been obtained with tellurium by Cartwright and Haberfeld-Schwartz (*Roy. Soc. Proc.*, **148** A, 648, 1935). This substance differs from ordinary metals in that it contains a very small number of conduction electrons, in fact, it is an *intrinsic* semi-conductor with only about one free electron to one million atoms, so that the effect of impurities is here relatively enormous. Hence, Cartwright and Haberfeld-Schwartz added small quantities of ordinary metals to very pure tellurium and examined their influence on the electrical conductivity and thermoelectric power. As would be expected, they found that the conductivity was increased, although the increase was not proportional to the conductivity of the added metal. The temperature coefficient of resistance of the pure tellurium was negative, but became less so on addition of copper, and became positive when more than 0·3 per cent. of antimony was added. The thermoelectric current flowed from tellurium to copper at the cold junction, and generally increased with the addition of another metal.

These results are discussed in the light of Fowler's *normal extrinsic* and *abnormal extrinsic* models. It is found that the upper unoccupied band in tellurium is about 0·6 electron volts above the lower filled band. The top of the intermediate band resulting from the addition of antimony lies about 0·11 electron volts above the lower tellurium band and produces *abnormal extrinsic* properties. The band resulting from the addition of copper lies about half-

way between the two tellurium bands. It would appear that both *normal* and *abnormal extrinsic* properties usually result from the addition of most impurities; the effects appear to assist each other in respect to the electrical conductivity, the abnormal predominating, but to oppose each other in respect to thermoelectric power.

In discussing further properties of semi-conductors those of a semi-conductor/metal rectifier will now be described. It is always assumed that in the system metal/semi-conductor/metal the two contacts are quite different—one forming an appreciable gap across which current has to flow, and the other being a very intimate contact. If the Schrödinger equation is applied to the transmission of electrons across the gap, in the case of a *normal extrinsic* semi-conductor the solution contains two terms, one representing an electron wave proceeding towards the gap and the other representing an electron wave reflected back, the relative intensities of the two expressing the properties of the rectifier. It can be shown that if the potential of the metal is raised by a small amount V, the current transmitted across the gap will be given by

$$\alpha \, . \, e^{-\beta V} . \{e^{eV/kT} - 1\},$$

where α is a constant depending on the absolute temperature and the magnitude of the contact resistance under zero applied voltage, and β is a constant which determines how the transmission depends on V. Similarly, when the potential of the metal is lowered by an amount V, the current in the reverse direction will be

$$\alpha \, . \, e^{\beta V} . \{1 - e^{-eV/kT}\}.$$

Treating the problem as one-dimensional, Frenkel and Joffé deduced that β should be 1·5.

Now, the modern Sperrschicht cell consists of a metal plate coated with a layer of semi-conductor which in turn is covered with a transparent metal film. Thus Barnard (*Proc. Phys. Soc.*, **47**, 477, 1935) used a mixture of selenium and sulphur as semi-conductor—many interesting practical details will be found in his paper—the upper surface being covered with a transparent layer of sputtered metal, the lower being in contact with a steel plate. The metal portions form the two electrodes of the cell and can be directly connected to the terminals of a current-measuring instrument, to record a deflection when light falls upon the transparent electrode. When light of energy F passes through the semi-conductor a number of electrons directly proportional to F is emitted. These cross the gap and enter the steel plate, causing the contact

difference of potential to rise by some amount v, whence arises a photocurrent proportional to F, given by

$$i_p = \alpha \,.\, e^{-\beta v}\{e^{ev/kT} - 1\}$$

which, when the cell is on open circuit, must be balanced by an equal reverse current i_r.

On closed circuit, however, these currents will not be equal and a current i_e will flow through the external circuit, given by

$$i_e = \text{const.}\cdot\{\text{B}\,.\,\text{F} - \text{small correction terms}\},$$

where B is a constant. This equation holds for the majority of the cells made by Barnard. Since from the above it is clear that F is proportional to i_p and therefore depends on β, the value of β can be obtained from the experimental data. Barnard finds that the value of β can be about 10 times the calculated value for the cells used by him. In fact, he finds β depends upon the ratio of area illuminated to area covered by the collecting plate electrode, and that the Frenkel and Joffé value is obtained when extrapolation to zero illumination area is made.

This is not the place to survey the wide range of experimental data. Gudden's survey to which reference is made above gives a very clear picture of the main features and the more recent papers by Fritsch and by Auwers (*Zeit. für Phys.*, **93**, 90, 1934) give an idea of the present trend of the work on metallic oxides.

THE NATURE OF A POLISHED SURFACE.—The subject of the polishing of metals is one of considerable interest both to the academic worker and the technician, but it is only within recent years that it can be said that the problem has been approached in a scientific manner. The scientific study really starts with the observations on polished surfaces made by Sir George Beilby and described in his book, *Aggregation and Flow of Solids*, 1921. His method was to note the consecutive changes observed through the microscope when a surface was polished, and the changes in reverse order when the polish was removed by etching the surface. These observations led him to conclude that polishing caused the surface to flow, so that in its final state the surface was covered by a glassy or amorphous layer; in other words, he pictured the polished surface as a supercooled liquid, and the surface layer is now often termed the Beilby layer. Attempts have, of course, been made to present other conceptions of the polished surface; some workers regarded the amorphous state as an unnecessary assumption, and others, while accepting the amorphous state, did not accept the view that the surface was liquified during polishing.

The recognition of the importance of the electron diffraction method of examination caused the whole question of films on metal surfaces to be re-opened. The first worker to use this new method was French (*Proc. Roy. Soc.*, A **140**, 637, 1933) who investigated the surfaces of polycrystalline and single crystal specimens. A fine beam of high speed electrons was allowed to fall upon the surface at a small glancing angle and diffraction systems were formed upon a photographic plate set perpendicular to the direction of the incident beam, the arrangement forming an electron diffraction camera. In general, for an unpolished surface the diffraction system is a characteristic ring pattern (or nearly a half ring, as recorded on the plate). French showed that these rings became blurred as polishing proceeded, until finally two broad diffraction maxima alone remained. If a single crystal was used the diffraction system was originally a series of regularly spaced spots, but after prolonged polishing two diffuse rings were obtained.

French's results were extended by Darbyshire and Dixit (*Phil. Mag.*, **16**, 961, 1933), who used also specimens of iron pyrites and galena, and by Raether (*Zeit. für Phys.*, **86**, 82, 1933). The latter investigated in a very thorough manner the changes produced in the surfaces of metals and insulators, such as NaCl, $CaCO_3$, FeS_2, etc., by etching, rubbing, polishing, compressing and hammering. He also used a wide range of polishing materials. Finch, Quarrell and Roebuck (*Proc. Roy. Soc.*, A **145**, 676, 1934) condensed a thin layer of a second metal upon a polished surface and took electron diffraction photographs during the deposition. They found that the structure of the condensed layer gradually vanished, until only general background scattering was obtained, provided that the polish was adequate. They likened the disappearance of the pattern to the solution of crystals supernatant on a solvent, and considered it excellent support for the Beilby layer.

French pointed out that his results could be interpreted on the view that when a metal surface is ground and polished the crystal size is gradually reduced until there finally results a random arrangement of atoms, similar to that of the atoms in a liquid, which are separated by a common distance. This common distance he was able to compute, and found good agreement with X-ray data. He suggested that in an alternative picture of the polished surface it might be assumed that any crystal structure in this surface was very small. This suggestion was seized upon by some workers, but Darbyshire and Dixit concluded that it was not in accord with their experimental results. Raether came to the same conclusion, although his results in the cases of ionic crystals, *i.e.* crystals such as NaCl in which

the atoms are oppositely charged, showed that here amorphous layers do not exist. The details given above, then, overwhelmingly support the conclusion that the Beilby layer is real; the evidence in its support and the discussion of alternative views were more extensively reported by G. P. Thomson (*Phil. Mag.*, **18**, 140, 1934).

Two important questions now remain to be considered. First, what is the thickness of the Beilby layer, and, second, what is the state of the metal immediately below it? The first was answered by H. G. Hopkins and by C. S. Lees in papers read before the Faraday Society last March. Hopkins worked with gold, as a polycrystalline metal whose surface would not readily oxidise. He polished the surface and measured the thickness of the layer that had to be removed before the polish disappeared. This was not done by etching, but by a method suggested by L. C. Martin. The polished metal was made a portion of the cathode in a sputtering apparatus, and its surface was gradually sputtered away under controlled conditions which permitted a steady state of removal. The surface, 1 sq. cm. in area, was initially ground by light rubbing on 00 and 000 emery paper, using benzene as a lubricant, final polishing being done with 0000 emery paper. The procedure was that previously used by French; the surface was first rubbed in one direction until it showed a series of fine scratches all parallel to the direction of rubbing. The surface was then turned through 90° and rubbed across the scratches until they disappeared and were replaced by a new set running parallel to the new direction of rubbing, the surface then being washed to remove traces of emery, polished by rubbing lightly in all directions with chamois leather, and finally washed in alcohol.

The surface was now examined with an electron diffraction camera, using a glancing angle of about 1°. It then exhibited the two characteristic rings described by French and others. The polish was next removed step by step by sputtering from the surface for definite intervals of time, the amount of sputtering being found by microbalance weighings. The surface was periodically examined in the electron diffraction camera. The main results may be summarised as follows, on the understanding that the depths quoted refer to average values obtained from measurements made with many specimens. After the removal of a layer only 10 Å thick, two extra rings appeared in the diffraction pattern, and with further removal of surface material to a depth of 20 to 30 Å the pattern remained constant, though the bands became more intense. At a depth of 40 Å, however, the band corresponding to

the (331) (420) (422) and (333) planes of gold split up into two sharper bands at the mean positions of the (331) (420) and (422) (333) spacings respectively. At the same time the outer of the first two rings disappeared, to be replaced by two separate rings at the (220) and (311) (222) positions. Further, an additional faint ring occurred outside the (531) (600) (422) maxima. At depths greater than 50 Å the pattern gradually became sharper, and at about 150 Å the (111) (200) rings were resolved. At 400 to 500 Å the normal sharp pattern for gold was obtained.

Hopkins explained these facts on the assumption that when only 10 Å thickness of material has been removed, a fraction of the incident electrons is scattered in a substratum of very small gold crystals; *i.e.* the Beilby layer is assumed to be 20 to 50 Å deep. The fine crystals of the substratum increase in size with depth, accounting for the observed progressive sharpening of the pattern and the gradual disappearance of general or background scattering as more of the surface is removed.

Lees polished his copper and gold surfaces by grinding with successive grades of emery with water and using either (*a*) water and rouge, or (*b*) rouged flannel, or (*c*) flannel treated with a commercial "liquid polish" for final polishing. The polished surface was carefully freed from grease by washing it with redistilled acetone and drying it in an air blast. Successive thicknesses of 30 to 6000 Å were removed from the surface by electrolysis, the surface being periodically examined by electron diffraction. The unetched surface gave no diffraction rings or only two blurred ones, as recorded by other workers. (After etching by electrolysis, however, the rings were those of cuprous oxide and not of copper.) The thickness of the amorphous layer varied with the method of polishing. Thus method (*a*) gave with copper a thickness of about 45 Å, method (*b*) about 27 Å, while method (*c*) always produced an incomplete amorphous layer.

When Lees removed the amorphous layer, a layer of crystals orientated with their 110 directions within about 20° of the normal to the surface was found, a feature which was found to be unconnected with the direction of polishing. The thickness of the orientated layer was about 150 Å for surfaces polished by method (*a*) above, and about 500 Å for method (*b*), and the crystals in this layer were not much smaller than those in the non-orientated crystals in the metal below. In the case of gold the orientated layer extended to a much greater depth than in copper. Such orientation effects are important, and, incidentally, it may be recorded that Jenkins (*Phil. Mag.*, **17**, 457, 1934) reported that

orientation could be produced by polishing a thin layer of graphite powder.

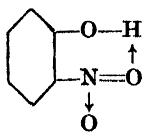
These orientations were presumably due to the deformation of the microcrystals, and, as the direction of polishing had no influence, it would appear that the deformation is a pure compression in a direction perpendicular to the surface. Now, slip in a compressed crystal of copper or gold is known to cause the 110 direction to approach the direction of compression. In the case of copper the observed orientation would be explained by the action of a pressure of 10,000 lb. per sq. in., which would mean that the area of metal in actual contact with the polishing agent must be very small and that very little friction must exist at the regions where the pressure is exerted. This explanation, if correct, would mean that different degrees of orientation should be obtained with different degrees of compression, and it might happen that above a certain pressure the direction of polish would influence the orientation.

In conclusion, we turn again to Raether's results with polished surfaces of insulators. Here, no amorphous or Beilby layer is produced, but it appears that under emery treatment very tiny crystals are torn from the surface to cover it with a fine dust. The electron diffraction patterns then obtained consequently depend upon the fineness of the dust. For example, graphite and pyrites give rather coarse crystallites, which to some extent produce spots or cross-grating spectra and to some extent produce rings, depending on whether their distribution is random or not. Sodium chloride, however, gives a typical powder, or ring, diagram. We see, then, to what a great extent the Beilby layer explains the nature of polished metal surfaces, and how firmly it rests upon direct experimental evidence, while it would appear that much useful information still remains to be garnered by the electron diffraction camera.

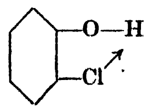
GENERAL AND ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

The Co-valency of Hydrogen.—The possibility of hydrogen having a co-valency of two has for some time been a point at issue between some chemists and physicists. On purely chemical grounds the evidence in favour is very strong. Sidgwick and his co-workers in a series of papers extending from 1915 have shown that certain *ortho*-substituted benzene derivatives have abnormal physical properties compared with the *meta* and *para* isomerides. Sidgwick and Callow (*Jour. Chem. Soc.*, 1924, 527) summarised these results and

drew attention to the fact that the observed anomalies could be accounted for by the co-ordination of a hydrogen atom in one substituting group with another atom in a second substituent in the *ortho* position with the formation of a stable five- or six-membered ring; for example, the abnormal volatility and solubilities of *o*-nitrophenol (I) and *o*-chlorophenol (II) were explained by ring formation as shown.



(I)



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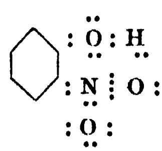


(III)

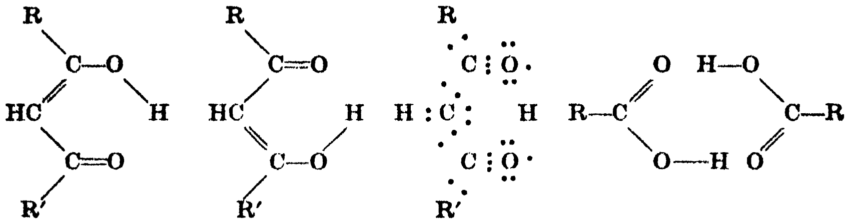
The properties of chelate compounds such as the β-diketones have been similarly explained. The subject is well reviewed by Sidgwick in the *Annual Reports of the Chemical Society* for 1933.

Now the original suggestion was that the electronic structure of such a co-ordinated hydrogen atom was that it had four shared electrons, *i.e.* *o*-nitrophenol was represented as (III). On the physical side, however, this solution was untenable. An atom cannot have more than two electrons in the first quantum group, so that if hydrogen in such compounds as the above has four electrons associated with it, two must be in the second quantum group, but the energy with which two-quantum electrons would be held is too small to account for the stability of the co-ordinated hydrogen. In a further review, Sidgwick (*Chem. Soc. Annual Reports*, 1934, 40) invokes the theory of resonance to surmount the difficulty. There is evidence that if two electronic structures are possible for the same molecule, the wave-mechanical function for the normal state of the molecule is the resultant of the two, that is, either the molecule is passing from one to the other with enormous rapidity or more probably has a structure which cannot be represented by the usual structural formulæ.

Applying this idea to co-ordinated hydrogen compounds the phenomena may be divided into two classes: (*a*) those in which resonance can occur without ionisation and the co-ordinated structure, as ordinarily written, is cyclic, and (*b*) those in which ionisation is involved.

An example of the first class is the enolic form of the β-diketone where neither (IV) nor (V) represents the true condition of the molecule which should be formulated as a combination of the two in

resonance, the consequent extra energy content accounting for the stability.

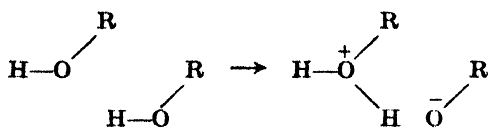


(IV) (V) (VI) (VII)

Sidgwick does not adopt the " singlet " link, but if this possibility be accepted the compound could be formulated electronically as (VI) which would represent the intermediate form between (IV) and (V).

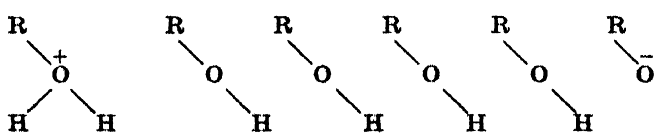
Ring formation may involve two molecules as, for example, the carboxylic acids (VII) which are known to polymerise to give double molecules.

The second class comprises such simple hydroxylic compounds as water and the alcohols which are associated into large molecules. Here it is suggested that polymerisation occurs through oxonium ion formation (VIII), the simple molecules being held in the positions



(VIII)

required by the energy of the resonance. This process can be extended through a series of molecules, the hydrogen passing from one to the other along the chain, giving as the ionised form (IX).



(IX)

The theory explains many of the properties of water and other hydroxyl compounds. As long as the resonance persists, the hydroxonium and hydroxyl ions are not free, but they may be released when thermal agitation breaks up the chain, hence the low conductivity of water and the increase in the conductivity with rise of temperature.

Organic Compounds containing Deuterium.—The practicable methods available for the preparation of organic compounds containing all the protium replaced by deuterium are not very many if allowance be made for the comparative costliness of the material. Urey and Price (*Jour. Chem. Physics*, 1934, **2**, 300) have prepared tetradeuteromethane and Murray, Squire and Andrews (*ibid.*, 714) deuteroacetylene by the action of deuterium oxide on metallic carbides. The latter compound offers opportunities for further syntheses, but an even more important reaction from this point of view has now been carried out by Wilson (*Jour. Chem. Soc.*, 1935, 492) who has acted upon carbon sub-oxide with deuterium oxide of not less than 99·5 per cent. purity, $C_3O_2 + 2D_2O \rightarrow CD_2(CO_2D)_2$. The dideuteromalonic deuteracid so obtained, in almost quantitative yield, melted at 128–130° with decomposition, definitely lower than malonic acid prepared for comparison under the same conditions (134–135° decomp.). On heating to 150° the compound gave a quantitative yield of trideuteracetic deuteracid

$$CD_2(CO_2D)_2 \rightarrow CD_3CO_2D + CO_2.$$

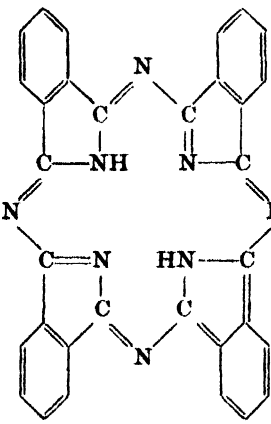
This compound melted at 15·8° compared with 16·6° for acetic acid prepared similarly.

By a direct comparison of the vapour pressures of acetic acid and trideuteracetic deuteracid over a range of temperatures from 20° to 100°, the latter has been found to have the higher vapour pressure, thus providing another example, in addition to deuterium fluoride, DF, and acetic deuteracid, CH_3CO_2D, of Lewis and Schutz's (*Jour. Amer. Chem. Soc.*, 1934, **56**, 493) third class of substance where association occurs in the vapour phase where they predicted that the vapour pressure would be increased by replacing hydroxylic protium by deuterium owing to the existence of co-ordinated protium or deuterium bonds and the greater strength of the deuterium over the protium bond.

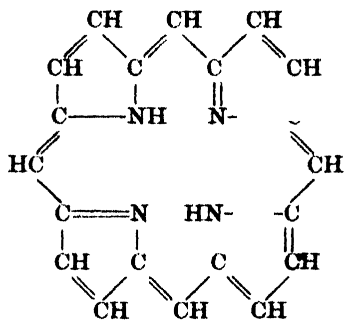
Phthalocyanines.—A new and interesting class of deeply coloured organic compounds has been described by Linstead and his collaborators (*Jour. Chem. Soc.*, 1934, 1016). The parent compound, to which the name phthalocyanine has been given, has been assigned the structure (X) which is reminiscent of the fundamental porphin ring system (XI) present in the porphyrins.

The first derivative, in the form of an iron salt, was discovered by chance in the course of the manufacture of phthalimide by passing ammonia into molten phthalic anhydride in iron vessels when sometimes traces of a dark blue substance was formed. It has now been found that *o*-cyanobenzamide or *o*-phthalonitrile react with metals

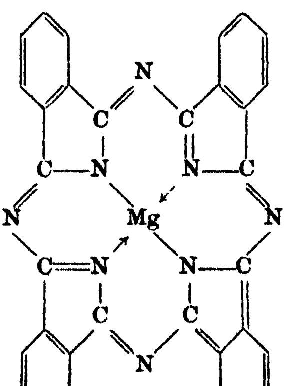
to give metallic phthalocyanines; for example, *o*-cyanobenzamide heated with magnesium for 15 minutes at 230–240°, the melt powdered, extracted with dilute sodium hydroxide, dilute sulphuric acid and hot water successively and then repeatedly with boiling alcohol gave a bright blue solid, which, crystallised from quinoline, gave magnesium phthalocyanine as blue needles with a purple lustre.



(X)



(XI)



(XII)

This compound when stirred into concentrated sulphuric acid and the solution slowly run on to ice gave a blue precipitate of free phthalocyanine which crystallised from quinoline as a bright, rather greenish-blue solid with a strong purple lustre. The co-ordinated structure (XII) is suggested for the metallic compounds. Phthalocyanine and its metallic salts are remarkably stable, they are not readily decomposed by acids or alkalis and the copper

compound can be sublimed at ordinary pressures in an atmosphere of nitrogen or carbon dioxide or under reduced pressure at 550–580°.

THE TETRAZOTISATION OF ARYL DIAMINES.—The application of the many well-known synthetic methods involving diazonium compounds has been restricted in the case of aromatic diamines containing both amino groups in the same ring by the difficulty of preparing the tetrazonium compounds, particularly in the cases of *m*-diamines owing to the formation of compounds analogous to Bismarck Brown and *o*-diamines owing to the formation of aziminobenzenes. The method of tetrazotisation described by Hodgson and Walker (*Jour. Chem. Soc.*, 1935, 530) is, therefore, of considerable importance. They add a solution of the diamine in glacial acetic acid to a well-stirred solution of nitrosyl-sulphuric acid prepared by dissolving sodium nitrite in concentrated sulphuric acid. Pouring into ice-water gives a solution of the tetrazonium compound or into a solution of cuprous chloride in hydrochloric acid, a 70 per cent. yield of *o*-, *m*- or *p*-dichloro-benzene from *o*-, *m*- or *p*-phenylene diamines respectively.

PHYSICAL CHEMISTRY. By H. W. MELVILLE, Ph.D., Colloid Science Laboratory, Cambridge.

UNIMOLECULAR REACTIONS AND RELATED PHENOMENA.—The history of the development of our knowledge about unimolecular reactions has been a chequered one. Controversy has been acute and even to-day there is by no means unanimous opinion on the interpretation of the experimental results. The definition of such a reaction is of course rigid—it is simply that the rate of reaction is proportional to the first power of the concentration of the reacting substance. In this review consideration will only be given to those gaseous reactions which are believed to proceed homogeneously, for it is in this field that material progress has been made in gaining a general picture of the nature of unimolecular reactions.

When it was shown in bimolecular reactions that a sufficient criterion of reaction was that molecules of sufficiently high energy content should collide, such molecules being present owing to the Maxwell distribution of energy among degrees of freedom, no further cause for reaction required to be sought. As, however, unimolecular reactions possessed a high temperature coefficient, which implied that only activated molecules took part, the question arose as to the origin of the energy of activation, for if a reaction is truly unimolecular it would appear that collisional processes cannot be responsible for activation. This led to the suggestion that isothermal radiation supplied the necessary energy (the radi-

ation hypothesis), but the theory was not countenanced for long, firstly because there was insufficient energy present to activate molecules quickly enough and secondly irradiation of the gas with light of a frequency corresponding to that required by the energy of activation failed to accelerate the rate of decomposition. At this time—about 1925—the only well-established example of a unimolecular reaction was the thermal decomposition of nitrogen pentoxide, so it could hardly be said that an adequate test of the hypothesis was possible. Somewhat earlier two attempts had been made to explain the occurrence of unimolecular reactions in spite of the assumption that activation really involved two molecules. Lindemann suggested that after a collision in which a molecule became activated it did not immediately react, as in a bimolecular reaction, but that there was a time lag between activation and decomposition during which the great majority of the molecules were deactivated. In other words, the lifetime of the activated molecule was long compared with the interval between collisions. At low enough pressures these times may become comparable with the consequence that the reaction will tend towards a bimolecular course. The other attempt was made by Christiansen and Kramers, who suggested that the products of reaction, containing not only the original energy of activation but also the exothermic heat of reaction, could selectively activate fresh reactants, thus setting up what may be termed a thermal chain reaction. Here, too, theory predicted a unimolecular reaction at high pressures and a bimolecular one at low pressures. The specificity in the interchange of thermal energy on this theory was, however, a drawback as it was then unsupported by any direct experimental evidence.

The need for further experimental work became urgent and was soon satisfied when Hinshelwood examined the decomposition of relatively complicated molecules such as aldehydes, ethers, etc., and found, as predicted from Lindemann's theory, that these reactions conformed to a unimolecular mode of decomposition at pressures of several hundred millimetres of mercury but became bimolecular as the pressure was lowered. Once the fact was established that polyatomic molecules tend to decompose unimolecularly examples accumulated rapidly. Another fundamental point arose from this work. At low pressures, in the bimolecular region, the rate of reaction ought to be equal to the number of collisions $\times\, e^{-E/RT}$, E being the energy of activation. It turned out, however, that the rate is often many times greater than that expected, for example, in the decomposition of dimethyl ether into methane, carbon monoxide and hydrogen at 800° K. and 400 mm.,

the rate is 5×10^3 times too large. To surmount this difficulty it was suggested that the energy residing in bonds other than that which was broken could contribute to the energy of activation by the transmission of this energy from one part of the molecule to another. Since unimolecular decompositions are characteristic of complicated molecules the probability of a molecule possessing the requisite energy of activation was increased considerably. Mathematical analysis showed that the factor was $(E/RT)^{(s/2-1)}/(s/2-1)!$ where s is the total number of square terms, there being, for example, two square terms associated with the kinetic and potential energy of a single bond, the vibration being along the line joining the nuclei of the atoms concerned. That energy can in fact be transmitted through several bonds in a molecule has been strikingly demonstrated by Norrish (*J. Chem. Soc.*, p. 874, 1934) who found that in the photochemical dissociation of methyl butyl ketone the quantum (90 kg. cal.) is absorbed at the chromophoric keto group but the fission occurs along the hydrocarbon chain to yield propylene and acetone, the energy required to break the bond being probably about 70 kg. cal.

From the pressure at which these reactions become bimolecular it is possible to measure approximately the mean life of the activated state, which is of the order of 10^{-8} sec. The time for the transmission of energy cannot be greater than this; no information is available about the minimum value. If the explanations are valid some relationship ought to be apparent between the number of square terms and the position of the compound in a homologous series. In the azo decompositions a rough parallelism is evident, the number of square terms increasing with complexity

Molecule.	No. of Square Terms.
$CH_3{-}N{=}N{-}CH_3$	25
$CH_3{-}N{=}N{-}C(H)(CH_3)_2$	33
$(CH_3)_2(H)C{-}N{=}N{-}C(H)(CH_3)_2$	40

The absolute number of terms, especially in azomethane, is considerable and for the reactions cyclopropane $\rightarrow$ propylene and the decomposition of ethyl azide—25 and 28 respectively—so large as to throw doubt on the interpretation of the data as unimolecular reactions.

Complications must be expected when complex polyatomic molecules are used for kinetic investigation. During the develop-

ment of the above-mentioned ideas, somewhat elaborate theoretical investigations have been made, which are hardly justified by the relatively crude data available owing to experimentally uncontrollable side reactions, heterogeneous secondary reactions and the like. None the less, Hinshelwood has recently put forward an extension of the theory which permits a more refined analysis of the kinetics without introducing any important modification of the original hypothesis and defines more precisely the modes by which a molecule may reach the activated state and subsequently decompose.

The thermal decomposition of nitrous oxide at pressures of the order of several hundred millimetres is bimolecular and furthermore the absolute rate of reaction may be approximately calculated from the observed energy of activation by considering the energy to be supplied by two square terms. Upon examining the reaction more carefully (Volmer, *Z. physikal. Chem.*, several papers 1930–4; Hunter, *Proc. Roy. Soc.*, A, **144**, 386 (1934)) it is found that if the reciprocal time for 50 per cent. decomposition is plotted against the initial pressure a curve exhibiting a segmented appearance and concave to the pressure axis is obtained. Had the reaction been strictly bimolecular the result of such a plot would have been a straight line passing through the origin.

The first segment starts from the origin and stops at 50 mm., the second extends from 50 to 4000 mm. and the third from 4000 to about 30000 mm. A pseudo-unimolecular reaction would give a smooth curve starting from the origin (the bimolecular part), but bending round to become parallel to the pressure axis (the unimolecular part). A segmented curve would be most simply explained by supposing that, in the nitrous oxide reaction, there are three separate pseudo-unimolecular reactions each leading to primary dissociation into a nitrogen molecule and an oxygen atom and only differing in the mode of activation and in the mean life of the activated state. For example an analysis of the segmented curve at different temperatures reveals that a high reactivity, that is, a short life, is associated with a high energy of activation—a relationship to be anticipated, but as will be shown later not of universal validity. Activation of nitrous oxide molecules may also be brought about by collisions with chemically inert gases. The following series with coefficients relative to nitrous oxide as unity was obtained by Volmer: H_2O (1·62), CO_2 (1·31), He (0·65), Ne (0·44), A (0·28), Kr (0·24), N_2 (0·24), O_2 (0·23). The striking feature about this series is the specificity of the activation coefficients. Since these molecules activate nitrous oxide they must, by the principle

of microscopic reversibility, remove the energy from the complex formed by the collision of an oxygen atom with a nitrogen molecule to yield stable nitrous oxide. A similar specificity is found in the gas phase combination of bromine atoms in three body collisions and in the deactivation of chlorine molecules acoustically excited to the first vibrational level. It is therefore not true to say that the removal of energy from what may be loosely termed a "hot molecule" is equally probable for all gases. The energy is of course degraded ultimately, but the important point in all kinetic processes is the competition for that energy, and if it so happens that the reactants deactivate more readily than other molecules in the system, such as products, there is no inherent difficulty in adopting, at least in part, reaction schemes of the kind proposed by Christiansen and Kramers some considerable time ago.

The fact that the segmented curve for nitrous oxide is made up of three parts indicates there are three modes of activation, one would be the excitation of the —N—N— bond, the second that of the —N—O— bond, the third is doubtful and might be ascribed to excitation of the deformation vibration —N—O↑. On examining a homologous series of molecules a better idea of the modes of activation may be gained. The aliphatic aldehydes which decompose to carbon monoxide and a residue have been studied from this point of view (Hinshelwood, *Proc. Roy. Soc.*, A, **146**, 327, 1934). The simplest molecule—formaldehyde—decomposes strictly according to a bimolecular mechanism, that is, reaction on collision, two square terms sufficing to account for the absolute rate. On the other hand, the decomposition of acetaldehyde is a pseudo-unimolecular reaction with three modes of excitation. At high pressures —above 300 mm.—the velocity of reaction is adequately accounted for by two square terms and upon analogy with formaldehyde the predominant mode of activation is apparently the excitation of the —C—C— bond or the migration of the aldehydic hydrogen atom or both. At lower pressures a greater number of square terms is required to obtain concordance in the calculated and observed rates, thus pointing to the activation of bonds in the methyl group. It is noteworthy that although the energy of activation for acetaldehyde is some 6 kg. cal. greater than that for formaldehyde yet the larger number of square terms involved in the former reaction more than compensates for this, the half life being shorter than that of formaldehyde. In contrast with nitrous oxide the energy of activation increases with decreasing pressure, that is the shorter lived molecules have the smaller energy of activation. Similar behaviour obtains with propionic aldehyde except that the greater

number of degrees of freedom makes the mean lives shorter still, in spite of increased energy of activation. While it is stretching quantitative analogy far, it is of interest to note that chloral behaves in a similar way, though rates comparable with those for aldehydes are obtained at 100° lower on the temperature scale. The remarkable specific effect of hydrogen in activating acetaldehyde molecules extends indiscriminately to all modes of activation. For a given proportion of hydrogen the segments become steeper and are displaced to lower aldehyde pressures. The limiting rates attained by the constituent pseudo-unimolecular reactions are, in accordance with theory, exactly the same.

The evidence for the time lag between activation and dissociation rests primarily on physical theories rather than on concrete chemical evidence, which latter indeed is difficult to accumulate. Metallic alkyl compounds, *e.g.* germanium tetraethyl, lead tetraethyl, decompose according to a unimolecular mechanism. It is now well established by Paneth's experiments (*Ber.*, **62**, 1335; **64**, 2702; **64**, 2708) that free radicals are among the primary products of decomposition and it is not unlikely that the primary thermal dissociation of simple hydrocarbons yields methyl and ethyl radicals. Attempts (Paneth, *J. Chem Soc.*, p. 380, 1935) to prepare propyl, butyl and phenyl radicals by pyrolysis of the lead compounds have not met with success for apparently these are dissociated very easily to fragments of smaller molecular weight at the relatively high temperatures required to decompose the lead derivatives at a measurable rate. Free benzyl radicals on the other hand from tetrabenzyl tin have a much longer life approximating to that of methyl. The radical may also be obtained by the interdiffusion of benzyl chloride and sodium vapour following Polanyi's technique of highly dilute flames.

It has been mentioned above that the number of square terms required to obtain the necessary velocity of activation is so large, in certain instances, that there may not be a sufficient number of degrees of freedom in the molecule to accommodate them. This would suggest immediately the existence of some type of chain mechanism formally similar to that postulated by Christiansen and Kramers. Two papers appeared recently which show indeed that a chain mechanism may play a part in the decomposition of acetaldehyde. Azomethane (CH_3—N=N—CH_3) decomposes thermally to nitrogen and ethane at a temperature much lower than that at which aldehyde shows any sign of a measurable reaction. Furthermore the fragments of the dissociated molecule can remove lead mirrors and therefore there is every reason to believe that the

molecule breaks up with the production of methyl radicals in the first instance (Leermakers, *J. Amer. Chem. Soc.*, **55**, 3499, 1933). Allen and Sickman (*ibid.*, **56**, 2031, 1934) consequently added small amounts of azomethane to acetaldehyde and found induced decomposition of the aldehyde. For example at 313° C. and 111 mm. pressure with one per cent. of azomethane the ratio of rates (*i.e.* chain length) of decomposition of aldehyde to azomethane was 91. On increasing the percentage to 10, the chain length diminished to 20, the chain length actually being proportional inversely to the square root of the azomethane concentration. This means the chains terminate in the gas phase by the mutual destruction of the carriers, the sequence of reactions probably being

$$(CH_3)_2N_2 \longrightarrow N_2 + 2CH_3 \text{ (Starting, energy of activation 51·2 kg. cal.)}$$
$$CH_3 + CH_3CHO \longrightarrow CH_4 + CH_3CO \text{ (Propagation 13·4 kg. cal.)}$$
$$CH_3CO \longrightarrow CH_3 + CO$$
$$CH_3 + CH_3 \longrightarrow C_2H_6 \quad \text{(Termination 0 kg. cal.)}$$

The rate at which the chain length *decreases* with increasing temperature is proportional to $e^{+13000/RT}$, for half the energy of activation for starting exceeds that for propagation by 13 kg. cal.

In a similar way the photolysis of acetaldehyde at temperatures above 100° C. goes by way of chains. In this reaction there is some doubt about the primary products of dissociation, methane and carbon monoxide being the principal end-products at room temperatures. Had the methyl radical been liberated as such, some considerable quantity of ethane and of hydrogen should have accumulated. In support of this contention Pearson (*J. Chem. Soc.*, p. 1718, 1934) showed that unlike ketones, aldehydes on photolysis at room temperature did not yield radicals capable of removing metallic mirrors. To explain Leermakers' results it must be supposed that the methyl radical has a very transitory existence, undetectable by the usual methods, but at higher temperatures it does have an opportunity of reacting with an acetaldehyde molecule thereby initiating a chain. The photo reaction may be regarded as consisting essentially of two distinct parts. The first is concerned with the primary photo dissociation, and the second with the effect of the products inducing a chain reaction. If the temperature is about 100° C. the latter becomes comparable in rate with the former, so that chain propagation is only observed above this temperature. The kinetic expression for the total rate of reaction (equation 1) therefore contains two terms, the first of

$$-\frac{d[CH_3CH:O]}{dt} = 0{\cdot}3I_{abs.} + k\sqrt{I_{abs.}}[CH_3CH:O] \quad . \quad . \quad . \quad . \quad (1)$$

($I_{abs.}$ is the number of quanta absorbed per unit time.)

which represents the primary decomposition not involving a chain. Here the quantum yield is not unity since approximately 70 per cent. of the absorbed light at pressures of the order of 100 mm. is re-emitted as fluorescence. The second term pertains to the chain part of the reaction. In accordance with the findings in the thermal sensitised reaction the chain length is proportional to the square root of the rate of starting. The temperature coefficient for the chain length yields an energy of activation of 10 kg. cal. in fairly good agreement with the value deduced from Allen and Sickman's data. The absolute value of the chain length depends of course on the intensity of the light, but it may be mentioned that quantum yields of the order of 10^2 were obtained at 300° C. and 100 mm. pressure again in good accordance with the thermal experiments.

These experiments demonstrate conclusively the possibility of the propagation of reaction chains in the dissociation of molecules which at higher temperatures undergo pseudo-unimolecular decomposition. This latter proviso is important, because it may well happen that the chain length at these higher temperatures has become so short as to be of no importance in augmenting the rate of decomposition calculated from the measured energy of activation and two square terms. Only further experiment can decide whether a chain mechanism may be invoked to reduce the number of square terms required to account for the observed rates of reaction.

A series of interesting experiments carried out by Kistiakowsky (*J. Amer. Chem. Soc.*, **54**, 2208, 1932 ; **56**, 638, 1934) throw light on another aspect of reactions of organic molecules, the molecular statistics of which give rise to some speculation. These are the cis-trans transformations of molecules possessing a double bond, which appear to proceed homogeneously and fortunately, in a few cases, are not accompanied by appreciable decomposition of the molecule as a whole. Gaseous dimethyl maleate is readily transformed to the fumaric ester, the reaction being of the pseudo-unimolecular type and possessing the comparatively low energy of activation of 26·5 kg. cal. The diethyl ester isomerises at about the same speed. The surprising point emerges that the rate of activation is much smaller than that calculated by assuming only two square terms to supply the requisite energy, the discrepancy amounting to a factor of 10^5. Even the limiting velocity constant at high pressures is 10^5 times smaller than that usually observed with unimolecular processes. The low rate of activation might be ascribed to the fact that the molecule must be oriented in a particular way in order that efficient transfer of energy may occur, but the

low value of the limiting unimolecular constant would seem to imply that the transmission of energy is especially ineffective. It is of interest to mention that the energy required to twist the CH_2 group through 180° has been calculated from a knowledge of the electronic structure of the ethylene molecule ; it amounts to 28 kg. cal. (Penney, *Proc. Phys. Soc.*, **46**, 1934). To what extent this may be modified by the substitution of two $COOCH_3$ groups is too difficult to calculate. Isostilbene (cis diphenyl ethylene) likewise isomerises to stilbene upon heating, the energy of activation now however rising to 42·8 kg. cal. and no less than 24 square terms are necessary to account for the observed rate of reaction. While it would be premature to generalise upon these results it must be emphasised that a rather different type of excitation is involved. In normal decomposition reactions it is almost certain that activation consists in the stretching of the bond to be broken (how far deformation vibrations need be considered is not yet known) whereas in these ethylene isomerisations a twisting frequency is excited. Transmission of energy from the remoter parts of a molecule to break a single bond or to twist a double one may possess widely different probabilities. It is to be hoped that further examples amenable to experimental study will be discovered to throw more light on the nature of the transfer of energy in these reactions.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., The University, Glasgow.

Igneous Activity and Igneous Rocks—Extra-European.—Some dike rocks from Mount Johnson, one of the Monteregian Hills, have been described by F. F. Osborne and N. L. Wilson (*Journ. Geol.*, XLII, 1934, 180–7). Two radial joint planes traversing the plutonic core of the mountain are occupied respectively by a tinguaite rich in magnetite and ilmenite, and another tinguaitic type rich in alkali-amphibole. Dikes of camptonite occupying joints have occasionally been chilled to glass.

The alkaline stock of Pleasant Hill, Maine, described by W. F. Jenks (*Amer. Journ. Sci.*, XXVIII, 1934, 321–40), consists mainly of nordmarkite with subordinate syenites. It belongs to the New England–Quebec (Monteregian) petrographic province. The intrusion may have been emplaced by a modified ring-fracture mechanism, or by piecemeal stoping accompanied by abyssal assimilation.

J. E. Maynard has re-examined a large collection of quartz-bearing plutonic rocks from the state of Vermont (*Journ. Geol.*, XLII, 1934, 146–62) in the light of Johannsen's classification, and has worked out their systematic position and nomenclature.

Granites, granite-gneisses and related intrusive rocks underlie much of western Connecticut, occurring as dikes, sills and larger massifs. With their associated pegmatites they abundantly inject and impregnate the older formations. W. M. Agar (*Amer. Journ. Sci.*, XXVII, 1934, 354–73) describes those intrusions which can be recognised as separate units and discusses their distribution and relative importance in penetrating and altering the older schists and gneisses.

Dr. C. S. Hitchen describes the interesting pegmatites of Fitchburg, Maine (*Amer. Mineralogist*, **20**, 1935, 1–24), which fall into five groups, namely, biotite, tourmaline, beryl, titanite, and allanite-bearing types. These types appear to have been injected in the given sequence. A progressive increase in soda content has been noted in the series, leading to the replacement of microcline by albite in the later members of the succession.

Hypersthene-granodiorite occurs for a distance of 260 miles as an occasionally interrupted belt on the north-west side of the Catoctin–Mountain Blue Ridge anticlinorium of Virginia. It is fully described by Dr. A. I. Jonas (*Bull. Geol. Soc. Amer.*, **46**, 1935, 47–60), and its associations with granite and unakite elucidated.

The Snowbank Stock of Minnesota, of which R. Balk and Professor F. F. Grout have made a structural study by the Cloos method (*Bull. Geol. Soc. Amer.*, **45**, 1934, 621–36), consists of successive intrusions of syenite and granite. It is shown that the mass rose as an elliptically-shaped body which crowded aside its walls so that the adjacent schists were deformed for about a mile from the contacts.

The same authors have also studied the great Boulder Batholith of Montana by Cloos's methods (*Bull. Geol. Soc. Amer.*, **45**, 1934, 877–96). They have come to the conclusion that this igneous mass is a typical transgressive batholith which has moved in a general upward direction, and has a wide direct connection with magma in great depths.

A full description of Mount Shasta, the great Cascade Mountains volcano, has been given by Professor Howel Williams (*Zeitschr. f. Vulk*, XV, 1934, 225–53). He discusses the volume and form, growth, structure, age and petrography of the cone.

The same author continues his studies of the Cascade volcanoes with a description of Newbery volcano in central Oregon (*Bull. Geol. Soc. Amer.*, **46**, 1935, 253–304). The lavas are rhyolites, andesites and basalts. The history of the volcano is highly complicated with the formation of domes and parasitic vents. Its magmatic history supports Kennedy's view that olivine-rich basaltic magma is intimately connected with the centralisation of igneous activity.

In a controversial paper contributed by R. E. Fuller and M. G. Hoffman (*Journ. Geol.*, XLII, 1934, 311–28), the authors discuss some structural features in the Columbia lavas of central Washington. Hoffman criticises Fuller's view that ellipsoidal lavas and beds of granulated glassy breccias, etc., were formed by advance into shallow water basins along the margins of the lava fields, and maintains that air cooling would be sufficient to produce the disputed structures.

J. B. Stone and E. Ingerson have written a valuable account of the little-known volcanoes of southern Chili (*Amer. Journ. Sci.*, XXVIII, 1934, 269–87). The lavas are described as mainly olivine-bearing basalts, but from the given descriptions and from two recorded silica percentages it is possible that the rocks would be more correctly described as basic pyroxene andesites.

Dr. W. F. Hume presents a third series of rocks analyses from Egypt (*Geol. Mag.*, LXXII, 1935, 8–32). The analyses, which comprise gneisses, schists, serpentines, and various igneous rocks, have been carried out by H. F. Harwood and L. S. Theobald.

One of the outstanding petrological memoirs of the year is that by the veteran Professor A. Lacroix on the volcanism and lithology of Tibesti in the eastern Sahara (*Mem. Acad. Sci. Inst. de France*, **61**, 1934, 169–371). Tibesti consists largely of enormous volcanic massifs which are probably of Tertiary age. The rocks are rhyolites, trachytes and phonolites, with a basaltic facies comprising dacitoids, andesites, basalts and basanitoids. The ancient basement rocks of the country are penetrated by a set of dikes including nepheline-syenite, theralite, tinguaite, etc. New analyses to the number of 106 are published, and Professor Lacroix gives instructive comparisons of Tibestian petrology with that of regions in north and east Africa.

The "Younger Intrusive Rocks of the Kudaru Hills, Nigeria," described by A. D. N. Bain (*Quart. Journ. Geol. Soc.*, XC, 1934, 201–39), are chiefly notable for the occurrence of acid igneous rocks containing fayalite and riebeckite, and for the fact that most of the minor intrusions form a series of cone-sheets. This is so far the only known occurrence of cone-sheets outside the Tertiary province of the West of Scotland and North of Ireland.

The petrography and tectonics of the Pretoria–Johannesburg Granite, an inlier of the Basement Granite of the Transvaal, is described by J. Willemse (*Trans. Geol. Soc. S. Afr.*, XXXVI, 1934, 1–28). The locus of the intrusion seems to be intraformational. A considerable amount of cataclastic deformation has occurred, and the encircling sediments are domed.

B. V. Lombaard describes the felsites of the Bushveld Complex

with several new analyses (*Trans. Geol. Soc. S. Afr.*, XXXV, 1933, 125–90). An occurrence at the Premier Mine appears to be a composite sill consisting of quartz-gabbro, intermediate types and felsites, grading into one another. This is explained by a theory of mixing between magmatic fractions generated at different periods. The Regenstein vent in the Auas Mountains south of Windhoek, South-west Africa, contains a number of interesting alkaline igneous rocks, which have been described by T. W. Gevers (*Trans. Geol. Soc. S. Afr.*, XXXVI, 1934, 77–88). These include phonolites with analcitised varieties and analcitite, containing inclusions of shonkinite and alkaline peridotite. Trachytes and limburgites also occur within this region.

Dr. Gevers also describes the Kaoko Eruptives and Alkali-Rocks at Cape Cross, South-west Africa (*Trans. Geol. Soc. S. Afr.*, XXXV, 1933, 85–96). There is here a series of basalts of Stormberg age, which are intruded by late differentiates consisting of gabbro, granophyre and porphyrite. A younger series of alkali rocks—soda-syenite, bostonite and tinguaite—are of Post-Karroo age.

Four Deccan Traps from Linga, Chhindwara District, Central Provinces, India, have been petrographically described by Dr. L. L. Fermor (*Rec. Geol. Surv. India*, LXVIII, Pt. 3, 1934, 344–60) with four new analyses by Raoult. While olivine-dolerites and olivine-poor basalts are represented, the chemical analyses are remarkably similar. Comparison with Washington's analyses of Deccan Traps shows that there is a progressive change in composition from the Lower to the Upper series of Traps, leading to increasing alkalies and alumina.

The Pre-Cambrian " Gwalior Traps " from Gwalior, India, are described by M. P. Bajpai (*Journ. Geol.*, XLIII, 1935, 61–75) as quartz-dolerites occurring mainly as sills. Six analyses are given. The richness of these rocks in alkalies, especially soda, the absence of quartz, the invariable presence of olivine and in four cases nepheline, in their norms, are totally at variance with their description as quartz-dolerite.

The igneous rocks of Indo-China, described in a fine memoir by Professor A. Lacroix (*Bull. Serv. Geol. de l'Indochine*, XX, Fasc. 3, 1933, 1–208), belong to two main age-groups, Pre-Tertiary and Tertiary. The former comprises a granite-diorite-gabbro and a syenite-monzonite-nepheline syenite series with corresponding lavas excepting phonolites. The Tertiary series is composed of basalts, trachybasalts, basanitoids and limburgites. No fewer than 141 new chemical analyses are published, and comparisons are instituted with Malaysia, the East Indies, and Eastern China.

L. F. Yih and T. Y. Yü describe "The Igneous Geology of the Nanking Hills" (*Mem. Nat. Res. Inst. Geol., Nanking*, Ser. B, Vol. 1, 83 pp. qto, 18 plates and maps). Dacite and rhyolite lavas erupted at the close of the Lower Cretaceous, were followed by granodiorite intrusions penetrating Silurian to Cretaceous strata with strong contact metamorphism. These were followed by gabbro-diorite and aplite intrusions probably in the early Tertiary. A very widespread Quaternary basalt lava flow occurs on both sides of the river Yangtse. The petrography and petrology of all these igneous rocks are very fully treated.

T. Tomita describes olivine-trachybasalt from Ching-Hsing, North China (*Journ. Shanghai Sci. Inst.*, Sect. II, Vol. 1, 1933, 1–10). This flow, in common with all other analysed Kainozoic basalts from China and neighbouring countries, contains normative nepheline. An anomalous feldspar of high temperature formation, potash-andesine, which elsewhere has been called anemousite, occurs in these lavas.

The so-called "leucite-basalt" from Ryûdô, Korea, is shown by the same author to be a quite typical analcite-basalt (*Journ. Shanghai Sci. Inst.*, Sect. II, Vol. 1, 1933, 25–39). Two new analyses support this view, and render the figure, K_2O, 10·04, given in an earlier analysis, "incomprehensible" (*sic*).

T. Nemoto describes two kinds of alkaline rhyolites from Hokkaido, Japan (*Journ. Fac. Sci. Hokkaido Imper. Univ.*, Ser. IV, **2**, No. 4, 1934, 299–321), occurring in the midst of that dominantly calcic igneous province. One is an ægirine-augite hyalo-rhyolite (or *okawaite*), the other a comendite, of which chemical analyses are given.

S. Kôzu describes in great detail the great outburst of the volcano Komangataké in 1929 (*Tschermak. Min. u. Petr. Mitt.*, **45**, 1934, 133–74). Amongst other topics he discusses an excellent series of temperature measurements of the ejected pumice for three years after the eruption, and deduces therefrom its rate of cooling. Petrological and chemical studies of the pumice and of its interesting cordierite-rich inclusions were made.

N. Krijanovsky has given a descriptive list of the 127 known volcanoes of Kamtchatka, 19 of which are active or in a solfataric condition (*Bull. Geol. Soc. Amer.*, **45**, 1934, 529–50). This work is valuable as much of the literature is in Russian.

Dr. A. B. Edwards describes the Tertiary dikes and volcanic necks of South Gippsland, Victoria (*Proc. Roy. Soc. Vict.*, XLVII, Pt. 1, 1934, 112–32). The dikes, which form a N.W. swarm, are of Lower Oligocene age and are intruded into Jurassic sediments.

The suite comprises trachyandesites, olivine-analcite-dolerites and -basalts, monchiquites and olivine-nephelinites. These rocks are considered to have been derived from an initial olivine-analcite-basalt magma by gravity-differentiation under "agpaitic" conditions (Backlund). The analcitisation, however, is regarded as an independent process.

Miss G. A. Joplin has described an interesting case of reaction between diorite and limestone at Ben Bullen, New South Wales (*Geol. Mag.*, LXXII, 1935, 97–116). Diorite injections in limestone have given rise to a series of contaminated rocks which contain definite mineral assemblages. These are sharply defined and occur with a rough concentric arrangement about the igneous injections. The concentration is regarded as the most important physical factor in contamination. The process seems to have taken place at low temperature and in the presence of abundant volatiles of which water was the most important.

The problem of the origin of the Brisbane Tuff formation has been attacked anew by Professor H. C. Richards and W. H. Bryan (*Proc. Roy. Soc. Queensland*, XLV, No. 11, 1934, 50–62). The lower stratified portion of the formation presents no unusual features; but the upper or Massive Tuff is explained as due to an enormous eruption of the incandescent avalanche type, having much in common with similar occurrences in New Zealand and Alaska.

Dr. G. J. Williams discusses the mechanism of injection illustrated by a granite—schist contact in Stewart Island, New Zealand (*Quart. Journ. Geol. Soc.*, XC, 1934, 322–53). An interesting petrological conclusion is that the formation of myrmekite is due to gaseous emanations passing through the consolidated granite, a reconstitution which may be regarded as a mild form of greisenisation.

PEDOLOGY. By Professor N. M. Comber, D.Sc., A.R.C.S., F.I.C., The University, Leeds.

It is fitting to refer here to the services to soil science of Sven Odén of Sweden, and of Gintaro Daikuhara of Japan, who have died during 1934. Odén did pioneer work in the study of soil colloids and made notable contributions to our knowledge of humus. Daikuhara's work was mainly associated with problems of soil acidity and lime requirement, and he played important parts in the major developments of that branch of soil science.

The Mineralogy and Chemistry of Clay.—This, the main subject of this section of "Recent Advances" two years ago, has been the subject of a lot of work in the last year or two and it was intended chiefly to devote this space to an account of that work.

Conceptions are changing so rapidly however, and major developments seem so imminent, that no more will be done here than to refer to the prominence of this topic and to express the belief that a full and interesting statement will be possible in the near future.

Soil Water and Soil Structure.—Among the many tacit assumptions of the early soil scientists was that of the capillary movement of water in soil, replacing, in the effort to re-establish equilibrium, water removed by plants and by evaporation. The recent shattering of that view is doubtless well known, and the view that roots go in search of water has largely replaced the teaching that the water creeps to the roots.

The movement of water to drains which also had a capillary explanation is now known to be essentially a gravitational movement through natural cracks or through cracks and fissures induced by the operations of laying tiles and through the coulter cut of mole drains. In this connection recent observations and work by H. H. Nicholson of Cambridge are interesting—particularly a study of changes in the structure of mole drains (*J. Agric. Sci.*, **24**, 185). The disturbance of the surrounding soil during the operation of mole draining—once tacitly assumed to be inimical—appears to be helpful to the flow of water to the drain although *per contra* the incidence of a breaking of the wall of the drain tends to silting up.

There are two converse spheres of study in connection with problems of water movement and soil structure, the crevices through which the water moves (the importance of which has been vividly realised in the recent practice of studying soil profiles and taking monoliths) and the structure of the aggregates whose surfaces are the sides of the crevices.

The study of the large aggregates has long been prominent in the Russian work on soil genetics, and some rather elaborate attempts have been made to classify different types and shapes of aggregates and to correlate them with the genetic soil types. The importance of this study of structure is to receive emphasis apparently at the International Soil Congress to be held in this country this year. It may be contended with some reason that this line of study yields something which the long standing study of the prime soil particles—the familiar mechanical analysis—can never give, and mechanical analysis is therefore being given a new perspective in the conspectus of Pedology.

E. W. Russell (*J. Agric. Sci.*, **24**, 315; *Phil. Trans.*, **233** A, 361) has published some papers describing studies of the imbibition of water and of non-polar liquids. The most interesting issue of this work is a new hypothesis of crumb formation, namely, that by bridges

of dipolar water molecules, cations bind the negative charges of clay particles. There is a correlation between the exchangeable ion content of a clay and the contraction in volume of a polar liquid. Moreover, the strongest crumbs were formed in clays of small particles containing sufficient small exchangeable ions and dried from a polar medium the molecules of which are small.

In spite of the increased recognition from various points of view of the vertical and horizontal heterogeneity of soil, the practice of taking a mixed sample for certain analytical purposes still persists. An interesting paper by Sokolov (*Z. Pfl. Düng*, **34** A, 129) deals with the importance of more fully recognising soil heterogeneity. It is shown for instance that the influence of ammonium sulphate on the availability of phosphate is seen when the two are applied together more than when applied separately. The total amount of each in a given volume of soil, and even the uniform distribution of each, are inadequate considerations. It is the places where they are in contact that have to be considered.

SOIL GENETICS.—The examination and description of soil profiles continue to increase and to make effective, if sometimes slow, contributions to our knowledge of the processes of soil genetics. A paper by Lundblad (*Soil Sci.*, **37**, 157) on some Swedish soils is significant in making a contribution to the clarification of our views about podsols and brown forest soils. It has been held by some that brown forest soils are podsols in the making, while others have regarded them as a distinct type. Lundblad has examined several Swedish podsols and brown forest soils by an oxalate extraction method due to Tamm.[1] The results give some weight to the view that brown forest soils and podsols are distinct. For instance, the variations with depth of oxalate-soluble iron in the brown forest soils show no correspondence with the figures from the podsols or any evidence that those soils are potential podsols.

NITROGEN AND ORGANIC MATTER.—From the Bureau of Chemistry and Soils U.S.A. Department of Agriculture (Anderson and Byers, *Soil Sci.*, **38**, 121), there has recently been published a paper on the C/N ratio of soils in relation to soil classification. There is confirmation here of other recent observations that the ratio is very much more variable than was at one time supposed. The C/N ratios of Chernozens, Prairie soils, Podsols and Laterites are considered. In each group there is *generally* a decrease in the C/N ratio with depth but the variation is often irregular. The ratio

[1] The oxalate solution is said to extract the migratory iron, aluminium and silica associated with the soil colloids, although Lundblad brings evidence to show that the ageing of the colloids reduces solubility in oxalate solution.

varies among the soils of any one group—the Chernozens are the most consistent—but there is some evidence of an essential difference in the organic matter of the various groups.

The Research Station at Jealotts Hill and the Rubber Research Institute of Malaya have produced two papers on soil microbiology particularly in reference to tropical conditions (Corbet, *Soil Sci.*, **37**, 109 ; **38**, 407). It is found that under the conditions of temperature and humidity that prevail in Malay the number of soil micro-organisms remains fairly constant at about 500,000 per gram—a low figure compared with those of temperate soils. The soil temperature in the forest is constant at about 24–25° C. It is raised when the forest is cleared and then there is a fall in the nitrogen and organic matter, but no increase in soil population. The usual theory that the loss in organic matter follows an increase in bacterial activity with the higher temperature appears therefore to be under grave suspicion.

Corbet also shows that under laboratory conditions the evolution of carbon dioxide from a soil follows the equation (in a modified form) of Lemmermann and Wiessmann, namely

$$y = Ft^m$$

Where y is the total yield of carbon dioxide after a time t and m is a constant expressing retardation in gas evolution under the experimental conditions and F is the yield of carbon dioxide in unit time at the beginning of the experiment. F therefore represents the microbiological activity of the soil under constant temperature conditions and is, therefore, of practical significance in tropical soils. It is further found that while the evolution of carbon dioxide is proportional to the bacterial numbers while those numbers are increasing, during the phase of decrease only a portion of the micro-organisms participate in carbon dioxide production.

Soil Phosphorus.—The phosphorus compounds of soil have received considerable attention in the last year or two. Dean (*Soil Sci.*, **37**, 253) has studied the removal of phosphorus by electro-dialysis. From this work it seems that for the time being the utilitarian purpose of determining the availability of soil phosphorus is still likely to be best served by acid extraction, but electrodialysis will probably be very useful in studying the more fundamental problem of the place of the phosphate ion in the soil—a place which is probably more essentially related to the constitution of the soil than has hitherto been thought. Electrodialysis may however be useful in determining availability in calcareous soils, as shown by McGeorge (*Soil Sci.*, **38**, 347).

Other papers (Heck, *Soil Sci.*, **37**, 243 ; **38**, 463) show that the

fixation of phosphorus is intimately related to the degree of base saturation of the soil.

Spencer and Stewart (*Soil Sci.*, **38**, 65) have concerned themselves with the differentiation between the unavailability of phosphorus on account of its form of chemical combination and that due to its fixation in the upper horizons with consequent deficiency in lower parts of the root range. They consider that phosphorus in certain organic combinations can sink to greater depths and escape the fixation of phosphorus in inorganic compounds.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

Geographical Distribution.—Several contributions to our knowledge of plant distribution in Denmark have recently been published. Knud Jessen (*Bot. Tidskr.*, **42** H. 2, 1935) deals with the Liliales and maps the distribution of 32 species. Amongst the more interesting species, it may be noted that *Maianthemum bifolium* and *Convallaria majalis* are of frequent occurrence throughout the entire area : the oceanic *Narthecium ossifragum* has a western distribution in Denmark : *Polygonatum verticillatum* has a discontinuous occurrence mainly in eastern Jutland which, together with its extension to the extreme north of the Scandinavian Peninsula and its absence from south-east Sweden and Finland, is held to indicate that it immigrated from the south along two routes corresponding to those of the berry-eating migratory birds.

Polygonatum officinale, which is a marked calcicole in Britain, is absent or rare from several of the most calcareous parts of eastern Jutland and the islands.

The species of *Gagea* exhibit a series of increasing restriction to the eastern parts. *Gagea lutea* occurs sparsely in west Jutland ; *G. minima* and *G. pratensis* only occur sparsely in eastern Jutland and Funen, mainly in Zealand, whilst *G. arvensis* is confined to the south-eastern islands.

The southern species are conspicuously coastal in their distribution, which is attributed to the more favourable conditions of the milder and longer autumns for the ripening of their fruits.

One cannot but be struck by the fact that of the thirty-two species mapped only one *Polygonatum officinale* appears to show any tendency to be restricted to the drier sands and gravels of middle Jutland.

The distribution of the Danish Rhodophyceæ is described by L. K. Rosenvinge (*Mec. Acad. Roy. d. Sci. Denmark*, **6**, No. 2, 1935, 1–45). Six species although only known from these waters, *viz.*

three species of *Rhodochorton*, two of *Melobesia* and one of *Ceramium*, are regarded as unlikely to prove endemic. The number of species diminishes greatly as one passes from the Kattegat to Baltic waters, a diminution which is attributable mainly to the decreasing salinity. Near the island of Bornholm there are only twenty-six species, of which half are cold-boreal types, about a quarter sub-arctic and less than a sixth warm-boreal. In the Kattegat area the total number of species is about six times larger (158 app.) of which more than half are warm-boreal types (53 per cent.), about one-third cold-boreal and about a tenth sub-arctic.

The third contribution by Aage Lund (*ibid.*, **6**, No. 1, 1–98) gives an account of the freshwater Phycomycetes with particular reference to the reaction of the water. Species of *Aplanes*, of *Saprolegnia*, *Pythiomorpha undulata* and *Sapromyces Reinsepii* are typical of highly acid waters, whilst species of *Monoblepharis* of *Blastocladia*, *Pythiomorpha gonapodyides*, *Achlya radiosa* and *Macrochytium botrydioides* exhibit a marked preference for alkaline waters. Nevertheless the results of experimental cultures suggests that the *p*H of the medium is not the master factor.

B. Lindquist has studied the distribution of the types of *Pinus sylvesties* in Sweden (*Norrlands Skogsvardsforbunds Tids*, **1**, 1935, 1–86), and finds that these fall into three categories. A type with a broad crown about six metres wide predominates in the south and this is further distinguished by having needles with a modal length of about 4 cm. and air sacs on the pollen grains of about 40μ in length. A narrow-crowned type predominates in the north and is closely related to the narrow-crowned type of the south and a broader-crowned type which occurs in the north; all these have needles with a modal length of about 3 to 3·5 cm. and air sacs from 36 to 40μ. The determination of the precise relationship of *Var. scotica* to these Swedish types is a matter of some interest.

Reproduction.—Kostoff records species crosses of *Nicotiana* with 72 chromosomes which were pronounced dwarfs despite the high chromosome number. The species crossed were *N. glauca* and *N. rustica* and the interesting point was observed that whereas the cross using the variety *humilis* of *N. rustica* resulted in embryo formation, these embryos die during the early stages of development. When *N. rustica* v. *texana* was employed the embryos developed normally (*Current Science*, 1935, 356).

According to Kansik (*ibid.*, p. 357) the embryo sac of *Utricularia cœrulea* when it has reached the quadrinucleate stage has grown to such an extent that the egg apparatus is constituted as an extrusion outside the micropyle, the externally situated egg forms

a tubular outgrowth after fertilisation which passes down the micropyle into the endosperm, the embryo being differentiated at the tip.

The phenomenon of polyembryony is a familiar one in the Rutaceous genera *Citrus* and *Xanthoxylon*. In the Grape Fruit one embryo is the product of a fertilised egg, the other is nucellar in origin. In Xanthoxylon all the embryos are nucellar. In *Murraya Koenigi* (R. S. Chakiavarthy, *Current Science*, 1935, 361) there is fertilisation as in Citrus, but numerous nucellar embryos may develop although only two or three attain the cotyledonary stage.

In 1907 eighty strains of Turkey red wheat were selected and grown at the Nebraska Agricultural Station. The marked differences between their yields when first separated have in fourteen or fifteen years reverted to that of the original variety. The data furnished indicate a gradual diminution in the divergence from the original stock, which divergence by 1922 had become negligible (H. F. Smith, *Jour. Council Sci. and Indust. Res.*, **8**, 1935, 37). An interesting case of viviparous development of seedlings is reported by M. Ghouseuddia and M. Sayeeduddin (*Current Science*, III, 1935, 434). The plant concerned was the Cucurbitaceous *Coccinia indica* and the embryos had emerged through the pericarp. The chief feature however was the apparent transformation of one cotyledon into a lobed leaf and the other into a tendril. If this interpretation should be substantiated it would afford strong support for the view that the tendrils of this family are modified leaves.

Economic.—The economic seaweeds of the Kwangtung province of southern China is the subject of a paper of C. K. Tseng (*Lingnan Sci. Jour.*, **14**, 1935, 93). The most important are *Digenea simplex*, used as an anthelmintic drug and of which the annual crop is valued at about £665,000 : *Eucheuma gelatinæ*, used as a mountant and for the manufacture of agar-agar, which yields a crop valued at £266,000 : *Gloiopeltis furcata*, used as a sizing material in the silk industry : *Porphyra*, used as a food, and *Sargassum*, used as a drug, as food and for making a beverage. Eleven other kinds of minor importance are also cited.

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—A recent paper by R. J. Tillyard (*Ann. Ent. Soc. America*, **28**, 1935, 1–45) deals with evolution of the scorpion-flies (Order Mecoptera) and their derivatives. Among his conclusions are the following: the Mecoptera originated in the

Upper Carboniferous times from an unknown ancestor shared with the Neuroptera. In the early Permian the Mecoptera were divided into the two sub-orders, the Protomecoptera and the Eumecoptera. The former was never a successful group and to-day there remain but three isolated genera. The Eumecoptera on the other hand were dominated in Permian times by the Permochoristidæ. This family by the Upper Permian had penetrated as far as Australia and was probably world-wide. The Diptera arose in the Southern Hemisphere in Upper Permian times from Paratrichopterous ancestors; the Trichoptera, first found in the Lower Lias of the Northern Hemisphere, were descended from specialised Permochoristidæ; the Lepidoptera are closely allied to the Trichoptera and must have been derived from a common ancestor with them in the Trias; and the Siphonoptera are not descended from the Diptera as is usually believed nor from the Trichoptera (in this Tillyard disagrees with Crampton), but must be traced back either to some highly reduced and specialised type of Permochoristidæ or to an allied form within the Paratrichoptera. Their evolution is bound up with that of Mammals and Birds so demanding an ancestral type with a terrestrial larva. Tillyard summarises his conclusions in the form of a phylogenetic table.

In insects with a complete metamorphosis usually most of the larval tissues disrupt and the imago regenerates from scattered masses of dormant embryonic tissue (imaginal discs). But this process is not always the same; for example in the beetle *Galerucella* Poyarkoff showed that the epidermis becomes directly converted into that of the imago after its cells have undergone a process of rejuvenation. Also usually the imaginal tissues arise from undifferentiated parts of those larval organs which they will eventually replace. But Mansour demonstrated that in *Calandra* the imaginal mid-gut arises not from local replacing cells as in other insects but from the fore-gut of the larva. Further, extensive phagocytosis takes place in Muscids but in some other insects it apparently does not even occur. Florence V. Murray and O. W. Tiegs (*Q.J.M.S.*, **77**, 1935, 405–95) have described in detail the metamorphosis of *Calandra oryzæ*. Several interesting observations have been made. The larval epidermis and its derivatives are directly converted into those of the imago, the larval cells extruding some of their chromatin and cytoplasm and multiplying considerably. This same phenomenon is seen in the dormant imaginal cells (*e.g.* the myoblasts), thus throwing doubt on the hypothesis of "rejuvenation." Some tissues (*e.g.* muscles, Malpighian tubes) regenerate from local dormant cells, but the mid-gut is almost entirely regenerated from the fore-gut,

the local imaginal "replacing" cells taking only a small part in its formation. In the insect studied phagocytosis is only of minor importance in the removal of larval tissues. The last point of major interest is that some larval elements may not disappear till after the imago emerges.

In an earlier paper that has already been noted (SCIENCE PROGRESS, No. 113, 1934, 128–9) V. B. Wigglesworth studied the histological changes in the epidermis during moulting and metamorphosis in *Rhodnius prolixus*. More recently (*Q.J.M.S.*, **77**, 1934, 191–222) the same author has analysed some of the factors which regulate these processes. He finds that in moulting there is a "critical period" before which if the head of the insect is removed moulting does not take place. This period corresponds with the time when mitotic divisions in the epidermis begin. After this period the blood contains a factor or hormone which will induce moulting even in insects decapitated soon after feeding. Wigglesworth suggests that this hormone may be secreted by the corpus allatum as the cells of this gland show signs of the greatest secretory activity during the critical period. Stretching the abdominal wall by a large meal provides the stimulus which causes secretion of the moulting hormone. The stimulus is conveyed by nerves to the brain. The fact that insects, sharing the same blood, moult simultaneously shows that the whole process of growth is coordinated by chemical means; the factors concerned apparently being produced by the growing cells themselves.

With regard to the causation of metamorphosis the author finds, by various experiments with decapitated nymphs, that the absence of metamorphosis in normal nymphs before the fifth stage is due to an inhibiting factor or hormone in the blood. This inhibitory hormone is normally produced in small quantities and the head of the insect is necessary for its secretion. It appears after the moulting hormone. These results confirm the suggestion of Bodenstein (*Arch. Entw. Mech.*, **128**, 1933, 564–83) that the hormones associated with pupation differ from those which cause larval moulting and also that the larval epidermis is capable of metamorphosis at a comparatively early stage. At the conclusion of this paper Wigglesworth suggests that diapause in other insects (when it is not brought about by the direct effect of the environment) may result from the temporary failure of growth-promoting hormones due sometimes perhaps to an inborn rhythm, sometimes perhaps to the indirect effect of environmental factors (see O. Theodor under Diptera).

The conclusions of this paper fit in with Goldschmidt's hypothesis

that abnormalities, such as prothetely (premature formation of pupal characters) and metathetely or neoteny (pupæ with their metamorphosis imperfect), are due to an upset in the proper timing of developmental velocities. Wigglesworth has shown that the secretion of the moulting hormone precedes secretion of the hormone inhibiting metamorphosis; and if the early nymphal stages are decapitated before this inhibiting hormone is fully formed, they develop adult characters in some degree (*i.e.* prothetely). On the other hand, if final stage nymphs receive the blood of earlier nymphs containing this inhibiting hormone they develop into adults showing imperfect metamorphosis (*i.e.* metathetely).

In this connection it is interesting to note in passing that R. H. Nagel (*Ann. Ent. Soc. America*, **27**, 1934, 425–8) has obtained cases of metathetely in larvæ of *Tribolium confusum* by submitting them to low temperatures, especially −6° C. for one to six hours. It should be mentioned that in larvæ younger than those of the last instar no striking abnormalities were observed, those that were found being confined to the legs and thoracic segments.

There are three ways in which an insect might lose body water apart from loss via the alimentary canal, namely (1) through the general surface of the body wall, (2) through the spiracular system or (3) through both. K. Mellanby (*Proc. Roy. Soc.*, *B*, **116**, 1934, 138–49) has devised an apparatus which will measure accurately the amount of water evaporated from an insect to one-hundredth of a milligramme. From experiments carried out using this apparatus it appears that practically all the water evaporated from an insect is lost by way of the tracheal system and that a thin integument may be just as water-tight as one which is highly "sclerotised."

The importance of serious studies on weed-insect parasites is emphasised by B. B. Pepper and B. F. Driggers (*Ann. Ent. Soc. America*, **27**, 1934, 593–8). Non-economic insects may well be intermediate hosts of parasites of the more important economic insect pests. The significance of this in biological control work is obvious. In this paper weed-insects are shown to be important in the case of the parasites of the oriental fruit moth.

A paper by T. H. C. Taylor with three sections by R. W. Paine (*Bull. Ent. Res.*, **26**, 1935, 1–102) deals fully with the campaign against *Aspidiotus destructor* Sign. in Fiji. Attempts at biological control were made with parasites from Tahiti in 1920 and from Java in 1927. These were unsuccessful, but the introduction of predators in 1928 and 1929 from Trinidad was completely successful and these have controlled the scale insect ever since. The

most valuable predator was *Cryptognatha nodiceps*, a Coccinellid beetle.

In studying the influence of climatic conditions it is useful to have a record of the duration of moonlight. A new instrument for this purpose has been made by C. B. Williams and G. A. Emery (*J. Sci. Instr.*, **12**, 1935, 111–15). It consists of a cylindrical lens mounted on a light-tight drum which rotates at a speed of one revolution in 24 hours and 50 minutes, which is the average time of the moon's apparent rotation round the earth. The axis of the drum is set pointing to the pole star and by means of a timing disc the drum is set each afternoon so that the lens follows the position of the moon. Inside the rotating drum there is a fixed drum on the outer surface of which is a strip of photographic bromide paper. On this the line image of the moon, produced by the cylindrical lens, is focussed. The darkening of the bromide paper gives an indication both of the duration and of the intensity of the moonlight.

COLEOPTERA.—The whole question of the so-called symbiotic relationship between Coleopterous insects and intracellular micro-organisms is exceedingly interesting. According to P. Buchner (*Holznahrung und Symbiose*, Berlin, Springer, 1928, and *Tier und Pflanze in Symbiose*, 2. Aufl., Berlin, Borntræger, 1930) these organisms play a vital rôle in the digestive processes of wood-eating insects and thus ensure an ample supply of carbohydrates for their host through their assumed ability to break down cellulose. This view, however, has been criticised by K. Mansour (*Q.J.M.S.*, **73**, 1930, 421–36) and W. Ripper (*Z. vergl. Physiol.*, **13**, 1930, 314–33) who claim that in certain cases the intracellular micro-organisms are of no importance for the digestive processes. Recently K. Mansour and J. J. Mansour-Bek (*Biol. Rev.*, **9**, 1934, 363–82) reviewed this whole subject. More recently, however, K. Mansour (*Q.J.M.S.*, **77**, 1934, 255–72) has marshalled together the available evidence against the hypothesis of symbiosis between wood-eating insects and intracellular micro-organisms. He comes to the conclusion that the micro-organisms should more correctly be referred to as commensals or parasites causing no conceivable harm to their host. As Ripper (1930) pointed out, the analogy between this association of micro-organisms and wood-eating insects and gall-forming insects and their host plants is attractive. The method of transmission of these micro-organisms from one generation to the next always takes place by elaborate methods. In *Calandra oryzæ*, for example, there is a definite infection centre (Mansour, 1930). A. Koch (*Z. Morph. Oekol. Tiere*, **23**, 1931, 389–424) showed that the micro-organisms penetrate the wall of the oviduct and

invade the advanced ovum. In a recent study of some Bostrychid beetles (*Q.J.M.S.*, **77**, 1934, 243–54), K. Mansour has shown that the micro-organisms from the paired mycetomes invade the testis lobes, multiply and mix with the sperms. Then they pass with the sperm during copulation into the bursa copulatrix of the female. From this region they pass through the micropyle of the fully formed eggs during their passage to the outside. The mycetomes throughout life remain quite isolated from the alimentary tract.

The latest account by J. C. F. Fryer (*J. Min. Agric.*, **41**, 1935, 1058–62) of the recent outbreak of the Colorado beetle at Tilbury (see SCIENCE PROGRESS, No. 113, 1934, 131) states that at present there is no place in the country that is now known definitely to be infested. However, it is emphasised that the counties, Essex and Kent, in which the beetles have been found must be regarded as under suspicion for at least another twelve months.

LEPIDOPTERA.—The importance of immunological reactions for the determination of relationships of various bacteria and indeed some plants such as hybrid wheats has been demonstrated in recent years. Now S. Martin and F. B. Cotner (*Ann. Ent. Soc. America*, **27**, 1934, 372–83) have studied the serological specificity of the proteins of certain moths. They have found from the reactions of certain proteins that the precipitin reaction is useful in determining phylogenetic relationships between genera and sub-families of the Phalænidæ.

In a paper by D. L. Collins (*J. Exp. Zool.*, **69**, 1934, 165–85) on iris-pigment migration and its relation to behaviour in the codling moth, it is shown that the position of the pigment granules varies according to the intensity of the light to which the moth is subjected. Under natural conditions light-adaptation began about half an hour before sunrise and was complete half an hour after sunrise regardless of the season of the year. Dark-adaptation began in March about an hour before sunset and was completed at, or soon after, sunset. In June, however, this adaptation to darkness began 20–30 minutes before sunset and was complete 30–50 minutes after sunset. No artificial light used in the experiment was strong enough to cause light-adaptation to proceed at a faster rate than occurred in nature. Further, it was found that the codling moth is positively phototropic to a marked degree only when dark-adapted. Perhaps the most interesting result of this investigation is to know that all the vital activities of the moths—mating, oviposition, feeding, migration—are carried on almost exclusively during periods of changing light intensity, *i.e.* when the iris-pigment movements are taking place. The iris-pigment migra-

tions are an important factor in determining the behaviour of the moths because the nature of the moth's reactions to constant or changing light varies according to the position of the iris-pigment.

HEMIPTERA.—W. Maldwyn Davies (*Ann. Appl. Biol.*, **22**, 1935, 106–15) has shown that migrating alatæ of aphides infesting the potato crop in North Wales are more numerous in the Flintshire districts than in the south Caernarvon area. The mean monthly values for relative humidity are consistently lower in the former district. Controlled experiments showed that high humidities inhibit the flight of these insects. This influence of humidity upon flight is considered to explain the fact that in North Wales infestations of aphides are always greater following a period of east winds, because from an examination of the meteorological data it is seen that the humidity is definitely lower on days when east winds are blowing compared with that during the prevailing south-west winds.

HYMENOPTERA.—In a paper on the developmental stages of *Bracon tachardiæ* Com. (*Bull. Ent. Res.*, **25**, 1934, 521–39), P. M. Glover has shown that the length of mandible is for practical purposes identical in a larva of any given instar and in its exuviæ. The range of a given instar is wide but the extremes do not overlap. The lengths calculated on Dyar's head-width principle approximate closely with the observed lengths, sufficiently to preclude overlooking an ecdysis. This makes possible the identification of the instar of fixed and mounted specimens. The factors of increase of head-width and mandibular length are fairly similar and can be substituted for each other. This fact if of general application may be useful in the study of ecto-parasites. Preliminary investigations have shown that similar observations apply to *Aphrastobracon flavipennis*. The growth of the body of the larva from instar to instar was found to be independent of the growth of the head capsule. The larvæ increased in weight and volume by about 3·6 at each moult, while the head-size increased by the usual double.

DIPTERA.—The terms used at present in the systematic study of Diptera are defined and illustrated in a paper by P. H. Grimshaw (*Proc. Roy. Phys. Soc. Edinburgh*, **22**, 1934, 187–215). It also includes an English translation of the keys for the determination of families which appeared in Lindner's *Die Fliegen der Palæarktischen Region*.

The hibernation of *Phlebotomus papatasii* was studied a few years ago by E. Roubaud. His observations showed that there was an asthenobiosis; this he defined as a resting stage independent of external factors and which occurs cyclically after a number of

active generations as a consequence of an intoxication of the tissues of the larval organism by a surcharge of urates. There was no hibernation due only to external factors. Roubaud stated that this intoxication disappeared during a prolonged resting stage at a low temperature, during which the metabolism of the larva is much reduced while excretion proceeds. After this purification the larva is able to resume its normal course of development and usually the actual awakening from the resting stage is effected by a rise in temperature. However, he found some irregularities in the asthenobiosis of this insect compared with that of other insects he had previously studied. Later Roubaud stated that in *P. papatasii* an intoxication of the eggs may occur in the body of the insect if oviposition is delayed and further that larvæ hatching from such eggs incline towards asthenobiosis. O. Theodor (*Bull. Ent. Res.*, **25**, 1934, 459–72) has attempted to see whether in Palestinian *P. papatasii* there is an asthenobiosis as defined by Roubaud or whether there is a hibernation due to external (or other) factors. This investigator comes to the conclusion that hibernation of this insect in Palestine is primarily caused by low temperature, but that cyclic (? hereditary) factors exist which modify the phenomena of hibernation to a certain extent. He also finds there is always a certain number of resting larvæ throughout the summer in otherwise active broods. This inhibition of development shows that a latent inclination to diapause exists in all generations and that diapause may also be brought about by unfavourable conditions. Theodor could find no relation between the interval between hatching and oviposition and asthenobiosis.

An important paper on the chemoceceptors of blowflies by N. E. McIndoo (*J. Morphol.*, **56**, 1934, 445–75) has appeared in which the author deals with the morphology of all the so-called gustatory and olfactory organs of blowflies, and describes tests conducted to determine whether these insects taste with their tarsi and smell with their antennæ and palpi. The outstanding feature of this contribution is that it is reported by this investigator for the first time that the olfactory pores are really olfactory. For the last score of years or so their morphology has been described.

A. C. Evans (*Bull. Ent. Res.*, **26**, 1935, 115–22) has studied the growth of the adult fat-body and ovaries in the sheep blowfly in relation to diet and to the abnormal air-sacs. He puts forward a new hypothesis that the abdominal air-sacs in flies of the Muscid type have but little respiratory function and are chiefly concerned in (1) preserving the increased volume of the newly emerged fly and (2) providing ample space within the abdomen for growth of

the imaginal fat-body in the male and for the fat-body and ovaries in the female, both of which organs increase greatly in size.

OTHER ORDERS.—M. E. Dodson (*Q.J.M.S.*, **77**, 1935, 383–403) has described the structure of the female genital ducts, their accessory structures and development in the Trichoptera and made a comparison with the structure and development of the female genitalia in Lepidoptera. The ectodermal genital structures in female Trichoptera correspond in general plan to the supposed primitive condition in Lepidoptera, *i.e.* in which there is a single opening for egg-laying and copulation and only the oviduct has a direct opening to the exterior with the accessory copulatory structures leading dorsally on to the oviduct. The view is stated that the copulatory opening in Coleoptera represents the original common gonopore which is still present in Trichoptera. Backward shifting of the gonopore has taken place in many groups of insects but the Lepidoptera are peculiar in the retention of the original opening.

ARCHÆOLOGY. By E. N. FALLAIZE, B.A.

EARLY MAN IN AFRICA. EAST AFRICA.—Professor P. G. H. Boswell's investigations on the ground of the problems raised by Dr. L. S. B. Leakey's interpretation of his later archæological discoveries in Kenya have had, to say the least, a disturbing effect. In a preliminary report (*Nature*, March 9, 1935, p. 371) he regards the whole question of the early appearance of a type of modern man in East Africa as once more in suspense. Not only was he unable to locate the exact site of the discovery of the Kanam mandible and the cranial fragments from Kanjera, which Dr. Leakey holds to be the earliest evidence of modern man yet known, and of the occurrence in Kenya of a precursor of modern man, but he also concludes that the character of the quaternary deposits in the area is such as to afford no certain evidence which would determine the dating of the remains at the age suggested by Dr. Leakey. Further he is of the opinion that it is not possible to establish in this area a culture sequence of Stone Age industries from the earliest type onward corresponding to that at Oldoway in Tanganyika as Dr. Leakey maintains.

While the pendulum thus swings in Kenya, more stable and only a degree less stimulating conditions obtain in the archæological exploration of Uganda. Here Mr. E. J. Wayland has been at work on the geology of the country since 1919. Now, in place of the intermittent view of the prehistory of the country he has afforded hitherto, he has prepared a continuous outline covering completely in chronological order the course of his investigations to date (*J. R. Anthrop. Inst.*, **64**, Pt. 2) as a preliminary to a Geological

Survey Memoir to be published shortly. He points out that recently the sphere of the prehistorian has been much enlarged and that it is now necessary to take into account climatic and physiographic conditions and their changes in relation to their effect on the activities of man. This is especially important in Uganda where there is evidence of fundamental changes in the lake and river-direction systems and the latest of earth-movements has been responsible for such surface features as the Rift Valley and Lake Victoria Nyanza.

In summarising the results of his investigations bearing upon the prehistory of Uganda as shown in his chronological survey, Mr. Wayland interprets his evidence as pointing to two pluvial periods separated by an interpluvial period of aridity and each having an intrapluvial period of comparative dryness. These periods can be correlated, though only tentatively, with the periods which have been discerned in the glacial oscillations of the European system. Associated with these climatic variations are progressive series of Stone Age cultures.

In the first part of Pluvial I the rivers which previously ran east to west into the Congo system were deflected by rift into the Nile system. This period, Plio-Pleistocene and early Pleistocene, is correlated (tentatively as in all correlations mentioned below) with Günz and associated with the early Kafuan industry. An intrapluvial oscillation follows, during which the rivers and lakes dwindled and the Lake Albert rift and Victoria basin were intensified. It was followed by the second stage of Pluvial I when Lake Victoria is definitely established and glaciers appear on Ruwenzori and other mountains. This is correlated with Mindel and associated with the Later Kafuan industry. In the following interpluvial the lakes dry up more or less completely. In the first phase of period Pluvial II, correlated with Riss, Pre-Chellean and Protosangoan are followed by Chellean-Acheulean and early Sangoan. In the following intrapluvial oscillation Acheulean appears as a valley culture and Sangoan as a hill culture. In the second phase of Pluvial II the river-flow direction and Lake Victoria finally become more or less as they are to-day, owing to earth movements, Mousterian and Lower Aurignacian appear and develop into Still Bay and Upper Aurignacian, this period being correlated with Würm. It is to be noted that Aurignacian in Uganda has all the appearance of being a foreign culture and its distribution suggests an intrusion from north or north-east. In the post-Pluvial, corresponding to the Achen retreat, the Magosian appears and later the Wilton in a phase corresponding to the Bühl

stadium. Here there are only relatively small fluctuations in lake and river levels in accordance with climatic variation. In these microlithic industries pottery appears with Wilton, though it seems to be earlier in Kenya.

Mr. Wayland himself has of set purpose avoided search for remains of early man, owing to difficulties of supervision, but records two recent discoveries, one, by Archdeacon Owen of a primitive negro in association with stone tools fifty miles from Kampala, and a skull and skeletal parts of a pygmy-like individual with a broken crystal microlith from pistolitic red earth at Buggungu. These are to be published shortly. Though Mr. Wayland's exploration of the quaternary deposits of Uganda may not have brought to light further skeletal remains of early man, they have demonstrated the character of the typical early industries and indicate with some degree of certainty the conditions in which the remains of early man may be expected to appear. This in itself is a notable advance.

South Africa.—Meanwhile Professor T. F. Dreyer of Bloemfontein follows a like line of argument in approach to the problem of "prehistoric man" of South Africa and the associated cultures. Writing before Professor Boswell's report on Dr. Leakey's observations was available for his use, he points out (*Nature*, April 20, 1935, p. 620) that, assuming that Dr. Leakey has over-estimated the age of his deposits, the results from Kenya are very closely similar to those from the south coast of Africa. The cranial fragments from Kanjera resemble closely crania associated with the Mossel Bay culture of holocene age and a Late Stellenbosch skull belonging to a period of aridity immediately preceding the holocene. On this and still assuming an over-estimate on Dr. Leakey's part, Middle Stellenbosch, presumably of the same race, would be completely contemporaneous, more or less, with Kanjera man.

On the other hand in the Free State, with the same reservation in regard to time, there are other resemblances. The Lydianite industry develops in a series of five distinct climatic cycles from a Clacton-like phase through Mousterian, Aurignacian and Magdalenian-like phases, all associated with extinct species of mammalia.

Professor Dreyer concludes, therefore, that in South Africa, as is now accepted for Europe by the Abbé Breuil, the Chellean-Acheulean technique is contemporary with the Clacton-Mousterian technique, the difference in South Africa being due to difference in the material. A like distinction is to be seen in the human remains. Stellenbosch is associated with Kanjera man (or better

"prehistoric South Africans") and the Clacton-Mousterian with the huge, very primitive ancestral form of *H. Sapiens*.

NORTH AFRICA.—In both east and south Africa looks to the north for help in the solution of its archæological problems. The most recent research in North Africa indicates that reconsideration of certain problems on a wide basis is at hand, though the exact line it will take is by no means as yet completely evident. The necessity for this revision is pointed out in at least two recent studies of the Stone Age in North Africa.

Miss G. Caton-Thompson, who has been in charge of the expedition of the Royal Anthropological Institute to Kharga Oasis, of which one of the objectives was the investigation of the associations of culture and pluviation, in her Rivers Memorial Lecture to the Institute (summarised in *Man* for May) dealt with the bearing of her results on the problems of the North African stone age generally. Her investigations bring out very clearly the vital importance of water-supply in determining the distribution of early man. Its fundamental character is apparent even in the instance of surface finds in the desert, where digging has revealed the previous existence of water, although all traces of it have now vanished. The Kharga oasis itself is peculiarly favourable for determining increase of pleistocene moisture, free from fluvial influence. Conditions there show that even in the Middle Palæolithic, when there was abundance of rainwater, man's movements were still not unfettered. Implements still cling to the two areas of artesian mound-springs, and elsewhere they are scarce. On the scarp tools crowd only in the localised region of travertine formation deposited by springs and seepage. The importance of the mound-spring appears also to be evident in North Africa; but there the Upper Acheulean exhibits non-local peculiarities, which link with Palestine. Nothing comparable has been found in the Nile Valley.

In the succession of cultures at Kharga the Acheuleo-Levallois phase is succeeded by three different facies of Levalloisian. The Kharga Aterian belongs to a period of oncoming aridity, and thus it may be used as a convenient datum for the oncoming Upper Palæolithic distinction. Its typology has an archaic aspect, in which tanged points are scarce while the tanged end-scrapers and diminution in size, which mark many groups in French North-West Africa, are absent. Miss Caton-Thompson here points to the revision of views on Aurignacian origins in Western Europe and Kenya necessitated by M. Vaufrey's views of Capsian as late palæolithic or mesolithic; but the evidence for the late survival of a debased Aterian into very "late," such as found in the north-

west, is absent at Kharga. The Kharga Aterian is succeeded unconformably by a blade culture of Capsian affinities with microliths, and querns, but no pottery, and in spite of the presence of tanged arrow-heads and other types usually regarded as neolithic further west, is probably mesolithic. At other sites it develops by certain additions into a true neolithic. It is suggested that later Aterian groups, yet to be discovered, may bridge the gap.

The important question of the exact determination of these phases of the later Palæolithic and succeeding cultures in North Africa is further discussed in two papers to which a brief reference must here suffice. M. Vayson de Pradenne in *Antiquity* for March discusses the problem of implements of "archaic" type and cognate tradition with special reference to North Africa and the recent discoveries in the Abri Alain (see SCIENCE PROGRESS, **29**, 1935, 521), and suggests that they represent a "fossil" tradition, arising from the re-working or re-use of an implement, which as indicated by the occurrence of two different patinas on one specimen, belonged originally to a stratum earlier than that in which it was found. M. de Pradenne considers that certain implements found in the Abri Alain and in the adjacent caves belonging to the later (neolithic) period are ancient stone tools collected from the neighbouring Champ de Tir, an Aterian site, and that their re-use or their employment as models introduced a new type into the industry which took them over.

The second paper is a study of skeletal remains and archæological material from an Algerian cave, Abu Afalou, by MM. Arenbourg, Boule and Vaufrey (*Institut de Paléontologie Humaine, Mém.* 13). This memoir is too long and too important for a brief summary, but without entering into the details of discovery, it may be mentioned that it affords the authors the opportunity to enter into a consideration of the cultural relations of the Capsian-Ibéro-Maurusian industries of French North Africa, as well as to give—what has not hitherto been attempted—a comprehensive account of the important skeletal material which has been discovered in a period of over twenty years since investigations first began in the rock-shelter of Mechta el-Arbi. The general conclusion at which they arrive is that these and analogous industries wherever they occur, East Africa, South Africa, Egypt, Palestine or Western Europe, are localised developments of a fundamental Aurignacian industry just as the associated skeletal remains at el-Arbi, at Natufa in Palestine, and elsewhere are specialised forms of a fundamental Cro-Magnon.

It is evident that what cannot but be regarded as a brilliant study of difficult material will have to be considered very carefully

in its detailed application before it can be accepted as universally applicable. It is none the less stimulating and suggestive.

Archæology in Ireland.—During 1934 archæological studies in Ireland profited through the schemes for assisting the unemployed. Excavations already projected were enlarged in scope and others were put in hand which otherwise would not have been undertaken through lack of the necessary funds. Operations were carried out on twelve separate sites in various parts of Ireland ranging in period from post-glacial times down to the fifteenth century.

Of these excavations one of the most important from the point of view of the prehistorian was that at Ballybetagh, Co. Dublin, a site already noted for discoveries of the Irish elk. The object of the investigation was to classify events in their chronological sequence and the assistance of Professor Knud Jessen, of Copenhagen, the well-known authority on pollen analysis was sought. Professor Jessen took samples of peat from a number of sites, and although at the moment the results of the analysis are not fully available, a preliminary report indicates in general outline some very interesting results of great chronological and cultural importance (*Irish Naturalist, Quaternary No.*, Nov. 1934). It is, for example, now demonstrated that the Irish elk survived to a much later period than had been thought, and the association of remains of this animal with human remains or artefacts is no longer to be regarded as irrefutable evidence of high antiquity. This was in line with the general result that early types of fauna and flora were found to have survived to a much later date in Ireland than elsewhere. It was, moreover, demonstrated that conditions in Ireland correspond with those on the Continent in so far that the peat of the late Bronze Age was shown to have been laid down in much moister conditions than that of the early and middle Bronze Age. It is further noted as a point of interest that the climatic change correlates with a definite change in cultural development, the native culture which developed steadily during the two earlier phases of the Bronze Age now showing definitely the effects of external influence.

Another investigation of no little interest, in view of the prolonged discussion of the occurrence of palæolithic man in Ireland, was that of a cave at Kilgreany, Co. Waterford. It was this cave which was excavated some years ago by members of the Bristol Spelæological Society and yielded what were then claimed on the evidence of associated fauna to be the remains of palæolithic man. Unfortunately this conclusion has not been substantiated by the

present excavations and the question of palæolithic man in Ireland is once more in suspense.

Of excavations of later date one of the most important for Irish social and economic conditions in the late Bronze and Iron Ages was on a site at Cush near Kilfinane, Co. Limerick. The site is a complex of ring forts and burial mounds. Usually ring forts are found singly only; but here there was a unique arrangement of six forts conjoined and connected with a rectangular enclosure surrounded with a ditch and bank. Four of these forts were completely excavated, and each was found to contain a souterrain. For the first time one of these curious structures was dated with certainty as earlier than the Iron Age. One which had collapsed was found to predate a Late Bronze Age burial. Also for the first time houses were found in connection with the forts—not indeed within them, but in the rectangular enclosure. The examination of this area is not yet complete; but it would seem likely that it will furnish an answer to the as yet unsolved problem of the existence of towns in prehistoric Ireland. A preliminary account of these and other investigations carried out under the Free State's scheme for the relief of unemployment will be found in *Discovery* for April.

The very active movement at the moment for the promotion of Irish archæological studies adds to the interest, if not to the value, which is patent to all, of the Presidential Address of the Abbé Breuil to the Prehistoric Society of East Anglia (*Proceedings*, 7, Pt. 3) which deals with the origin and distribution of rock and dolmen engravings, with special reference to those of Ireland. The learned Abbé described these in detail and analysed their art motifs. In summarising his results he suggested that they showed important relations between Iberia and Ireland, and reached Scandinavia through England and Scotland, but in Brittany, though undoubtedly present, they did not penetrate to the interior of the country.

NOTES

Zirconium and Titanium (M. S.)

Although both zirconium and titanium are found widely distributed in nature and although discovered long ago, the full possibilities of utilisation of compounds of these two elements have been recognised only within recent years. Zirconium metal was isolated more than a century ago, but it was the oxide zirconia which attracted all the attention, Berzelius noticing in 1829 that thoria and zirconia gave out an extremely brilliant light when a non-luminous flame played on these rare earths. Just fifty years ago Dr. Carl Auer, known later as Baron Auer von Welsbach, began his studies on the rare earths which resulted in the establishment of the incandescent gas-mantle industry. Zirconia was one of the chief earths used in these early experiments ; and although it soon took an unimportant position when thoria containing 1 per cent. ceria became the most favoured mixture for gas-mantles, yet zirconia has now returned to a greater prominence, while the gas-mantle trade has declined with the universal predominance of electric light. It is in the refractory industry that this is so noticeable. A high melting-point, an inertness towards both acids and alkalis, a low thermal conductivity, a low coefficient of expansion which enables furnace walls to withstand sudden changes in temperature without fracture, low porosity, and the possibility of applying it by means of a binding agent to other refractories : all these explain the importance of zirconia in this direction. For laboratory purposes combustion tubes of zirconia are now in use, while in America it has been found possible to use the raw mineral zirkite containing up to 84 per cent. of zirconia for the construction of electrical wire-wound furnaces. Difficulties in treating zirconium ores seem to be overcome, the United States Bureau of Mines having favoured an extraction process using sulphuric acid under pressure ; while a second contrasting method for dealing with silicate ores such as zircon is to fuse with alkalis and to leach out the product. The recent scare with regard to poisoning caused by the use of antimony oxide in enamels recalls the fact that as an opacifying agent for this industry zirconia

has a high efficiency and is entirely harmless. Although the 10 per cent. of zirconia necessary in iron enamels means a somewhat higher cost, there is such an improvement in strength and resistance to attack that the extra expense is to be recommended.

Unlike titanium, the compounds of which have also become prominent again, zirconium is tending to add to its importance in the form of the free metal. The latter is silver-white, very hard, and remarkably resistant towards corrosion; its future among special metals for special purposes should be assured when the research which is now going on gives us cheaper methods of separating it than the use of calcium or magnesium in displacing it from its compounds. In America increased attention has been given to the use of aluminium, a metal which gave trouble at first since about 1 per cent. of it remained in the zirconium metal liberated. A recent process includes potassium chlorate in the melt in order to oxidise this impurity. Electrolytic processes also hold out promise, a graphite crucible being used to hold the melt consisting of a double fluoride of zirconium and potassium with the addition of other salts of sodium and potassium. The powdered metal obtained in such processes needs a treatment similar to that used for tungsten, *viz.* it has to be pressed, de-gasified, rolled, and swaged. Ferro-alloys for use in high-speed steels and for introducing the metal (0·34 per cent.) in French armour-plate are at present undergoing a greater development than the metal itself. There is also a tendency to utilise zirconia in other directions than as a refractory, for it has been found to be a substitute for the quartz used in glass for lenses and thermometers and in porcelains for the electrical industry; and there are other new uses such as the inclusion as a filler in rubber.

The large deposits of a titanate of iron (known as ilmenite) in Norway should give an impetus to developments of titanium in Europe comparable to the intensive research which has borne fruit in America. Titanium metal has long been known, but even with the demand for all types of metals for special industrial purposes there are no uses for the metal itself to-day. Ferro-titanium is in common use for removal of oxygen and nitrogen in steel manufacture, this combination with nitrogen having a second significance in the use of titanium nitride in American foundry practice for giving resistance to cores and moulds. Titanium dioxide under such proprietary names as Titanox is one of the most important white pigments in the paint and enamel industries, for its covering power is higher than that of any other product, while its extreme insolubility and stability explain its inertness and absence of toxic properties. Modern methods of extraction tend more towards the process in which

sulphuric acid is used for digesting the ilmenite, the solution being hydrolysed under conditions which free it from iron impurities. A natural dioxide, rutile, is now concentrated for commercial use. The textile industries have greatly benefited within recent years from this progress in titanium research; for titanic acid is a mordant, titanyl chloride and sulphate solutions are of extreme value as stripping agents, while titanium potassium oxalate (abbreviated to T.P.O. by both manufacturer and dyer) is a fixing agent. Manchester manufacturers have provided for commercial users a stable form of titanous sulphate which avoids the formation of precipitated titanic acid as a "bloom" on striped materials. The double oxalate seems to have two different uses, the first as a fixing agent when dyeing with basic colours, and the second being its use in itself as a colouring matter in leather dyeing, giving a yellowish-brown shade without other added colouring agent. In addition to all these applications, titanium compounds are included in inks, abrasives, cements, porcelains, special glasses, and in a host of other commodities.

British Sunshine (E. J. S.)

The "Averages of Bright Sunshine for the British Isles for the periods ending 1930," which has recently been issued by the Air Ministry (H.M. Stationery Office, 1*s*. net), provides a mass of interesting data presented both as actual totals and as percentages of the total possible sunshine at the divers stations, which number over a hundred and ninety. The information is of considerable interest to the student of public health as well as to the biologist. For the latter the importance of the number of hours of sunshine is both as a measure of the assimilatory value of the light and because the capacity to set seed by some plants is apparently determined by the amount of sunshine available. The range in total amount is considerable, from between 1,800 and 1,900 hours in the Channel Islands to 1,035 in the Shetlands. Oldham Road, Manchester, has the unenviable average total of only 970 hours' sunshine (1906–1930), though the average for almost the same period at Whitworth Park is 46 hours greater. The monthly means show a maximum for May in many areas, though in general the amount of sunshine is fairly evenly distributed during the months from April to September. The present data are limited to the thirty years ended 1930 and the averages obtained from shorter periods have not been adjusted as in the previous publication. Data obtained from Eastbourne and York are furnished which indicate that the variations in the average values are but very slightly affected by increasing the period over which the

average is obtained from ten to thirty or even fifty years, although for individual months the difference may amount to about 20 minutes of sunshine.

Royal College of Science Annual Dinner (H. J. E.)

This Annual Dinner was held on May 4 in the Rembrandt Rooms, South Kensington, the President of the R.C.S. Association, Prof. W. W. Watts, F.R.S., being in the chair. The occasion was noteworthy in that the principal guest was Prof. H. E. Armstrong, F.R.S., and the dinner was held on the eve of his 87th birthday. One of his old pupils, Prof. W. P. Wynne, F.R.S., proposed the health of Prof. Armstrong, recalling the fact that this year marks the seventieth anniversary of the latter's entry into the Royal College of Chemistry as a pupil of Hofmann. Prof. Wynne's speech gave expression to the feelings of real affection of all present towards "H. E. A." The reply was characteristically vigorous. Wearing a tricoloured waistcoat to celebrate the early days of the dye industry in this country, Prof. Armstrong deplored the insufficient use of colour by women to-day. He also expressed the belief that the application of science to brewing has spoiled beer for the connoisseur. This speech was a mixture of wit and good sense which appealed to all. The toast of the guests was proposed by Mr. Hewer, who took the opportunity of handing to Prof. MacBride a portrait presented by the students and staff of the department of Zoology, of which he is now Emeritus Professor. The proceedings concluded with a dance in the ball-room, which was thoroughly enjoyed by all those present.

The "Geographical Magazine" (C. B. F.)

This is a new monthly periodical whose aim is to give to the general public such a knowledge of the geographical stage on which the human drama is played that they can form from it a sound basis for an understanding of world problems. To-day no one doubts that geographical facts are an essential foundation for such knowledge; and there is a wide field awaiting a magazine of the type of the *Geographical Magazine.*

The method, as far as it can be judged from the first number and the editorial Foreword, is to give each month a group of short articles on different geographical areas or problems. Each article is very well illustrated by reproductions of photographs. There are nearly a hundred illustrations in all. Each is written by an author who has some personal knowledge of the places he describes, and who may, or may not, have some knowledge of geography. Four

of the articles deal with areas within the British Empire, two with areas wholly outside it, and the remaining two overlap its boundaries. The topics are Abyssinia; the Lake District, a National Park?; Aborigines of Central Australia; Rajputana; Arctic Air Route; the Syrian Desert; India's North-East Frontier; and Amsterdam. Most of them are very short, for there are only 83 pages of text, and nearly half of this space is taken up by illustrations. Captain Kingdon Ward's description of the forests which form the north-east frontier of India is an excellent piece of work, and shows what can be done within these limits, though he has no map to show the precise location of the places he mentions. The account of Ethiopia (Abyssinia) is of very topical interest, and indicates well the dangers to peace inherent in undemarcated boundaries under modern conditions.

Such a magazine must be popular if it is to continue. Only a large circulation can pay for such a wealth of vivid pictorial illustration. Yet if it is to be worth while it must also carry out its purpose of giving sound geographical knowledge. We hope there is a sufficient public willing to purchase, at the modest price of 1*s*., such a magazine, and that the *Geographical Magazine* can succeed in establishing a reputation for presenting reliable information vividly and clearly, without any sacrifice of accuracy to mere picturesqueness or of good English to journalese. This first number sets a high standard, and is a welcome addition to British periodicals.

Miscellanea.

The following is a list of the names of those elected this year to be Fellows of the Royal Society: Dr. N. K. Adam, physical chemist, University College, London; Prof. E. N. da C. Andrade, Quain professor of physics, University of London, University College; Sir Frederick G. Banting, professor of medical research, University of Toronto; Prof. S. P. Bedson, professor of bacteriology, London Hospital Medical School; Mr. E. T. Bowen, chemist, University College, Oxford; Mr. G. E. Briggs, plant physiologist, University of Cambridge; Prof. H. Graham Cannon, professor of zoology, University of Manchester; Prof. W. E. le Gros Clark, Dr. Lee's professor of anatomy, University of Oxford; Prof. J. S. Foster, professor of physics, McGill University, Montreal; Dr. A. L. Hall, lately assistant director of the Geological Survey of the Union of South Africa; Dr. W. H. Hatfield, Brown-Firth Research Laboratory, Sheffield; Dr. J. de Graaff Hunter, lately of the Survey of India; Dr. B. A. Keen, physicist, Rothamsted Experimental Station; Prof. R. A. Peters, Whitely professor of biochemistry,

University of Oxford; Prof. J. Read, professor of chemistry, University of St. Andrews; Dr. R. N. Salaman, director of the Potato Virus Research Station, Cambridge; Dr. R. Stoneley, lecturer in mathematics, University of Cambridge.

H.M. the King has approved the award of the Founder's Medal of the Royal Geographical Society to Major R. A. Bagnold for his journeys in the Libyan Desert and of the Patron's Medal to Mr. W. Rickmer Rickmers for his travels in the Caucasus and Russian Turkistan.

Prof. R. Robinson, professor of chemistry in the University of Oxford, Prof. F. D. Adams, emeritus professor of geology in McGill University, Montreal, and Prof. A. V. Hill, Foulerton research professor of the Royal Society, have been elected to be Foreign Members of the Royal Academy of Sciences, Stockholm.

Prof. A. C. Seward has been elected to be a member of the Norwegian Academy of Science and Letters, and Prof. F. G. Donnan an honorary member of the Chemical Society of Roumania.

The Kelvin Medal, awarded triennially by the Institution of Civil Engineers, has, this year, been awarded to Sir J. Ambrose Fleming.

Miss Gertrude Caton-Thompson has been awarded the Rivers Memorial Medal of the Royal Anthropological Institute for the year 1934 and the Duddell Medal of the Physical Society has been awarded to Dr. W. E. Williams for his work in general and particularly for that on the reflection echelon spectroscope.

Sir Henry Lyons has been appointed Chairman of the Advisory Council for the Science Museum in place of Sir Richard Glazebrook, who has resigned.

The following is a list of the names of those recently elected to be Presidents of the scientific Societies mentioned: Royal Astronomical Society—Mr. J. H. Reynolds; Geological Society—Mr. J. F. Norman; Chemical Society—Dr. N. V. Sidgwick; Society of Chemical Industry—Mr. W. A. S. Calder; Institute of Metals—Dr. H. Moore; Ray Society—Sir Sidney Harmer; Royal Aeronautical Society—Lieut.-Col. J. T. C. Moore-Brabazon; Society of Public Analysts—Mr. John Evans.

We have noted with great regret the announcements of the death of the following well-known men of science during the past quarter: Prof. H. B. Baker, F.R.S., emeritus professor of chemistry in the Imperial College of Science; Sir John Rose Bradford, F.R.S., emeritus professor of medicine in the University of London; Sir John Collie, chief medical officer of the Metropolitan Water Board; Mr. C. F. Cross, F.R.S., of viscose fame; Dr. Shepherd Dawson,

psychologist; Prof. W. Duane, of Harvard University, biophysicist; Dr. Carl Duisberg, chairman of the I.G. Farbenindustrie; Mr. John Fraser, botanist; Mr. W. J. A. Grant, Arctic explorer; Prof. A. Hantzsch, of Leipzig, chemist; Prof. R. O. Herzog, chemist; Prof. W. R. Hodgkinson, formerly professor of chemistry in the Ordnance College, Woolwich; Mr. H. R. Kempe, author of the *Engineers' Year Book*; Prof. J. J. R. Macleod, F.R.S., regius professor of physiology in the University of Aberdeen; Sir William Morris, at one time superintendent of the Geodetic Survey of South Africa; Mr. J. Milton Offord, president of the Quekett Microscopical Club; Prof. Ganesh Prasad, Hardinge professor of pure mathematics in the University of Calcutta; Mr. H. C. Ponting, photographer; Sir E. Sharpey-Schafer, F.R.S., emeritus professor of physiology in the University of Edinburgh; Prof. W. J. Sinclair, director of the Museum at Princeton University; Prof. Arthur Thomson, of Oxford, anatomist; Sir James Walker, professor of chemistry in the University of Edinburgh.

H.M. the King has graciously consented to become Patron of the Institute of Chemistry. The Institute received its charter from Queen Victoria fifty years ago, eight years after its foundation. It is now in a very strong position with over 6,000 fellows and associates and some 800 registered students.

The Air Ministry has set up a small committee to investigate the application of scientific discoveries to assist in defence against aerial attack. Mr. H. T. Tizard has consented to act as chairman and the other members are Prof. A. V. Hill, Prof. P. M. S. Blackett and Mr. H. E. Wimperis.

The Admiralty has decided to construct a magnetic survey ship to continue the work formerly performed by the American ship *Carnegie* which was destroyed by fire in the harbour at Apia, Western Samoa, on November 29, 1929. In particular it has become necessary to survey the southern Indian Ocean where recent changes in the secular variation have rendered calculated values of the magnetic elements based on older data unreliable. The design of the ship has not yet been announced but a sum of £10,000 odd has been set aside in this year's estimates towards the cost of its construction.

Sir James Jeans has been elected to be professor of astronomy in the Royal Institution. The chair is a new one, for never before has the Institution appointed a professor of this subject; it is also the first new chair to be established there for more than seventy years.

Prof. W. J. de Haas of Leiden announced on February 15 that he had succeeded in reaching a temperature only 0·005 deg. C. above the absolute zero.

In the *Report* of the Department of Scientific and Industrial Research for the year 1933–34 it was stated that the Wool Industries Research Association's process for "producing unshrinkable wool, yarns and fabrics, claimed to withstand the rigours of laundering" was being "exploited under mill conditions" and that it was hoped that materials made of the new yarn would be available for the public early in 1935. We regret to learn from *Nature* that the details of the commercial process have taken longer to work out than was anticipated and that, in consequence, the new material will not now be available until 1936.

A Central Laboratory for the United Dairies Ltd. was opened by the Parliamentary Under Secretary to the Ministry of Health on April 5. It occupies part of a 7½-acre site at Wood Lane, W., and is housed in a building 200 ft. long specially designed for laboratory requirements. The laboratory is divided into four sections: (*a*) research—four rooms each accommodating eight workers and their assistants, (*b*) routine control—six rooms devoted each to one type of work such as routine bacteriological tests and general foodstuff tests, (*c*) stores, (*d*) library and records. Investigations now in progress in the research laboratory include (i) the purification of creamery and factory effluents, (ii) the bacteriology of Cheddar cheese, (iii) the reductase test, (iv) thermoduric organisms in milk, (v) the significance of *B. coli* in relation to pasteurised milk. This work is directed by an Advisory Research Council, a chief chemist and a properly qualified staff of research chemists. The work of the new Central Laboratory will supplement, but not, of course, supersede, that carried out in the seven district laboratories in London and the twenty-six others located in the chief milk-producing areas.

On April 1 the scheme of artificial lighting at the National Gallery, which has been the subject of extensive research during the last six years, was brought into use, and the public will be able to visit the Gallery on three evenings in the week up till 8 p.m. Scientifically, it is clear that the aim should be to illuminate the pictures with considerable intensity without particular reference to the remainder of the room: actually, however, this produces a somewhat depressing effect, and therefore a modified scheme is in operation. In practice, an intensity of about 4 foot-candles is

obtained over the picture-carrying parts of the walls. Lantern fittings, each containing one frosted high-wattage lamp (1,500 watts), are suspended from the ceilings, and provided with an appropriate system of louvers and reflecting surfaces. Masks are employed to cut out direct glare in the direction of doorways, though the architectural design of the galleries makes it impossible in some instances to do this completely, having regard to the fact that the distance from source to walls is usually different in various directions (*i.e.* the rooms are not square). The viewing distance is assumed to be eleven feet from the walls.

The *Journal of Research* of the National Bureau of Standards for December 1934 contains a paper by Judson and Page on the present magnitude of the length standards used in the United States. The N.B.S. has four platinum-iridium metre bars obtained from and certified by the Bureau International des Poids et Mesures of which one, United States Prototype Metre 27, is considered to be the primary standard. This was made in 1888 and sent to the U.S.A. from Paris in 1890. It was sent back for test in 1903 and again in 1922–23. The 1903 test indicated that it had shortened by $0{\cdot}4 \times 10^{-6}$ metre, but Fischer, who was responsible for the measurements, was not satisfied with the accuracy of the result, being, in particular, doubtful of the accepted value of the coefficient of thermal expansion, and the length stated on the 1889 certificate was retained. Subsequent measurements justified his opinion, for it has been shown that the Paris standards themselves have altered in length and that the value of the coefficient of expansion needed modification. At the Eighth General (International) Conference on Weights and Measures which met in Paris in 1933 new corrections to the nominal lengths of the standard metre bars at 0° C. and new coefficients of expansion were authorised. As a result Metre 27 stated to be $1{\cdot}6 \times 10^{-6}$ metre too long at 0° C. in 1889 is now certified as $1{\cdot}47 \times 10^{-6}$ metre too long, *i.e.* it has remained unaltered to about 1 part in ten million for over forty years. On the other hand, Metre 21 (also of platinum-iridium) $2{\cdot}5 \times 10^{-6}$ metre short in 1889 now appears to be $3{\cdot}42 \times 10^{-6}$ metre short. Here there is in all probability a real change in length, but this particular bar "has probably had more laboratory use than any other platinum-iridium standard distributed by the B.I.P.M." As a matter of interest it may be added that the coefficient of thermal expansion of the metal in Metre 27 given as $(8{\cdot}657 + 0{\cdot}00100t) \times 10^{-6}$ per deg. C. in 1889 is now stated to be $(8{\cdot}6210 + 0{\cdot}00180t) \times 10^{-6}$—a decrease of 1 part in 360 at 15° C.

The March number of the *Journal of Research* contains two papers by Roeser and Wensel and others dealing with thermocouples. The first contains tables giving the e.m.f. of chromel-alumel thermocouples at various temperatures between − 200° C. and + 1400° C. and the other describes methods of testing thermocouples and thermocouple materials. Chromel and alumel wire for thermocouples is manufactured by the Hoskins Company. Chromel P is an alloy of 90 per cent. nickel and 10 per cent. chromium, alumel contains approximately 95 per cent. nickel with aluminium, silicon and manganese making up the other 5 per cent. The wire supplied regularly to pyrometer users is " matched " by the manufacturer so that a completed thermocouple will develop an e.m.f. of 36·20 millivolts when one junction is at 32° F. and the other at 1600° F. The useful range is − 200° C. (e.m.f. − 5·75 mv.) to 1200° C. (e.m.f. + 48·85 mv.) and the scale is nearly linear, for at − 100° C. the e.m.f. is − 3·49 mv., at 100° C. + 4·10 mv. and at 1000° C. 41·31 mv. No. 8 gauge wire is commonly employed to give a long period of usefulness but the e.m.f. is independent of the size of the wire. Other materials listed by Roeser and Wensel as suitable for general use are platinum-rhodium (useful range 0° C. to 1450° C., maximum temperature measurable 1700° C.), iron-constantan (− 200° C. to 750° C., 1000° C.) and copper-constantan (− 200° C. to 300° C., 600° C.).

Sir Gilbert Walker has drawn our attention to an error in his article entitled " Clouds in the Sky and in the Laboratory " which appeared in Volume XXIX, 1934–35. The sentence beginning on line 17 from the bottom of page 390 should read : Thus if hot water is poured into a dish and cold milk introduced *under* it *as an index*, the *water* descends . . .

ESSAY REVIEWS

THE PRE-CAMBRIAN GEOLOGY OF EGYPT. By G. W. Tyrrell, D.Sc., F.G.S., University of Glasgow. Being a review of **Geology of Egypt, Vol. II: The Fundamental Pre-Cambrian Rocks of Egypt and the Sudan; their Distribution, Age and Character. Part 1: The Metamorphic Rocks.** By W. F. Hume, D.Sc., F.R.S.E., F.G.S., late Director of the Geological Survey of Egypt. [Pp. lxv + 424, with 96 plates.] (Cairo: Government Press, 1934. Piastres 300.)

The first volume of Dr. Hume's great work on the Geology of Egypt was published in 1925 and dealt with the Surface Features of Egypt, their Determining Causes and Relations to Geological Structure. It also included a bibliography of over 900 items. Now, nine years later, we have the sumptuous volume under review which, while it is of about the same size as the first volume, nevertheless only covers the first part of its subject, namely, the older metamorphic rocks that occur within the fundamental Pre-Cambrian Complex of Egypt and the Sudan, a fact which sufficiently indicates the magnificent scope of the whole work. The comprehensiveness of this second volume is attested by the publication of the complete list of contents of which, naturally, only the pagination of the first part, the part that is now published, can be given. From this list the remaining parts of the Pre-Cambrian volume promise a feast of good things. Part II will deal with the Later Plutonic and Minor Intrusive Rocks of the Pre-Cambrian, and Part III with the Minerals of Economic Value associated with the Intrusive Pre-Cambrian Igneous Rocks and Ancient Sediments, closing with a general summary of Pre-Cambrian geology in Egypt and a comparison with the basement formations of other countries. Since general questions of Pre-Cambrian correlation and succession are thus to be discussed in a later part of the work, we shall not deal with them in the present review. Presumably further volumes will treat of the Post-Cambrian formations of Egypt. These are the bare architectural outlines of the vast edifice that Dr. Hume is building from the foundation of his thirty-four years of service on the Geological Survey of Egypt, of which he was latterly the Director.

The present volume begins with a long introductory section which includes a brief geological review of Egypt's past and an historical review of the growth of geological knowledge, clearly designed to aid the non-technical reader in understanding and digesting what follows. The general geology of Egypt in broadest outline is the subject of the incomparable first paragraph of Winwood Reade's *Martyrdom of Man*, and Dr. Hume might have used this passage as a motto for his memoir. In brief, the southern and eastern parts of Egypt consist of Pre-Cambrian rocks, the rest of its vast area being occupied by strata of Palæozoic, Mesozoic, and Kainozoic ages, mostly the two last-named groups. The Pre-Cambrian gneissic outcrops converge to the Cataracts, and through that narrow mountainous gateway the Nile pours to the north into the desert region floored by the younger deposits, making a narrow strip of fertile territory which expands suddenly at the delta.

The book under review consists of the first two chapters of the volume on the Pre-Cambrian Rocks of Egypt and the Sudan, dealing solely with the metamorphic rocks of that complex. Chapter I treats of the Fundamental Gneiss, Chapter II of the Pre-Cambrian Crystalline and Sedimentary Schists. The term " Pre-Cambrian " is used in the List of Contents for the heading of Chapter II, but it is transformed into " Pre-Carboniferous " on p. 7 and p. 86 of the text, the latter as a principal heading. Of course, as Dr. Hume points out, no definitely identifiable fossil organisms have yet been found in the oldest sedimentary rocks, and the oldest formation which contains identifiable fossils is of Carboniferous age. Hence the term Pre-Carboniferous is strictly more correct than Pre-Cambrian, notwithstanding the almost complete certainty that the basement rocks of Egypt are of Pre-Cambrian age. The non-uniformity of nomenclature referred to above is thus probably an oversight.

In the introductory matter to the present part of his work Dr. Hume gives a valuable summary of the whole volume on the Pre-Cambrian, chapter by chapter, although two-thirds of that volume are still to appear; also the classification of igneous rocks which he has adopted; a table of the Pre-Cambrian succession of igneous and metamorphic rocks in Egypt, Sinai and the northern Sudan, correlated with similar occurrences in other areas; and an historical review of the fundamental Pre-Cambrian of Egypt and the Sudan. This last section is lavishly illustrated by coloured maps taken from older memoirs.

Chapter I, on the Fundamental Gneiss, occupies 85 pages, and begins with a section explaining the distribution and characters of

the basement rocks on the earth's surface, including the significance of pegmatites and the occurrence of microcline. Next comes a section on the distribution of gneisses in Egypt and the Sudan illustrated, as indeed the whole volume is, by beautiful coloured plates of the rocks, both as hand specimens, in thin sections and in the field, and by numerous maps. Further sections deal with the characteristics of the older gneiss as developed in North-East Africa, and a summary account of the gneisses of Egypt and the Sudan. The Egyptian gneisses form part of the Fundamental Pre-Cambrian Complex of Northern and Central Africa, and the frequency of N. to S. strikes in the gneisses of the northern Sudan is regarded as bringing them into close relation with the basal gneisses of Central Africa. On the other hand, some of the gneisses are found in close association with the ancient schists of sedimentary and volcanic origin considered in Chapter II, and are thus probably transgressive batholiths or, in other cases, concordant sheets of *lit-par-lit* characters.

Chapter II, on the "Pre-Carboniferous Crystalline, Volcanic and Sedimentary Schists of Egypt and the Sudan," occupies 215 pages, the remainder of the volume. It is divided into eight unequal sections dealing with the various stratigraphical and petrological units. The first-hand accounts of the distribution and petrographical characters of the various groups are interspersed with copious quotations from authorities who have dealt with the comparatively few regions that Dr. Hume himself has not traversed or of which he possesses insufficient personal knowledge. Amongst the interesting rocks described in this chapter are serpentines which contain the well-known Egyptian emerald mines, as well as other economic minerals such as chromite, talc and asbestos. The beautiful ornamental stone of Gebel Dokhan known as "Imperial Porphyry" was the object of intensive quarrying by the Roman emperors. After the Roman period the very site of the quarries was lost, and it was only rediscovered in 1823. The "*porfido rosso antico*" comes from dikes, and its reddish-purple colour is due partly to the presence of the rose-red manganiferous epidote known as withamite, and partly to disseminated hæmatite.

A long set of excellent chemical analyses by Dr. H. F. Harwood illuminates the petrological aspects of the igneous and metamorphic rocks described in this memoir. Throughout the work the facts are very fully documented by copious abstracts from earlier writers on the geology of Egypt and, in many cases, their maps and cross-sections are reproduced in colour. Too great praise cannot be given to the excellence of the illustrations including, as they do, beautiful

photographs of scenery and a number of the late H. M. Cadell's instructive line drawings. The literary style of the work is leisurely, lucid and dignified, qualities well matched by its sumptuous format. It is a small quarto in size, bound in red cloth with gold lettering, well printed on fine paper with the title-page in black and red characters. A model index of 124 pages closes this impressive work, the continuation of which will be awaited with impatience by all geologists interested in the foundation stones of the earth's crust. The International Pre-Cambrian Association (see SCIENCE PROGRESS, Jan. 1935, p. 528) would do well to keep its eye on Egypt, for when Dr. Hume's work is completed no Pre-Cambrian area upon the earth, with the exceptions of the North-West Highlands of Scotland and certain regions in Scandinavia and Finland, will be so fully documented as this land of august and ancient memory.

THE ACHIEVEMENTS OF A GREAT ENGINEER. By PROFESSOR W. J. GOUDIE, D.Sc., the University of Glasgow. Being a review of **The Scientific Papers and Addresses of The Hon. Sir Charles A. Parsons, O.M., K.C.B., F.R.S.** With a Memoir by LORD RAYLEIGH. Edited by THE HON. G. L. PARSONS. [Pp. xxviii + 260, with 8 plates and 41 figures.] (Cambridge: at the University Press, 1934. 15s. net.)

IN the domain of engineering achievement, which has profoundly affected the welfare of mankind, the creative work of Charles A. Parsons stands out pre-eminently. While this work is fully recognised by scientists and engineers, it is questionable if it is equally understood by the public outside professional circles. It is well, therefore, that an opportunity should be afforded them to become acquainted, at first hand, with the writings of this distinguished engineer, who may be aptly termed the nineteenth-century successor to James Watt. This subject matter has now become available to the general public in the volume, having the above title, which has just been issued by the Cambridge University Press. It occupies 260 pages, and is divided into three parts. The first contains a selection of papers, arranged in chronological order, dealing principally with steam turbine development, which were read before scientific, engineering and ship-building societies; also several Presidential Addresses. The total number of these publications is twenty. The second part consists of papers describing experiments on various substances subjected to high pressure and temperature, and Parsons' many attempts to produce artificial diamond, which were contributed to the *Phil. Mag.*, and the Royal Society. The third part consists of three appendices, the first being an account

of Parsons' sound amplifier or auxetophone, written by the late A. Q. Carnegie. The second is a short account of his interest and activities in connection with the production of optical glass and the manufacture of astronomical telescopes, written by the editor. The third is an imposing list of his contributions to scientific and technical literature, singly and in conjunction with others, of which the selected papers in Part I of this volume form a considerable proportion.

When he commenced the work of steam turbine development, which has made his name famous throughout the world, Charles A. Parsons had many advantages, scientific and mechanical, that his great predecessor James Watt lacked. He also possessed the aptitude of the born mechanic, fostered from his childhood in the workshop of his father, the Earl of Rosse, at Birr Castle, where, to quote Sir Robert Ball, one of his early tutors, Parsons "was always making all sorts of machines." At the age of thirteen he constructed a sounding device, which was successfully applied to his father's yacht, the principle being the same as that of the Kelvin sounding machine. In conjunction with his brothers he constructed a steam motor car having a 4 h.p. engine, cardan shaft, bevel drive and gear box. Here was an apt illustration of the boy being "father to the man." Further, during his shop apprenticeship at Elswick Works, after his successful career at Cambridge, he was much influenced by the example of Lord Armstrong, a man of resolution who would not be daunted by any mechanical difficulty, however formidable. In 1880, when he was occupying his first professional post with Kitson's of Leeds, the dynamo and electric motor entered the industrial field, and called for speeds far in excess of any the reciprocating steam engine could produce without some speed-up gear. Parsons realised that the day had arrived for the introduction of a high-speed rotary steam prime mover. His first attempt was the epicycloidal steam engine, in which the cylinders rotated about the crank shaft at half the speed of the shaft; but he soon recognised its limitations, and decided that nothing less than direct rotary motion, due to dynamic action of the fluid on rotor blades, would give a satisfactory solution. The turbine idea was not original, in fact it was very ancient. Hero in 150 B.C. had enunciated the pure reaction principle; Branca, in 1629, the impulse one. In the list of British patent specifications Pilbrow (1842) had outlined the axial flow velocity compound-impulse turbine; Wilson (1848) the radial flow compound-impulse and the axial flow reaction machine (similar to Parsons'), and Heath (1838) had advocated the use of the diverging type of nozzle.

All the attempts of these pioneers to put their ideas into practice seem to have ended in failure. In one of this series of papers Parsons comments on the efforts of these men prior to 1880. He states, "It would take too long to trace the initiation of each idea, but one may say, in the light of recent experience, that most if not all the designs showed a want of knowledge of the properties of steam and of materials, and could not have given satisfactory results." This was written in 1906. In his Watt Anniversary Lecture to the Greenock Philosophical Society (1909) he further states, "I had a model made of Wilson's turbine eighteen years ago, and under steam all that could be said was that it went round the right way. I do not think that Wilson can have made a model and tested it before he applied for his patent, the course followed by James Watt, and one which is to be strongly recommended to the attention of inventors generally, under almost all circumstances, as saving time, money and disappointment."

Parsons had thus a very clear conception of the nature of the problem to be faced, and after weighing up the possibilities of the various systems he decided on the multistage reaction type. Reasoning by analogy from the water turbine, he decided to divide the total pressure drop over a number of turbines working in series. The practical outcome of his efforts, the pioneer machine, now housed in South Kensington Museum, is described and illustrated in the first and probably the most interesting paper of this series. This little machine, developing about 6 kw., drove a dynamo of his own design, directly, at the high speed of 18,000 r.p.m. The production of the latter machine, at this early date of electrical development, was as big an achievement as the turbine that drove it. The description is given in such clear and simple language that any layman can understand it. Reading between the lines, however, one can realise what an amount of resource, technical skill and experimental testing must have been involved in the production of this turbine. In one of the later papers Parsons deals with the experiments on shaft vibration connected with this machine.

It is characteristic of all these turbine papers that he makes no use of mathematical calculations. In only one paper, which has no reference to the steam turbine, is a differential equation introduced, and it is relegated to an appendix. As Lord Rayleigh points out in the accompanying memoir, "it was in fact very curious how little use Parsons ever seemed to make of the formal mathematical training he had at Cambridge," where he was 11th Wrangler in the Mathematical Tripos. Parsons had the equipment, but the

conditions of the design of this small multistage unit did not necessitate any elaborate mathematical analysis, like those of the single-stage high-speed impulse machine developed, at the same time, by his distinguished Swedish contemporary, Dr. Gustav de Laval. He regarded the problem mainly as a mechanical one and found the experimental method sufficient for his purpose. He succeeded in producing a small and practicable machine, but it had the great drawback of excessive steam consumption. Convinced, however, of the soundness of his deductions he doggedly persevered, in spite of the prejudice and opposition of his confrères, who regarded the turbine simply as a fad and a voracious steam-eater. Parsons was a man possessed of vision as well as determination. He foresaw that this type of prime mover was the machine required for the generation of electrical energy on a large scale. His faith has been amply justified by the great increase of unit output in large power stations, especially in America, during the past twelve years.

It was unfortunate that the disagreement with his partners in 1889, led to his withdrawal from the Clarke Chapman firm and the subsequent suspension of his patent rights in the axial flow machine, for six years ; and as he puts it, in one of these papers, " the radial flow type of turbine was reluctantly adopted." He succeeded, however, in 1892, in demonstrating with this type, " that the condensing steam turbine was an exceptionally economical heat engine " ; and immediately on the recovery of the axial flow patent, he designed and produced the famous Elberfeld 1,200 kw. turbines, which attracted attention all over the world, and started Continental and American engineers on the development of the various types of alternative compound-impulse turbines. These two machines clearly demonstrated the superiority of the compound-reaction turbine for the efficient generation of electrical energy, and ensured the success which crowned his subsequent efforts.

Among the general public, however, Parsons' name is more associated with marine turbo-propulsion. His opinion on this subject, as given in one of these papers, is, " The most important field, however, for the steam turbine is in the propulsion of ships. The large and increasing amount of horse-power and the greater size and speed of the modern engines tends towards some form, which shall be light, capable of perfect balancing and economical in steam." The absorbing story of the evolution of the directly driven turbine installation is set out in this volume, with great clearness. It again brings out in relief the power of initiative and the resource of the man, combined with the courage to take risks attendant on large increases in turbine size, which led in thirteen

years to the phenomenal rise from 2,500 s.h.p. of the famous little *Turbinia*, to the 42,000 s.h.p. of the capital naval vessel and the 70,000 s.h.p. of the great Cunarders, *Mauretania* and *Lusitania*. This in itself would have been a notable achievement, arising from the initiative of one man; but Parsons had not yet reached the goal he had in view, the application of the turbine to all classes of naval and mercantile vessels. The directly connected turbine was too inefficient for vessels having speeds below fifteen knots. Gearing down from a small, light and efficient high-speed turbine to an efficient low-speed propeller was a necessity. From 1882, marine engineers had the tried helical reduction gear of de Laval at their disposal, but nobody moved in the matter. Again Parsons had the courage to act as pioneer, and with the assistance of his partners of the Parsons Marine Turbine Co., he expended £20,000 in testing the feasibility of helical reduction in the small cargo vessel *Vespasian* in 1909. An account of this installation, its successful operation and the material reduction in steam consumption obtained over that of the previous reciprocator, is given in one of these papers. This final contribution of Parsons started the era of geared turbines in all classes of vessels. Large and heavy direct-connected turbines are now things of the past. The great value of this work of Parsons in the marine domain to the nation was demonstrated during the war. The whole navy, with the exception of a few small vessels, was equipped with geared turbines; and the magnificent steaming of the fleet in those days of stress would not have been possible but for their high efficiency and reliability.

With regard to turbine practice in large power stations immediately preceding and after the war, which is dealt with in two of the later papers, it may be said that Parsons was not only the pioneer of the practicable reaction turbine, but also the initiator of nearly all the advances in turbine design, and in increase of unit size, which took place up till 1924. The 25,000 kw. turbine installed in Fisk St. Station, Chicago, in 1915 was, at that date, the largest in the world. Five years after its installation it was giving a higher performance than the original guarantee figure of Parsons. It was christened "Old Reliability" by the station staff, as it never let them down in an emergency. After the war, in 1924, he advanced the size to 50,000 kw. in the machine supplied to Crawford Avenue Station, New York. This machine had the enlarged steam cycle involving steam reheating, regenerative feed heating, with increased pressure and temperature limits, which has now become the general practice in power stations. Since that date great advances in turbine size have taken place in America,

to meet the enormous demand for electrical energy by the large cities, the world's record being held by the 200,000 kw. turbine in the State Line Station, Hammond, Indiana. Such development, however, may well be regarded as the natural sequence to the earlier and courageous advances made by Parsons, when other engineers hesitated to take the risks involved.

So much for the professional side of Parsons' activities, as outlined in these papers; but his breadth of outlook in the domain of physical science is amply demonstrated in the presidential addresses to various scientific societies, which are also included in this volume.

Despite the strenuous time he passed through in his early professional career, he managed to devote attention to some other interesting side-lines of scientific enquiry. His efforts to produce artificial diamond carried on during a period of thirty-five years, at a total expenditure of about £30,000, are fully recorded in this volume. The ultimate failure of this pet hobby must have been a bitter experience to him. In his other scientific interests he was more fortunate. The early association with his father in astronomical affairs aroused his keen interest in optical work, and in 1887 he developed a method of producing parabolic searchlight reflectors, at a fraction of the cost and weight of the reflectors then in use. For many years these reflectors have been made at the Heaton works. In his later years he applied his fertile mind to the improvement of the method of production of optical glass. He took over the control of the Derby Crown Glass Works, and, to quote the editor of this volume, "Applying scientific principles, adopted methods considered to be revolutionary. These, however, proved successful and achieved his purpose." He is said to have expended £60,000 on the improvements in the manufacture of optical glass. An account of these activities is given in Appendix B. A final word may be added on another hobby which afforded him a welcome relaxation in the midst of his turbine problems. This was the auxetophone described and illustrated in Appendix A. The article gives an extremely interesting account of Parsons' progress, from the analytical investigation of the acoustic problem to the practical embodiment of the theory, and details the great amount of handicraft work he expended on it, and the progress until it was successfully applied to the cello and double bass instruments. It was, however, too clumsy for attachment to the violin, and did not give satisfactory performance when applied to the piano and harp. Where however applicable, it gave thirty years ago as good sound amplification as the modern wireless loud-speaker. On the advent

of wireless broadcasting in 1922 it was tried experimentally as a loud-speaker. Parsons was much interested in its performance, but as his patent rights had long expired, this interest was of a purely sentimental nature.

Lord Rayleigh's memoir adds a welcome personal touch to this notable collection of papers and addresses, which is worthy of a place, not only on the bookshelves of the scientist and engineer, but of all those who are interested in the history and progress of scientific invention.

REVIEWS

MATHEMATICS

Lectures on Matrices. By J. H. M. Wedderburn. [Pp. vii + 200.] (New York: The American Mathematical Society, 1934. $3.00.)

The origin of matrices is to be found in Hamilton's Lectures on quaternions, which appeared about eighty years ago. In the succeeding fifty years notable contributions to the subject have been made by many mathematicians, but it is only during the present century that the importance of the theory in diverse branches of applied mathematics has been appreciated. This book, which is Vol. XVII of the Colloquium publications of the American Mathematical Society, is based on lectures given at Princeton University on various occasions since 1920, and is primarily addressed to the pure mathematician. It is clearly and concisely written, and the text gives a fair indication of the directions in which the theory has chiefly developed. These qualities should make the book of value to applied physicists, engineers, statisticians and others who desire to utilise matrices in their work, in addition to those interested in the subject as a branch of algebra. Particular features of the treatment are the extensive use made of unit matrices (one co-ordinate with the value 1, the rest all 0) and of the scalar product of vectors. Many of the more difficult results are reached with remarkable ease. Among the subjects dealt with in the latter half of the book are commutative matrices, functions of matrices, and the automorphic transformations of bilinear forms. A final chapter deals with the classification of algebras.

The book is provided with an excellent index, and with a bibliography which is the more valuable for being critically selected. Incidentally it displays well the historical development of the subject. In another short appendix the author indicates the writers whom he is largely following in the various chapters.

The treatment is confined to square matrices of finite order. The only fault to be found with the printing is that the small italics used as suffixes, etc., are not as clear as could be desired. A few errors in the text may be attributed to this cause. A few other errors remain, some of which may trouble a reader new to the subject. Had space permitted a slight lengthening of the text and the inclusion of more illustrative examples these faults would have been of little importance. However, the student who solves these difficulties for himself will be amply rewarded.

T. S.

The Calculus of Finite Differences. By L. M. Milne-Thomson, M.A., F.R.S.E. [Pp. xxiii + 558, with 23 figures.] (London: Macmillan & Co., Ltd., 1933. 30*s*. net.)

The need for a text-book on subjects associated with finite differences will be readily conceded; the English reader has virtually only one source of collected

information, namely Whittaker and Robinson's *The Calculus of Observations*. The book is heterogeneous—highly theoretical matter interspersed with attempts to bring theory to the level of practice. Judged from the practical view-point, it can only be described as disappointing; theory is here the master, not the handmaiden.

The book opens with one of the most difficult and least used aspects of the subject, namely divided differences, which every good computer tries to avoid. The second chapter plunges into difference operators, which, combined with the fact that series are usually written in the Σ form, and rarely with the first few terms shown in full, serve to obscure any points that a practical reader might otherwise glean.

The third and fourth chapters cover interpolation and subtabulation. Under the heading "Modified Bessel's Formula" is given, without mentioning the name, the "throw-back," now established by the *British Association Mathematical Tables*, which enables higher order differences in many cases to be virtually dispensed with. The principle by which the coefficients for the throw-back are determined is that the greatest positive error should equal numerically the greatest negative error; they are here determined by making the mean error vanish. Perhaps this accounts for the omission of the first and important case where the fourth difference is thrown back to the second; the coefficient that would be derived from this theory is $-0{\cdot}1833$, as compared with the true coefficient of $-0{\cdot}1839$.

The reader is left vague (p. 90) as to the limits of permissible fluctuation in oscillating differences. A theoretical limit of ± 32 is derived for sixth differences; the practical limit, which is much less, is not discussed. Chapter V introduces the English reader to the reciprocal differences and interpolation formulæ of Thiele, but without demonstrating their practical utility, as no skilled computer would do the example given, namely the interpolation of $\tan x$ near $90°$, either by this process or by differences.

After an exposition of the polynomials of Bernoulli and Euler the important subjects of numerical differentiation and integration are reached. We note the absence of any reference to the classical work of von Oppolzer, or Kowalewski's *Interpolation und Genäherte Quadratur* (Leipzig, 1932). Throughout the remainder term has been unduly stressed; the climax to this theoretical one-sidedness comes on p. 162, "It will be seen that the complicated form of the remainder term may often render the use of Markoff's formulæ preferable to those with central differences." Practical computers often go through life without hearing the expression "remainder term."

Chapter VIII, on the summation problem, gives an English account of Nörlund's work on the "principal solution" of a difference equation of the first order. After chapters on the psi and gamma functions and factorial series, the remainder of the book is devoted to difference equations.

The author's diversity of practice, in sometimes attributing methods to contemporary authors with full reference and names, and at other times incorporating work without either name or reference, might easily mislead the reader as to the true authorship of the methods. An equivocal example is the inclusion (pp. 91–4) of the so-called "end-figure" method of subtabulation, first published in *Monthly Notices of the Royal Astronomical Society*, **88**, 512–18 (April 1928). The inspiration for the paragraph "Modified Bessel's Formula," already discussed, will be found on p. 515 of the same paper. The paragraph

on p. 94 commencing "Another method of subtabulation . . ." is based on another paper in *M.N.R.A.S.*, **92,** 537–38 (April 1932). The method of checking tables by differencing the end figure and building up (p. 91) was put forward on p. 539 of the same paper

L. J. C.

Five Place Table of Natural Trigonometric Functions to Hundredths of a Degree. Compiled by Amelia de Lella. [Pp. 52.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 6*s*. net.)

The opening paragraph of the Preface of this volume says "This table has been compiled in response to a frequently expressed desire on the part of engineers for a five place table of natural trigonometric functions of angles expressed in degrees and hundredths of a degree." Such a desire has been met perfectly for the last 25 years by Lohse's *Tafeln für numerisches Rechnen mit Maschinen* (Leipzig, Engelmann, 1909), an accurate and well-printed table giving the natural values of the six trigonometrical functions at intervals of 0°·01, with their differences.

This table does not possess either of the two virtues just mentioned. It has been compiled from Brigg's *Trigonometria Britannica* (not Brittanica, as given in the Preface), but with the inclusion of three gross errors (tan 1°·81, sin 18°·24 and cos 42°·09), and thirteen errors of a unit in the last decimal. These errors have all been introduced by the compiler; the thirteen errors of a unit are due to rounding-up when the rejected figures lie between 495 . . . and 499 . . .

The arrangement is as inconvenient as it could possibly be. Successive values are in rows instead of being in columns, while the four functions are jumbled in a way that can best be shown by illustration.

			18°			
		·00	·01	·09		
18·0°	sin	·30902	·30918	·31051	·31068	cos
	cos	·95106	·95100	·95057	·95052	sin
	tan	·32492	·32511	·32666	·32685	cot
	cot	3·0777	3·0759	3·0613	3·0595	tan
	sin	·31068	·31084	·31217	·31233	cos
	cos	·95052	·95046	·95003	·94997	sin
	tan	·32685	·32704	·32859	·32878	cot
	cot	3·0595	3·0577	3·0433	3·0415	tan
			. . .	. . .	. . .	
			·09	·01	·00	
			71°			

The copy has been prepared on an ordinary typewriter, and reproduced by photo-lithography, so that the appearance is anything but pleasant. The things we welcome in the book are the implied indications that engineers are appreciating the decimal division of the degree and the advantages of calculating machines.

L. J. C.

ASTRONOMY

General Astronomy. By H. Spencer Jones, M.A., Sc.D., F.R.S. Second edition. [Pp. viii + 437, with 28 plates and 113 figures.] (London: Edward Arnold & Co., 1934. 12*s.* 6*d.* net.)

The appearance of a second edition of the Astronomer Royal's book provides an excellent opportunity for the stock-taking of our astronomical knowledge and theories. An examination of the many additions to the first edition enables us to see where and how far the line of our battle-front against nescience has advanced in the last twelve years or where a change of emphasis suggests that a further advance is in progress or in view. The fact that the proportion of the volume devoted to the stars, as distinct from the solar system, rises from one-quarter to one-third is symptomatic of the changing outlook of astronomy. The expansion, to allow of the admission of much new matter in the present edition, has been effected by the very doubtful expedient of using smaller type. The next edition—and other editions are sure to follow—will call either for drastic excision (*e.g.* of the chapter on Astronomical Instruments) or for two volumes, unless the new knowledge of the coming years is to be described in a supplementary volume.

A few notes must be given on the additions to the book. In the case of the Earth we note the section on the Age of the Earth and the detailed account of the secular and irregular changes in the Earth's rotation. Recent observational work on the Moon is reflected in the discussion of its rills and rays while recent theories of the origin of lunar craters are given. Typical of a number of changes in the book is the statement, "It is certain also that there is no water on the Moon"; the earlier volume used the word "probable." The fresh notes on the planets refer mainly to their albedo, their atmospheres, their surface temperatures and their periods of rotation. On all these subjects recent work has been done, while the photography of the planets in ultra-violet and infra-red light has added to our understanding of their nature. Pluto represents a new planetary discovery since the first edition was published: while the number of asteroids known has increased from under 1000 to over 1200. Among the new asteroids mention is especially made of our two near neighbours Amor and 1932 HA. Turning to the Sun we may note the accounts of Bjerknes' theory of sunspots, of modern views of the chromospheric and other layers of the solar atmosphere, the discussion of the effective temperature of the Sun and the maintenance of its heat output. The astronomical applications of the theory of relativity are also introduced.

But the additions to the chapters on the Stars are much more striking and important—some of the chapters being practically rewritten. Here the author must have found it very difficult to select from among the mass of new material what should be published at this stage. The reviewer feels that at least as a signpost pointing along an important path into the realm of future work, some reference could have been made to recent work on spectral line-intensities and the application to such problems as are discussed in Professor H. N. Russell's Halley Lecture of 1933. But if everything that a critic looks for is not present there is a great mass of new material in the stellar chapters. Only a few items can be mentioned here but they will suffice to show the present range of the book. Stellar diameters, stellar rotations, the mass-luminosity law, the rotation of the Galaxy and some aspect of the asymmetry of stellar motions, the distances and recession of the spiral nebulæ, the

expansion of the Universe, white dwarfs, the loss of mass by radiation and the ages of the Stars and of the Universe, all represent work of the last twelve years and figure only in the new edition. The accounts of variable stars and novæ, of binaries, of nebulæ (both galactic and extra-galactic) of clusters, of stellar interiors and of cosmogonical theories are greatly expanded and brought up to date. Fresh figures recently determined by more accurate methods replace those given in the earlier edition.

In many cases there is to be noted a replacement of hesitation by certainty. In one case—the discussion of stellar evolution—the move is in the other direction; theory is in a state of flux and the comparative certainty with which the older theories were successively held have vanished. For the most part the Astronomer Royal states his views of current theories with confidence and conviction. Some carry within them no doubt, as good theories in a progressive science should, the seeds of their own destruction: a later generation of theories will spring from them and in due course replace them. Later editions of the work under review will show changes, which we may confidently hope will be as skilfully and wisely made as those in the present volume.

F. J. M. S.

Through Space and Time. By SIR JAMES JEANS, M.A., D.Sc., Sc.D., LL.D., F.R.S. [Pp. xiv + 224, with 53 plates.] (Cambridge: at the University Press, 1934. 8*s*. 6*d*. net.)

THE Royal Institution lectures "adopted to a juvenile auditory" delivered by Sir James Jeans at Christmas, 1933, are here presented in book form, and are nearly as fascinating to read as they were to hear. The journey through space takes the traveller to the remotest nebulæ which have yet recorded their existence on a photographic plate, and he is duly informed that not only is space inconceivably large but it is continually expanding. The journey through time traces the history of the earth from its birthplace in the sun, its gradual cooling into its present shape, the earliest forms of life, the age of the fearsome and gigantic reptiles, and finally the appearance and development of man. Answers are given to such diverse questions as How Saturn got his rings; What earthquakes tell us about the interior of the earth, and radio waves about the upper atmosphere; What the scenery on the moon is like; Whether life exists in Mars; How the sun maintains its heat; How meteors sometimes strike the earth and what havoc they can produce; What is the number of the stars. These are merely a few examples, and all are answered in so clear and illuminating a way as to ensure the interest of all "juveniles" of whatever age.

The illustrations are numerous, well chosen, and excellently reproduced, while an index has also been included.

As the circulation reached the sixth thousand within ten days of publication the record established by some of the author's previous books seems likely to be endangered. It is no small feat to cover so wide a field of astronomy and geophysics so attractively and so intelligibly. The book can be unreservedly recommended as a present for any "juvenile" who shows an inquiring interest in science.

R. W. W.

PHYSICS

Theoretical Physics. By Georg Joos, Professor of Physics at the University of Jena. Translated by Ira M. Freeman, Ph.D., Chicago. [Pp. xxiv + 748, with 3 plates and numerous figures.] (London and Glasgow: Blackie & Son, Ltd., 1934. 25*s*. net.)

In his preface to this work on Theoretical Physics the author describes it as a guide book to readily accessible territory from which the partially explored and more difficult country may be seen and the visible peaks attained.

A guide book to the well-established territory of physics must surely be equivalent in size to many volumes of Baedeker and with this book before one, with about 700 pages it is true, one may well wonder if too much has been undertaken for one volume.

The table of contents is a large and comprehensive one, covering the subjects which, in this country, we regard as appropriate to an honours course and in some cases going beyond it. The book is arranged in seven parts, beginning with a mathematical introduction on vectors, the representation of vibrations and waves and on the use of the complex variable. This is very clear and very useful and helpful. Six chapters are given up to mechanics, one of which is devoted to generalised dynamics and the last gives as much of the special theory of relativity as has come to be regarded as the necessary equipment of the physics student.

The electromagnetic theory is described in two parts, the field and the atomistic theories. The former includes a treatment of optical phenomena, the propagation of light in crystals and diffraction.

Geometrical optics is placed in the last chapter of this part and has only a scant treatment of about ten pages. The presentation is by means of the collinear relationships. It is too much to expect adequate treatment of all branches in a work of this nature and one can only regret that there is not space for the presentation of this subject, which is usually badly taught and much neglected.

The theory of heat consists chiefly of thermodynamics and statistics with an introduction to the new statistical theories of Bose–Einstein and Fermi-Dirac. The account given can be presented to students as easily as the classical theories and the writer has made the subject easier by giving a concrete example.

The last chapters are a brief interesting introduction to some of the modern developments from which, no doubt, many teachers of physics will receive their first impressions of the book. Some of the subjects introduced here are the Uncertainty Principle, the Wave Equation and Nuclear Physics.

When one remembers that the author is a German physicist and that the book was translated by an American it seems remarkable that it is so exactly suited to the needs of the English University student. The notation is well chosen and is that which is now very generally adopted in original papers. It will make advanced papers more accessible to students and it would be an advantage if teachers in this country could be persuaded to adopt it. The translation is excellent and there is little, if anything, to show that the original is in another language. We congratulate the translator on the final decision to retain the particular form of vector notation employed in the original, for if it is not universally adopted, it is, at any rate, universally understood. The work is one which teachers will find very useful. Students

will find the argument clear and will be helped in their difficulties by making use of the examples and their solutions which the author uses to assist in teaching and to supplement the material.

H. T. F.

The Physical Basis of Things. By J. A. ELDRIDGE. International Series in Physics. [Pp. xiv + 407, with frontispiece and 136 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 22*s*. 6*d*. net.)

THIS is an exciting book. It has been the author's aim to make it exciting ("something of the story must be lived through by the student") to the average American undergraduate in his second year of College. Naturally, a first year course in general physics is presupposed, but the aim is "to give appreciation of the meaning of modern physics," rather than to impart information. Books of this scope, addressed to future examinees—there are ten pages of questions at the end of the text—are not as yet common in this country; they are peculiarly a product of the American system of education. Nevertheless, this particular product may have an export value, if only because, in this case, the attempt to make physics intellectually exciting has so far succeeded. The reader is given curiosity-provoking glances into relativity theory, the kinetic theory of matter, the fields of optical, X-ray and infra-red spectrometry and nuclear physics; whilst, finally, a brief encounter with the ideas of the new quantum mechanics completes his survey. By way of diversion he is provided with portraits (and word-pictures) of five of the leading scientists of the day and full-page quotations from *The Torchbearers* and *Cavendish Post-prandial Proceedings*. For his encouragement, maybe, the names of Nobel Laureates in Physics (and, occasionally, in Chemistry) are given in an appendix—together with an indication of the cash value of the prize.

However, excitement is not for everyone; those whose concern with physics is likely to be more fundamental will find fascination enough in a presentation of the subject where trains of logical reasoning are less fragmentary. Moreover, such students will require facts. Here there is no dearth of facts, despite the disclaimer, but sometimes complete accuracy is lacking. Table 52 contains thirteen separate inconsistencies—mostly arithmetical.

N. F.

Atomic Structure and Spectral Lines. By ARNOLD SOMMERFELD. Translated by H. L. BROSE. Vol. I. Third edition. [Pp. xi + 675, with 151 figures.] (London: Methuen & Co., Ltd., 1934. 35*s*. net.)

THIS is a translation of the fifth edition (1931) of *Atombau und Spektrallinien*, which first appeared in 1919. Progress has been so rapid since that time that considerable changes in treatment and numerous additions to the matter were called for, and Prof. Sommerfeld found it necessary not only largely to recast the work but also to divide it into two volumes. The first, now under review, is an expanded and up-to-date version of the original "Atombau," omitting all detailed wave mechanics theory, which is reserved for Vol. II. Although this precludes a strictly logical and comprehensive development of the subject, the author's plan has much to commend it, and will probably prove acceptable to the majority of his readers, who will be disposed to agree with him that

"it is possible to understand the new theory only by building it up from the old theory."

Some indication of the importance of the new matter may be afforded by reference to a few of the items, such as the electron spin treatment of multiplet structure, the Pauli principle, the Compton effect, line intensities and hyperfine structure. This last, contributed by Dr. Gwynne Jones, and the section on band spectra, might justifiably have been expanded somewhat, but one can sympathise with the author's desire to keep the size of the volume within reasonable limits. As it is, there are some fifty pages more than in the first English edition, but greater compactness has been secured by the use of thinner paper. (The very slight saving effected by cutting down the index was definitely not worth while, however.) The translation is fully up to the high standard previously set and does full justice to Prof. Sommerfeld's lucidity and mastery of exposition. It is very occasionally indeed that one can sense the original German, as in the use of terms such as "bands shaded (*abschattiert*) to the red" and the "many-lines (*viellinien*) spectrum of hydrogen," for which well-established English equivalents are available. The revision has been so thoroughly done, too, that internal evidence of the very respectable age of the work is almost entirely lacking, with one notable exception, namely the retention of the $v'J'$ . . ., vJ . . . notation to indicate upper and lower electronic states of band systems instead of that now universally employed by spectroscopists, namely $v'J'$ . . ., $v''J''$ . . . The single appearance of the latter notation in the intensity formula (p. 592) is obviously an oversight.

A few of the outstanding discoveries of the last few years, for example in connection with cosmic rays, neutrons, and heavy hydrogen, are briefly described in a note by the translator.

W. E. C.

A Study of Crystal Structure and its Applications. By WHEELER P. DAVEY, Ph.D., Research Professor of Physics and Chemistry, School of Chemistry and Physics, The Pennsylvania State College. [Pp. xi + 695, with 255 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 45*s.* net.)

THIS book is one of the International Series in Physics edited by Prof. F. K. Richtmyer. It is a comprehensive review of the whole field of the analysis of crystal structure by X-rays and of the significance of the results. The author states in the preface that it is written primarily for men who have the maturity and training to be expected of a college graduate, and that its purpose is to put such a person in possession of enough knowledge of the theory and technique of crystal analysis to enable him to read the literature intelligently and to do independent work. It does not attempt to be an encyclopedia of the theory, nor is it a summary of data.

The literature of this subject has now assumed enormous proportions, and the rate of advance is very rapid. The author has indeed set himself a herculean task in attempting to write a book of this length, for it is almost impossible for any one man to make himself acquainted with the latest work in all fields, and at the same time by personal investigation and experience to acquire the sense of proportion which enables him to sift the wheat from the chaff and direct the attention of students to methods which have proved best in practice and conceptions which have proved to be the most productive of

further investigation. The chief criticism which may be made of the book is that much of the chaff has been retained in the sifting process and that a good deal of the wheat has been lost. A new subject such as crystal analysis passes through a stage where tentative hypotheses are made in order to summarise results and suggest further work. These hypotheses are admittedly imperfect and crude, but serve a most useful purpose at the time. When they have been replaced by a theory on more secure foundations, it is best to dismantle the old scaffolding and allow these gropings towards the truth to be forgotten altogether except for brief reference in an historical review. In the present work, they appear to the reviewer to play too large a part in a student's introduction to the subject.

The chapters on packing shapes of atoms, and on the calculations of lattice energies, may be cited as examples. The viewpoint adopted is of a very "classical" type with much discussion of static models, and as far as they are concerned the revelations of quantum mechanics might never have been made. When the concession of a reference to the quantum theory is made, it is to the original Bohr theory with its definite electron orbits. On the experimental side, the chapter on the powder method describes for the most part the use of molybdenum K radiation for this purpose, and the author gives the impression that an accuracy of one part in one thousand is a reasonable ambition. The use of the short Mo-rays has been abandoned just because the attainable accuracy is so poor, and one of the outstanding features of modern work is the use of a series of longer radiations and the consequent increase of accuracy towards 1 part in 50,000 by the back reflection method in cases where the crystalline material is sufficiently perfect.

The author is conspicuously fair and generous when describing the work of others, and the book contains many illuminating and suggestive ideas. One cannot help feeling, however, that it has been compiled from such sources as happened to be conveniently available, some of them rather out of date, and that it has been impossible for the author to undertake the wide review of literature in several languages which is desirable for a treatise of this scope.

W. L. B.

CHEMISTRY

Handbuch der Anorganischen Chemie. By R. Abegg, Fr. Auerbach and I. Koppel. Vol. IV, Pt. 3, Sec. 3, Sub-sec. 1: **Cobalt and its compounds.** [Pp. xvi + 626, with 170 figures.] (Leipzig: Verlag von S. Hirzel, 1934. RM.58.)

The latest edition of Abegg's famous *Handbuch* majestically approaches its conclusion with the volume covering part of the chemistry of cobalt. One wishes that a less complicated system of naming the various parts had been adopted or that there were more synonyms in English to translate Vierter Band, Dritte Abteilung, dritter Teil, Lieferung I; as each sub-sub-sub-part runs to some 500–600 quarto pages and there are 15 so far published, to describe the work as in four volumes implies a type of binding which would trouble the book-binder and impose considerable physical exertion on the user. Apart from this criticism there is little one can say on so masterly a production except to point out that the treatment, for which the earlier edition was so appreciated, has been retained, namely the physical chemistry of the element and its compounds has been kept to the fore.

The present part deals with the atomic weight of cobalt, the cobalt atom, metallic cobalt, compounds of bivalent cobalt, ammines of bivalent cobalt, compounds of tervalent cobalt, compounds with the metalloids of groups one to six and with metals and the colloid chemistry of cobalt. The ammines of tervalent cobalt are yet to come. The value of the work as a reference book is too well known for it to need further recommendation.

O. L. B.

Technical Gas Analysis. By G. LUNGE, Ph.D., Dr. ING. (H. C.) Revised and rewritten by H. R. AMBLER, Ph.D., F.I.C. [Pp. xvi + 416, with 138 figures.] (London: Gurney & Jackson, 1934. 21*s*. net.)

THIS volume retains a good deal of Lunge's original work in a shorter and revised form, but contains so much fresh material as to become practically a new work. Very early (p. 5) a warning is given of the possible toxicity of mercury vapour, a subject of serious import to all users of gas apparatus. A feature of the work is the fact that working details are in general sufficient for immediate use and references to accessible works are given in those cases where the analysis of a gas or the determination of a constituent is but rarely required. Sampling and measurement of gas volumes and pressures, as well as the determination of densities and descriptions of apparatus required, receive full treatment.

It is curious to note that with but comparatively small modifications the older types of apparatus such as that of Orsat and Hempel and the Lunge nitrometer, still keep their places as the best working instruments. While the description and diagram of Dennis pipette are valuable, it is not so certain that so many illustrations of modifications of Orsat's apparatus (Figs. 38, 40 and 41) need have been given. Dr. Ambler's own contributions to gas analysis are described in moderate detail but for some reason Fig. 59 showing the main features of the Ambler apparatus is badly lettered. Nevertheless, it is a definite advantage to have the descriptions of the Ambler apparatus, and that of Chamberlin-Newitt for small quantities of gases, brought together under one cover. It is to be emphasised that with these types of apparatus gas analysis can be made as conveniently and accurately (if not more so) with 1 or 2 ml. of gas, as with the older types requiring considerably more gas.

There is an excellent discussion of absorption methods for determining oxygen and of the various procedures for carbon monoxide, and fractional combustion methods also receive due attention. Some forty pages are devoted to physical methods such as density, viscosity, refractivity, thermal conductivity and fractional condensation. In general, these physical methods are likely to be limited to binary mixtures or to mixtures of gases in which one constituent alone is liable to variation.

A large part of the work is rightly devoted to individual gases. In this section it is shown that the determination of nitrogen can be made directly without too great difficulty, Ambler's apparatus being particularly suitable for the purpose. The section dealing with the calorific value of coal-gas is very short and largely a description of various types of apparatus. It has, however, a brief discussion of the calculation of the calorific value from the composition of the gas.

Part is devoted to flue gases and gases of chemical industry, the usual methods (automatic and others) with some variations, being described fairly fully. The well-known Haldane apparatus and modifications thereof receive

due consideration, perhaps in a rather condensed form. The book is well printed in bold type, is well indexed and is free from misprints. A few useful tables are incorporated, one devoted to physical constants of gases being given twice (pp. 206, 207 and 388, 389) for some reason which is not obvious. Dr. Ambler has succeeded in producing an up-to-date practical work which will surely be found in the libraries of all laboratories engaged in gas analysis. The printing and paper are so good that they form a distinct contrast to the general appearance of the cover.

J. J. F.

Crystals and the Polarising Microscope. A Handbook for Chemists and others. By N. H. HARTSHORNE, Ph.D., M.Sc., and A. STUART, M.Sc., F.G.S. (Pp. viii + 272, with 217 figures.) (London: Edward Arnold & Co., 1934. 16s. net.)

ALL chemists are concerned with matter in the crystalline condition; with the preparation, identification and characterisation of crystalline specimens: yet it must be admitted that few of them have a working knowledge of crystallography and of the way in which the polarising microscope can be used to increase the accuracy of their observations.

By the examination of the external shape and the optical properties of crystals under the microscope the laboratory worker can attain much greater precision when characterising a specimen, identifying one substance with another or discriminating between two similar crystalline samples, and he thus saves himself and others much time and trouble. In dealing with substances for which some other customary criterion, such as the melting-point, fails or gives uncertain results such methods may be of exceptional value, and it will be obvious that there is an immense practical advantage in a device the use of which often makes it possible to identify components of solid mixtures without separating them.

There can be no doubt that these methods would prove useful in all laboratories and that in certain classes of problem, such as the study of heterogeneous equilibria, serious error may arise from the omission of this type of observation.

Hitherto there has been no English work on this subject designed to meet the needs of the chemist as well as the geologist. The authors of the book now under review are therefore to be congratulated on having brought together in one volume all that is necessary to enable the chemical reader to add the technique of crystal microscopy to his repertoire of laboratory methods.

In the first three chapters a general account is given of crystallisation, and of the internal structure, external form and optical properties of crystals. The microscope and its use in the examination of crystals are described in the next three chapters, which form the kernel of the book. The two concluding chapters are of special chemical interest: a remarkable number of examples are here cited to illustrate the use which has already been made of the polarising microscope in a wide variety of published research in all the main branches of chemistry. The final section outlines the method of procedure to be adopted in examining a specimen, and a few cases are described in detail.

This book should do much to extend the use of the microscope in chemical laboratories of all kinds. It is not only a scholarly work, but at the same time a thoroughly practical book. The summaries appended to some of the chapters will prove very useful. Similar summaries derived from the chapters

on optical examination will no doubt be drawn up by any reader who makes serious use of the book. The authors might nevertheless consider the desirability in a future edition of adding further summaries and in particular a general determinative table to embrace simultaneously observations of both external shape and optical properties.

The production of the book is excellent and the price is not excessive in view of its special character and the large number of diagrams which are an essential feature.

G. M. B.

Physical Chemistry. By A. J. Mee, M.A., B.Sc. [Pp. xx + 780, with 208 figures and 5 plates.] (London: William Heinemann, Ltd., 1934. 15s.)

One of the most striking features of modern science is the deep interpenetration of physics and chemistry so that the boundary between the two sciences is almost obliterated. It was never more necessary than to-day for every chemist to be something of a physicist, and, it might be added, for every physicist to be something of a chemist.

The book under review illustrates this tendency for it is concerned with a far wider range of subjects than those which would have been included in a textbook of physical chemistry thirty years ago. Mr. Mee is to be congratulated on the large measure of success with which he has attained his object in writing "an up-to-date outline of the results and methods of Physical Chemistry." The topics usually included under this heading are adequately dealt with, whilst more recent work is incorporated in the form of clear accounts of such subjects as atomic structure, heavy hydrogen, nuclear disintegration, dipole moments, etc. A valuable feature is the provision of descriptions of underlying physical methods and principles which are elucidated by the aid of a number of simple diagrams.

A few errors may be mentioned which can readily be corrected in later editions. The diagram on p. 490 does not show Cottrell's apparatus but a highly simplified form of it. On pp. 613, 614, hydrogen peroxide is described as an endothermic compound; it is, of course, exothermic with a heat of formation from its elements of about + 47,000 cals. The value − 23,000 cals. given on p. 614 is the heat of formation from water and oxygen.

This book is primarily intended for the use of students up to B.Sc. Pass standard; it should however be of interest to a wider circle of chemists who wish to keep in touch with modern developments in physical chemistry.

S. S.

Electrolytes. By H. Falkenhagen. Translated by R. P. Bell. The International Series of Monographs on Physics. [Pp. xvi + 346, with 105 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. 25s. net.)

The development of the so-called "modern theory" of electrolytes has been so rapid and has involved mathematical treatment so far from elementary and the introduction of so many subsidiary hypotheses, often seemingly in conflict with one another, that the average chemist sighs for the good old days of the classical theory before the mathematical physicist came upon the scene, when a sodium chloride solution (at least, if it were very dilute) seemed a relatively simple system. But those days are gone, and there is now a danger that the

chemist, who has contributed so largely to the experimental development of the subject, may be afflicted with an inferiority complex and be inclined not only to accept unquestioningly the correctness of the mathematical operations but even to omit to examine with due care the nature and validity of the assumptions upon which the modern theory rests and the extent to which its conclusions are actually supported by experimental facts. Hans Falkenhagen's *Elektrolyte* which appeared in 1932 has proved of immense value because it not only sets forth in a clear and logical fashion the deduction of the principal equations of the theory of interionic forces in relation to the properties of strong electrolytes, but also includes a proper statement of the basic assumptions and limitations of the theory and a critical discussion and correlation of the various subsidiary theories which link up with the classical view by making allowance (or by avoiding the necessity for making any special allowance) for the existence of undissociated molecules, ion pairs, or complex ions. Moreover, a useful account is given of the recent experimental work on the dependence of conductivity on field strength and frequency. The treatment of the whole subject is such that whether the reader follows the details of the mathematical operations or not, he is able to visualise at least the essential outlines of a general theory which already goes a long way towards accounting for the properties of all types of electrolyte—strong and weak, simple and complex, aqueous and non-aqueous—provided that they are reasonably dilute.

Our gratitude to Dr. Falkenhagen must now be extended in no inconsiderable measure to Mr. R. P. Bell who has not only produced an excellent translation of the German text, but has, in consultation with the author, revised it in the light of advances which have been made during the last two years. This revision has involved the introduction of new experimental results with the corresponding diagrams and references, as well as some extension on the theoretical side. Some condensation has been effected, however, in other places, and one small sub-section of the original and the Author Index have been omitted, with the result that there is no appreciable increase in bulk. Very few errors appear to have escaped correction: it may be noted, however, that on p. 206 *acetonitrile* should read *benzonitrile*. An English version of Debye's admirable foreword has been included, and an interesting appendix on recent applications of quantum mechanics to the theory of electrolytes by Prof. R. H. Fowler, one of the general editors of this series of monographs, has been added. Since this appendix deals, however, mainly with electrode equilibria and reactions—subjects which lie entirely outside the scope of the book itself and have an extensive literature of their own—the desirability of including it in this place may be questioned. The printing, binding and general set-up of the book are worthy of the very high reputation of the publishers in these matters.

H. J. T. E.

The Electronic Theory of Chemistry. An Introductory Account. By R. F. Hunter, D.Sc., Ph.D., D.I.C., A.R.C.S. [Pp. vi + 125.] (London: Edward Arnold & Co., 1934. 8*s*. 6*d*. net.)

This book contains in extended form the substance of a course of lectures delivered by the author at the Muslim University, Aligarh. The first seven chapters are devoted to a general account of the modern electronic theory of valency, followed by five chapters describing the application of this theory to

the interpretation of various phenomena of organic chemistry. In the opening section the general development of the subject largely follows that of Sidgwick's classical work. The treatment is naturally compressed as the whole book only occupies 125 pages, but a fair idea of the essentials is conveyed. Some repetition could have been avoided ; thus, the last paragraph of p. 17 (continued on p. 18) is repeated almost exactly on p. 41. The space saved could have been devoted to a more detailed treatment of co-ordination, association and solvation, for Chaps. III and IV are somewhat hurried.

The chief criticism must, however, be directed against the chapters dealing with the application of the electronic theory to organic chemistry. It was to be hoped that any text-book for post-graduate students on this subject (particularly one with the title adopted by the author) would contain a broad general discussion of the mechanism of simple organic reactions. The author has preferred to plunge *in medias res* into the comparatively complicated subject of aromatic substitution. Moreover, he has confined his attention largely to the work of one school, without a balanced discussion of the development of the various topics or of the views of the various authorities. Thus it is something of a shock to find that the chapter on the "Theory of Tautomerism" contains no mention of the developments due to Lapworth, Wislicenus and Lowry. As against this, some of the theoretical matter which is included is speculative and little indication is given that, as a leading authority has recently said, "Our theories are still only 'roughly qualitative.'"

R. P. L.

Soil Analysis. A Handbook of Physical and Chemical Methods. By C. HAROLD WRIGHT, M.A., F.I.C. [Pp. viii + 236, with 6 figures.] (London : Thomas Murby & Co. ; New York : D. van Nostrand Co., Inc., 1934. 12*s*. 6*d*. net.)

THERE is a great deal more involved in compiling a handbook of soil analysis than might at first sight appear. That is due to the fact that with a rapidly developing knowledge of the constitution of the soil new methods are continually being suggested and comparatively few of the present methods have stood the test of any long period of time. It is therefore all the more necessary that the methods which are advocated with responsibility and the methods which are finding increasing favour should be collected together with full working details from the diffuse literature of the subject.

Mr. Wright has collected these methods together with discrimination and produced a compilation which will be very useful in our soil laboratories.

One cannot help thinking that more might have been said about qualitative tests and about the lime requirement methods, which although arbitrary have played, and for some time will continue to play, a very useful part both in the development of our conception of soil constitution and in our service to the husbandman.

It is perhaps unavoidable that such a book, which is intended mainly as a laboratory handbook, should be categorical, but one would like to see in future editions an expansion of the introduction and a fuller discussion on the sampling of soils. It would add value to the book too, if more could be included about the historical developments of soil analysis. Particularly is this so in connection with mechanical analysis, since for some time to come we shall

have to deal with mechanical analyses made by older methods and some knowledge of their meaning and degree of reliability is desirable. These are suggestions rather than criticisms, for Mr. Wright has given us a compilation for which we are very grateful.

N. M. C.

The Natural Organic Tannins. History: Chemistry: Distribution. By M. Nierenstein, D.Sc. [Pp. viii + 319.] (London: J. & A. Churchill, Ltd., 1934. 21*s*. net.)

Of all the natural products of commercial importance the tannins have, perhaps, made the smallest appeal to the organic chemist and it is to be hoped that the appearance of this book will stimulate interest in a subject in which there is much work to be done. After giving an account of the characteristics and classification of the tannins, illuminated with considerable historic lore, the early history of Cutch is described followed by a study of catechin. The structure of this compound has long been a matter of acute controversy and it would be impertinent for anyone who has not worked in this field to decide between the protagonists, so all the reviewer feels competent to do is to congratulate the author on the fairness with which he has put his own case and that of Freudenberg. The hydrolysable tannins are fully dealt with and there follows a chapter on the unclassified tannins about which so little is known. Finally there is a useful chapter on the Botany of the tannins by Dr. Skene.

The book is clearly written and very thoroughly documented, indeed the footnotes and references are so numerous as to make reading somewhat difficult at times. There is much historical information dating back to classical times which, though it may not appeal to the technician, adds interest for the scholar. The excellent subject index, author index and plant index add considerably to the value of the book.

O. L. B.

An Introduction to Practical Organic Chemistry. By W. A. Waters, M.A., Ph.D. [Pp. vii + 92, with 19 figures.] (London: Edward Arnold & Co., 1934. 3*s*. 6*d*.)

This unassuming work is a useful addition to the number of existing textbooks of Practical Organic Chemistry. It is designed specifically for the use of B.Sc. Students who are taking chemistry as a subsidiary subject to other sciences, and for others who have a limited time available for practical work. The author has obviously given careful consideration to their special requirements and the book should be very valuable for its purpose.

The book is divided into three parts. The first deals essentially with the purification of organic compounds, the second with their reactions and the third with their identification. The writing is clear and careful attention has been paid to experimental details. The general plan of Part II, in which a considerable number of small-scale experiments are described, is particularly good. On the other hand the necessity for a number of special exercises in purification (Part I) is less certain. If a few suitably selected preparations (such as that of acetanilide from benzene) are carried out *thoroughly*, the student takes the major operations of purification and the determination of physical constants in his stride.

The author states that the book should be used in conjunction with a

theoretical text-book, but even so it is to be regretted that no equations at all are given in a book intended for elementary work.

The detail throughout is accurate, but the following minor criticisms may be made. In the diagram on p. 29, the attachment of the melting point tube is incorrect. The sentence (p. 22) "In general an impure substance has a lower melting-point than a pure chemical compound" is rather loose. The design of the dropping-funnel in Fig. 19, p. 58, could be improved. Indigo should not be dyed on wool from a bath containing free caustic soda in the manner suggested (p. 63). Many of the melting and boiling points in Part III are lower than those recorded for the purest samples.

The diagrams and general printing are good, especially in view of the low price of the book.

R. P. L.

An Introduction to the Biochemistry of Nitrogen Conservation. By Gilbert J. Fowler, D.Sc., F.I.C. [Pp. viii + 280, with 6 plates, 13 figures and a folding diagram.] (London: Edward Arnold & Co., 1934. 12*s*. 6*d*. net.)

The possibilities of averting the great waste of nitrogen which has accompanied our advancing urbanization has intrigued the imagination of scientist and layman alike for a century or more. The fluidity of the atmosphere makes the conservation of excreted carbon from human lungs a matter in which Nature can continue to have her own way, but the disposal of nitrogenous excreta through necessarily localised sewage operations is a definite problem for human ingenuity.

Dr. Fowler has taken a very considerable part in the attempts to deal with this problem and he writes with authority. In this book he gives an account of the biochemical principles that have been invoked in negotiating the problem and he also gives, without a surfeit of technical detail, descriptions of the chief processes in operation.

The author "begins at the beginning" as we say. Anyone with a working knowledge of a chemical formula can be put in possession of a knowledge of the present position of the nitrogen conservation problem by this very readable book. Indeed, one is almost tempted to think the early chapters are a little too elementary, but that is an error, if error it be, on the safe side.

The book is based on lectures delivered in India but it is by no means restricted to Indian problems. It presents a conspectus of the nitrogen cycle in nature, with the problems that the intrusion of civilisation has incurred, in a way that should be appreciated by every class of reader.

N. M. C.

A Manual of Biochemistry. By J. F. McClendon, Professor of Physiological Chemistry, University of Minnesota Medical School. [Pp. viii + 381, with 58 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 31*s*. net.)

This book consists of six sections of which the first, entitled Introduction (67 pages), gives a general survey of the subject and includes a brief description of the more physical-chemical aspects of biochemistry. The vitamins appear in this section somewhat unexpectedly on p. 3. The second section gives a rather extensive survey in about 63 pages of the rôle of various elements and inorganic compounds in plant and animal biochemistry. In

section III (147 pages) various organic substances including the simpler compounds met with in biochemistry as well as the more complex natural products such as the carbohydrates, fats and proteins are described, whilst the next section, of 23 pages, headed Physiological Summary gives an account of various foodstuffs followed by a discussion on the successive stages of digestion and some remarks on a number of other metabolic processes. A large assortment of practical experiments are described in section V, whilst section VI consists of a list of 1,000 compounds, giving molecular weight, m.p. or b.p., density, crystal form or colour, solubility and a brief statement of any important physiological action. Curiously enough the formulæ of these compounds are not given. Though this book contains a large assortment of facts, some of which are curious and interesting and not generally known, it cannot be recommended for the use of students. It is not well written, the arrangement is illogical and disconcerting, whilst many of the statements are ambiguous and misleading. Thus we are told that "Ethyl alcohol is produced in muscular contraction" (p. 132), and that "ascorbic acid is one of the fatty acids" (p. 4). There is a curious mixture of very new with quite out-of-date information. Some of the juxtapositions produce a decidedly humorous effect. For example, in section II under a discussion of potassium salts it is stated that "Potassium salts added to water aid in putting out oil fires. There is enough of the element in sea water for this purpose," whilst "Chlorate in certain solutions liberates chlorine. It should be emphasised that free chlorine does not cure influenza nor the epidermophyton infection known as athlete's foot" (p. 79). With all its faults as a manual of biochemistry the volume may be recommended to chemists with a sense of humour and time to spare.

W. O. K.

Thorpe's Dictionary of Applied Chemistry. Supplement: Vol. I. A–M. By Jocelyn Field Thorpe, C.B.E., D.Sc., F.R.S., and M. A. Whiteley, O.B.E., D.Sc., assisted by eminent contributors. [Pp. xxii + 680, with illustrations.] (London: Longmans, Green & Co., 1934. 60*s*. net.)

The appearance of this volume affords a measure of the very remarkable advance in applied chemistry during the thirteen years that have elapsed since the publication of the first volume of the latest edition of the main work. Apparently it is going to take between twelve and fourteen hundred pages to summarise the progress during this comparatively brief period.

As the authors explain in the preface, the policy adopted in bringing the Dictionary up to date has been to issue two supplementary volumes dealing with those sections in the original work in which the greatest and most striking advances had been made, and to concentrate on monographs written by experts whilst retaining the various items in the existing Dictionary and recording any new knowledge concerning them. This task has been effectively carried out, the monographs summarise excellently the recent advances in the various subjects and their distinguished writers are a guarantee of their value. To mention but a few names in an impressive list of 120 contributors, E. F. Armstrong writes on carbohydrates, enzymes and glucosides, G. Barger and F. L. Pyman on alkaloids, W. A. Bone on coke manufacture and fuel, Sir Daniel Hall on cereals and grassland, A. Harden on

fermentation, T. P. Hilditch on catalysis in industrial chemistry, E. K. Rideal on disinfectants and L. J. Spencer on minerals. At the same time the shorter articles are full of useful information and the supplement is worthy of its progenitor. It is difficult to find any important topic missing, but some of the modern solvents are not yet mentioned, for example, dioxan and various furfural derivatives, but perhaps these will appear in the second volume under solvents.

Thorpe's Dictionary must surely be in every University, Work's or Consultant's chemical library and the supplement will be no luxury but a necessity.

O. L. B.

The Principles of Motor Fuel Preparation and Application. Vol. I. By A. W. Nash, M.Sc., and D. A. Howes, B.Sc., Ph.D. [Pp. 538, with 125 figures, including 9 plates.] (London: Chapman & Hall, Ltd., 1934. 30*s*. net.)

On the morning this review was penned the news was broadcast that Melbourne had been brought within seventy-one hours' flight from Mildenhall. What further testimony is needed to our subject and to its bearing on the Science Progress of our generation? To Kipling's famous dictum "Civilisation is Transportation" we of the oil age may add "Transportation is Motor Fuel" without disrespect to the future of gas and electricity in their respective spheres. In view therefore of the importance of the subject we welcome the appearance of what is undoubtedly the biggest, best and most up-to-date book on motor fuel. It should be mentioned that only the volatile motor fuels are included in Vol. I. Diesel oil, which many regard as the principal motor fuel of the near future, has wisely been reserved for a succeeding volume.

An examination of the table of contents at once impresses one with the remarkable completeness and efficiency with which the various sections have been compiled. Petrol as the chief motor fuel naturally takes pride of place and the methods employed in its manufacture from petroleum occupy the first half of the book. The section dealing with refining methods deserves particular commendation for it clearly indicates the intimate knowledge of current refinery practice which the authors possess. The merits of a petrol are defined by three factors, its volatility, its stability, and its anti-knock value, and an admirable account is given of the means whereby these qualities are conferred and controlled.

The alternative motor fuels are dealt with equally thoroughly. The potentialities of ethyl alcohol (whether produced by fermentation or by hydration of ethylene) and of methanol in admixture with petrol have attracted much attention in recent years, often, it is to be feared, without unbiased recognition of the demerits as well as the merits of such blends. Neither in this country nor in America is there much prospect of the wide use of alcohol as a motor fuel, but on the Continent and in certain of the Dominions this home-produced material has attained some importance, so that the section on alcohol fuels will be of exceptional interest to some readers.

To this country on the other hand the production of petrol from coal either by distillation or by hydrogenation is of both topical interest and potential importance. Carbonisation methods rather surprisingly receive a

minimum of comment, possibly because there are other books which deal with the subject in extenso, but the account given of the hydrogenation processes developed by the I.G., Standard Oil, and Imperial Chemical Industries bears the hall-mark of first-hand knowledge.

Other sources of motor spirit, relatively small but locally important, such as oil shale and natural gas receive adequate attention, and there is an excellent review of the chemical potentialities of the hydrocarbon gases which are at present an embarrassment to the oil producer and the oil refiner.

The bibliography, indexes and illustrations are on the same high standard as the text, and the book can be unreservedly recommended as the best of its class yet produced.

F. B. THOLE.

Alloys of Iron and Tungsten. By J. L. GREGG. [Pp. xii + 511, with 184 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 36s. net.)

THE volume under review is the third volume of the Alloys of Iron Research Monograph Series prepared at Battelle Memorial Institute, U.S.A., as a part of the Institute's contribution to Alloys of Iron Research, and is released by the United Engineering Trustees, Inc., for the Engineering Foundation.

It is claimed for these single volume monographs that they present a concise but comprehensive critical summary of information on the ferrous alloys, thus providing a reliable foundation for future research, and furnish the practical metallurgist, steel worker, foundryman, and engineer with the essential information scattered through more than two thousand different journals and text books throughout the world.

One gathers that through the courtesy of both American and foreign metallurgists it has been possible to obtain from specialists much unpublished information and detailed opinions regarding conflicting data; also, that each monograph to date has been subjected to the criticism of experts, and has, moreover, been reviewed and approved for publication by a committee comprised of representatives of metallurgical, engineering, and allied societies.

The author is to be congratulated on having compiled this particular monograph in a pleasing and well-ordered manner. One can affirm, with confidence, that no other published work on the subject of the iron-tungsten alloys is either as up-to-date or as informative as the present volume. The results of a number of researches published as recently as last year are included in the text.

A large section of the book is devoted to the constitution of the iron-tungsten and iron-tungsten-carbon alloys. The manufacture, characteristics and mechanical properties of tungsten steels, including the one-time popular magnet steels, claims another large section. Tool and die steels, and the development and nature of high-speed steels, including the cutting and other properties of the latter, also claims a further large section. A smaller section serves for a description of the miscellaneous alloys containing tungsten, and the constitution and properties of these alloys. The concluding pages of the volume are reserved, in part, for a very comprehensive bibliography of published information relating to tungsten and tungsten alloys, covering

the years 1860–1933. This bibliography alone, apart from the excellence of the monograph itself, makes this volume a most valuable book of reference. A name and a subject index, the latter of 17 pages, finishes off a splendid contribution to the literature of the ferrous alloys.

W. L.

Industrial Furnaces. Vol. I. By W. TRINKS. Third edition. [Pp. x + 456, with 359 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 37*s.* 6*d.* net.)

FURNACE technique becomes of great importance when we remember that almost every article of daily use undergoes some form of heating process in the course of manufacture. The object of the furnace is to carry out the heating process in as efficient a manner as possible. This implies efficient utilisation of the fuel. Much has been heard in recent years of fuel economy from a national viewpoint, but to the user a more convincing appeal for fuel economy is likely to be the great increase in the cost of all fuels since the War.

Development in the field of fuel technology has been rapid in this period—so much so that precise combustion conditions for any fuel can now be specified. The method of introducing the fuel into the combustion chamber varies with the nature of the fuel, but in no case should the mechanical equipment employed be described as a burner. The fuel is burned in the furnace combustion chamber. An oil burner, for example, may function perfectly in atomising the oil and dispersing it uniformly throughout the combustion chamber, but, if this latter is wrongly designed, efficient combustion becomes an impossibility.

The policy in the past was to leave the design of furnaces to the foreman bricklayer. With cheap fuel, low temperature combustion and simple furnace design, he was able to justify the confidence placed in him. But when fuel economy, and, therefore, high temperature combustion, becomes a necessity, furnace design ceases to be empirical and becomes a science.

A second factor that will influence furnace design, but one that we need not pursue further here, is the great increase in the cost of labour in the post-war years.

We welcome this new edition of Professor Trinks' book on industrial furnaces (Vol. I). When first published in 1923, it was the first of its kind in English where an attempt was made to design a furnace rather than to describe it. For lack of experimental data—although a notable contribution to the subject—it left much to be desired. In the present, completely revised and rewritten volume, the author is able to remedy the deficiencies of the earlier work. The book contains a good index.

There is one comment we should like to make with special reference to future editions of this work. Furnace design calculations are generally difficult and lengthy. Graphical methods of computation are, therefore, a great convenience. This, the author fully appreciates, but the size of the charts which he presents is too small and seriously restricts their usefulness.

The book needs no introduction to those engineers and metallurgists who are engaged in this work. The author has maintained close contact with recent developments in his subject, with the result that the present

volume is likely to be as great a success in its time as the earlier one was when first published. To students of metallurgy and engineering interested in the field of fuel technology, we confidently recommend it, even at the price of 37*s.* 6*d.*

J. P. M.

German-English Chemical Terminology. An Introduction to Chemistry in English and German. By ALEXANDER KING, M.Sc., A.R.C.S., D.I.C., and Dr. HANS FROMHERZ. [Pp. xvii + 324.] (London: Thomas Murby & Co.; Leipzig: Max Weg, 1934. 12*s.* 6*d.* net.)

THIS represents an ingenious method of familiarising English-speaking and German-speaking chemical students with one another's language; it consists of a general introduction to chemistry in five parts, elementary, inorganic, organic, physical and modern ideas of the structure of matter in English and German on opposite pages. The subject matter is very suitable for revision reading for honours students and anyone who will conscientiously read through the book in the tongue he wishes to acquire will gain, not only in his knowledge of the language, but also in his knowledge of chemistry. In this respect the work has great advantage over similar publications. The translation from one language to the other is as literal as possible and it is obvious that much care has been taken by the authors in framing their sentences so that this could be done without too much violence to idiom. In addition care has been taken to choose the subject-matter so as to ensure the reader acquiring as large a vocabulary as possible of special chemical expressions. This is undoubtedly the best book so far produced to give students practice in reading the two languages and can be unreservedly recommended.

O. L. B.

GEOLOGY

Geologic Structures. By BAILEY WILLIS and ROBIN WILLIS. Third edition, revised. [Pp. xviii + 544, with 202 figures, 12 plates and frontispiece.] (London: McGraw-Hill Publishing Co., Ltd., 1934. 24*s.* net.)

THE second edition of this work, in which Robin Willis was associated in authorship with his father Prof. Bailey Willis, the original author, was published in 1929. The number of pages has been increased in the present edition by 26 and the number of figures by 51. A change has also been made in the order of presentation of the material. The statement of relevant mechanical principles now precedes the general descriptive matter, and analyses of the stresses and strains involved now follow descriptions of each type of structure, such as joints, folds, and faults. Thus the book is not merely a revision of the earlier edition but a complete rearrangement, with the addition of much new material. There are chapters on stratified rocks, on the structures of igneous rocks and metamorphic rocks, and on the physiographic expression of structures. Field methods (with many useful hints), graphic methods (strike, dip, mapping, etc.), and various practical problems, form the subjects of three further chapters. The book ends with a valuable chapter on fundamental facts and concepts in which the constitution and forces of the earth, zones of pressure, strength and temperature in the outer crust, and isostasy are the topics dealt with. The

illustrations are numerous, well-drawn and well-chosen. The book is made available for both elementary and advanced courses by the device of distinguishing the two grades of material in the list of contents by roman and italic type respectively. The book is practically free from typographic errors, and but one error of fact has been noted. Ernst Cloos, the son, is confused with Hans Cloos the father, on pp. 242 and 243 and in the index, p. 515. It is Hans Cloos who is the originator of "granite-tectonics."

G. W. T.

An Introduction to Stratigraphy (British Isles). By L. D. Stamp, B.A., D.Sc. Second edition, revised throughout and enlarged. [Pp. xvi + 381, with 101 figures.] (London: Thomas Murby & Co., 1934. 10*s*. net.)

The first edition of this work, published eleven years ago, was reviewed in Science Progress, April 1924, p. 658. While its length, as measured by the number of pages, has not been increased, yet Dr. Stamp has somehow contrived to add seventeen figures to the original number, to incorporate the results of much recent work, and to rewrite some sections of the book. We are glad to see that the author has dropped the use of the term "lake" in referring to the Old Red Sandstone basins of deposition; but he still quite unnecessarily correlates different kinds of petrographic provinces with Atlantic or Pacific types of *coast line* (p. 10), and still uses the ambiguous term "Proterozoic" as a synonym for the Older Palæozoic era (p. 27). This book maintains its place as the first of a new type of stratigraphical textbook, in which the intrinsic interest of the geological story is kept well to the fore, the facts are made to flow naturally from a preliminary statement of the geographical, climatic and sedimentation conditions of each period, and the orogenic and igneous episodes which punctuate the story are given their full weight.

G. W. T.

Geologic History at a Glance. Compiled by L. W. Richards and G. L. Richards, Jr. [2 plates, with printed matter on covers.] (Stanford University, California: Stanford University Press; London: Humphrey Milford, 1934. 3*s*. 6*d*. net.)

This remarkable publication consists of two large plates with some explanatory printed matter on the covers and on the plates themselves. Plate 1 shows panoramic views of both the deep inner and the wide outer valleys of the Grand Canyon of the Colorado, probably the most complete natural section of the geological column in the world. The various formations are tied to a formal vertical section placed on the left-hand side of the photographs, and the latter in turn is connected with a tabular standard geological column in which eras, periods, epochs, distribution in North America, biological events and major physical events are displayed in parallel columns. The Kainozoic part of the succession is similarly connected with a Californian panorama and section. Plate 2 is similar in construction but different panoramas in the Grand Canyon and in California are used, the former showing the Archæan foundation in greater detail than in Plate 1. Furthermore the sections are now tied to an ingenious geological column in the form of a stepped or recessed skyscraper, in which the volumes of the successive stories, with those of the Archæan foundation and Proterozoic "base-

ment," represent the respective percentages of the column occupied by the various eras. A good list of selected references for supplementary reading is provided. This realistic representation of geological history should be of much value to keen new students of the science.

G. W. T.

BOTANY

A Textbook of General Botany for Colleges and Universities. By Richard W. Holman, Associate Professor of Botany in the College of Letters and Science of the University of California, and Wilfred W. Robbins, Professor of Botany in the College of Agriculture of the University of California. Third edition. [Pp. xxii + 626, with frontispiece and 463 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 25s. net.)

The third edition of a carefully prepared and well-thought-out textbook. In considering the anatomy of the stem secondary thickening is carefully worked out and a series of photographs of timbers and diagrams provided which should be of much assistance to students in this difficult part of the subject. The primary tissues and tissue systems are however given relatively little space; phlœm fibres are only mentioned and the treatment of cork and cork-formation seems inadequate (Fig. 51, p. 87, cork from the stem of *Aristolochia* is misleading). When dealing with the groups the type system has been abandoned and indeed too many examples are given, for example in the portion dealing with fungi several of the excellent illustrations are barely mentioned in the text and there is too much repetition, *e.g.* three figures of puffballs. In the description of Selaginella no mention is made of the ligule, a characteristic structure here, which is figured only in the embryo.

The tendencies in the evolution of insect-pollinated flowers are considered towards the end of the book, but strangely enough no descriptions are given of the families referred to. The book is, within the limitations mentioned, a good general introduction to the subject. It is well illustrated and printed.

E. M. C.

Researches on Fungi. Vol. VI. By A. H. Reginald Buller, D.Sc., Ph.D., F.R.S., Professor of Botany at the University of Manitoba. [Pp. xii + 513, with 231 figures.] (London, New York, Toronto: Longmans, Green & Co., 1934. 28s. net.)

This latest volume of Prof. Buller's begins with a summary of work on the biology of *Pilobolus*. Its parasites, the secretion from the subsporangial swelling, the projection of the sporangium, its method of attachment on coming to rest and more especially the ocellar function and the reaction to the stimulus of light, especially from two sources, are all considered. Mr. W. B. Grove has contributed a chapter on the systematics of the group including a new species first described by Prof. Buller.

The next part is concerned with the production and liberation of spores in the discomycetes. The first chapter which describes puffing generally and that of *Sarcoscypha protracta* in particular is in many ways the most important in the book. The observations of Ziegenspeck, who showed that just before bursting the ascus tips showed signs of strain, are of great interest.

The whole problem is probably to be solved only by a consideration of the fruit body as a whole and not merely by a study of the ascus or of the hymenium, while the action of certain poisonous substances in causing the asci to open does not necessarily have any bearing on the phenomenon in nature.

Sarcoscypha has an apothecium of campanulate form. The paraphyses act as spacers so that each ascus is allowed to discharge its spores unimpeded by adjoining asci. The asci when ripe stand out slightly above the hymenial surface and the future opening (ascostomes) are directed vertically upwards so that, when the asci do open, the spores are shot vertically upwards and continue straight up for some distance after which they scatter. By means of a simple and very ingenious piece of apparatus it is shown that the puffing produces a blast of air which acts as an agent in carrying the mass of spores vertically upwards before they separate. In general the phenomenon of puffing probably causes the spores to attain a greater height above the hymenium than would the opening of individual asci at different times. In other Ascomycetes, *e.g.* Ascobolus and Lachnea, the asci curve heliotropically towards the source of light, while in *Aleuria* (*Peziza*) *vesiculosa* and *Galactinia badia* the paraphyses curve heliotropically long before the asci mature.

Most of the rooting-bases (pseudorhizæ) of fungi would seem to be annual structures. The growth and behaviour of these in some fungi have been investigated and a more detailed description given of the perennial rooting bases of *Collybia fusipes*, which in time may grow to form a structure resembling a sclerotium in appearance. Strangely enough *Sarcoscypha protracta* forms a similar perennial pseudorhiza.

The last portion of this interesting work describes the structure, biology and dispersal of the gemmifers of *Omphalia flavida*, which are regarded as highly specialised sporophores and institutes a comparison between these and the biologically similar structures of *Sclerotium coffeicola*.

E. M. C.

Plant Chimæras and Graft Hybrids. By PROFESSOR W. NEILSON JONES. [Pp. viii + 136, with 21 figures.] (London: Methuen & Co., Ltd., 1934. 3*s*. 6*d*. net.)

PROF. NEILSON JONES has here given us an admirable and very readable account of these interesting plants, which have as it were a dual personality, consisting as they do of two or more components which differ in their genetical constitution.

The origin of chimæras by artificial grafting or by natural somatic segregation is discussed, together with the possible rôle played by hybridisation in their production.

The author describes a number of examples illustrating the great diversity of chimæral structure, and emphasises the frequent sterility, partial or complete, that accompanies this condition.

The familiar *Cytisus Adami* has aroused the curiosity of botanists and laymen alike and the present account should have a wide appeal.

A very useful feature is the documentation of the text with references to the bibliography of over 140 titles.

E. J. SALISBURY.

Mikroskopische Technik. By HEINZ GRAUPNER. Taschenbuch der biologischen Untersuchungsmethoden 1. [Pp. vii + 157, with 31 figures.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1934. Paper covers, RM.5.20; stiff board, RM.5.80.)

THE first part of the book deals with general microscopical technique and includes the treatment of living material, the fixation and manipulation of fixed material (staining, total preparations, sectioning, etc.), as well as sections on the microtome, the microscope and its accessories.

A second section is devoted to special techniques, and deals first with cytological, then histological methods. A few pages are given to bacteriological and protozoological methods, while space is found for descriptions of the fluorescent and polarising microscopes. Micrometry and graphic reconstructions are also dealt with.

The third section is chiefly an alphabetical list of apparatus, chemicals and formulæ.

Despite the book's small size—and the volume really is a pocket book—the author covers a wide field in a thorough manner: he is at all times concise, and this tends to enhance rather than to detract from the value of a book of this nature.

F. W. J.

ZOOLOGY

Cellular Respiration. By N. U. MELDRUM, M.A., Ph.D. Methuen's Monographs on Biological Subjects. [Pp. xi + 116, with 17 figures.] (London: Methuen & Co., Ltd., 1934. 3*s*. 6*d*. net.)

FOR the general biologist, few things have been more perplexing than the advances in the study of cellular respiration which have been made in recent years. Since the early work of Warburg, a succession of important discoveries has been made. The importance of each new system, glutathione, cytochrome, or the oxidation-reduction state of the cell, has been immediately obvious, but continued investigation has always shown that these systems are far more complicated than they at first appeared, and that their relationship with each other is obscure. For this reason, this little book is of quite unusual value for its size. The author, as he himself points out, has presented the data in a somewhat didactic manner, which however is exactly the manner required. In little more than a hundred pages he outlines the main features of the various cellular respiratory systems which have been analysed. He omits all reference to oxidation-reduction potential work, for, as he points out, there is still far too great a disagreement as to the interpretation to be placed upon this to allow one to relate it to other fields.

In the introduction, a general definition of cellular respiration is given. The second chapter deals with dehydrogenase systems, the third with the action and nature of the Atmungsferment of Warburg, the fourth with oxidases, peroxidases and catalases, the fifth with the cytochrome system, and the sixth with the glutathione system. The relation of each chapter to the others is fully considered, and there is an excellent bibliography given after each section. Reading this book makes one realise how great a calamity was the untimely death of the author.

C. F. A. P.

Insect Physiology. By V. B. WIGGLESWORTH, M.A., M.D., Lecturer and Milner Research Fellow in Medical Entomology, London School of Hygiene and Tropical Medicine. Methuen's Monographs on Biological Subjects. [Pp. x + 134, with 13 figures.] (London: Methuen & Co., Ltd., 1934. 3*s*. 6*d*. net.)

AN examination of earlier works, such as Marchal's "Physiologie des Insectes" in Richet's *Dictionnaire de Physiologie* and a comparison with the work under review, will reveal a considerable change in outlook on insect physiology and, as the author points out, this is due largely to the demands made by the Economic Entomologist for information on many points connected with the physiology of insects.

This little book reveals the author's excellent qualifications for writing a thesis upon this subject which is still, on very many points, in an early stage of development, and I have been struck, in the first nine chapters, by the fairness with which alternative possible explanations have been put forward. In the tenth chapter, however, I feel that there is a cleverly constructed vicious circle, first to make the last moult of the exopterygote insect homologous with true metamorphosis of the endopterygote and then to uphold the view that moulting and metamorphosis are of the same order of phenomena. To my mind the author slurs over the importance of the larval, as opposed to imaginal tissue, by arguing that "even in the extreme case of the Diptera many larval organs (Malpighian tubes, certain muscle groups) are remodelled without much change to form those of the adult" (p. 92), and then he arrives at what he has not really shown, and what to my mind is not true, "Thus, physiologically, metamorphosis differs from moulting only in degree." I regard the larva as an interpolation in the life-history of the primitive insect whose existence in the holometabolous forms is cut down to the final two stages (the nymph or pupa and the imago), and there is as much ground for this view as for that supported by the author. In this chapter also there is a tendency to be rather free with the use of hormones which, nowadays, are always being introduced to cover up absence of knowledge in various branches of biology.

FRANK BALFOUR-BROWNE.

The Locust Outbreak in Africa and Western Asia in 1933. Survey prepared by B. P. UVAROV. Economic Advisory Council Committee on Locust Control. [Pp. 66, with 9 maps.] (London: H.M. Stationery Office, 1934. 2*s*. net.)

THIS report gathers together further information as to the locust outbreak which has now lasted nine years. Three species of locust have been involved and some areas in East Africa have experienced attacks by all three in succession. Sometimes the locusts even affect one another, as when the swarms of the Red Locust help to maintain the Tropical Migratory Locust in the swarming phase when the numbers of the latter species are falling off and it might be expected to revert to the solitary phase.

Although it can be understood how such swarms should multiply when once a certain point has been reached, it is still a mystery what starts them up at the beginning of a major outbreak. Certain regularities in the phenomenon are, however, beginning to appear. The swarming seems to begin

with the species (Desert Locust) which breeds furthest north, and the tendency passes southwards. What it is (*e.g.* whether it is peculiar climatic conditions) that passes down Africa from north to south is not yet understood, but this movement, which seems to have characterised previous outbreaks, suggests at least that the initiation of swarming is a process common to all locusts and is not something different for each species.

The new facts recorded in the present report largely fill in the details of the scheme which has already been outlined in previous reports. The habits of the Desert Locust, in particular, are beginning to be well understood. Certain areas in the hinterland of tropical West Africa are still very insufficiently explored, and the need for reconnaissance in them is becoming greater as observations elsewhere become more complete.

The survey of locust-literature, which has formed a useful feature of previous reports, is brought up to date.

O. W. RICHARDS.

Economic Advisory Council. Committee on Locust Control. Sixth Report. [Pp. 55.] (London: H.M. Stationery Office, 1934. 1*s*. net.)

THE present report deals with the advances made in the study of the Locust-problem in Africa from 1931 onwards. Since that date the number of investigators has considerably increased and certain important points in Locust-biology have been established. The Desert Locust (*Schistocerca gregaria*) has been shown, in each part of its range, to have two breeding-sites, between which it migrates. Thus it breeds just south of the Sahara and, migrating across it in the winter, breeds again in N. Africa in the spring. The young travel south again to complete the cycle. In the Tropical Migratory Locust (*Locusta migratoria migratorioides*), the migrations are not along such definite lines, but it has at least been proved that the recent catastrophic outbreak originated entirely from swarms in a single area in French Sudan. The same spread from a single centre has been established for the Red Locust (*Nomadacris septemfasciata*).

Now that these facts are established, it is possible seriously to consider methods of control. The early stages of experiments in employing aircraft for the destruction of flying swarms are described. This work has not yet progressed sufficiently far for any conclusions as to its value to be drawn, but there appears no reason why the method should not be very valuable under certain circumstances. The ideal method of control is to destroy the swarms in their early stage, before they have begun to spread. This will probably soon be possible on a large scale, but meanwhile some of the breeding-places are very inaccessible or are in difficult country, so that it may be necessary for some time to attack swarms after they have got on the move.

Further important observations and experiments are mentioned in connection with the number of locust-generations in a year and with the physical factors influencing this number. It is probable that in this direction, too, important results will emerge, particularly in the question of prediction.

The report fully vindicates the view that locust-investigations must be international if they are to meet with much success.

O. W. RICHARDS.

The Anatomy of the Salamander. By Eric T. B. Francis, B.Sc. Lond., Ph.D. Reading. With an historical introduction by Professor F. J. Cole, D.Sc., F.R.S. [Pp. xxxi + 381, with frontispiece, 25 plates and 3 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. 25*s*. net.)

A monograph on the morphology of a tailed amphibian comparable to the classical work of Ecker and Gaupp on the Frog has long been needed, and this book fills a real gap in zoological literature. As the Salamander is the form usually dissected in zoological laboratories as an example of the Urodela it is somewhat surprising that up to now no adequate account of its anatomy has been available. But if zoologists have had to wait a long time for someone to undertake the task of providing one this has now been admirably done by Dr. Francis in the volume under review.

The anatomy of the animal is exhaustively described and very completely illustrated by effective black and white drawings from original dissections. We are glad that the author has made his book something more than a mere colourless description of anatomy, which however well done is bound to be somewhat uninspiring. He brings to bear on his subject a thorough acquaintance with the widely scattered literature and a sound knowledge of comparative vertebrate morphology. The result is an encyclopædic and scholarly treatment, taking full account of the results and conclusions of previous workers, which are critically described and discussed where necessary. Though the standpoint, as it obviously must be in any such work, is primarily morphological, the author does not forget (as morphologists are sometimes accused of doing) that form and function are complementary, and we notice with satisfaction useful accounts of such things as the separation of arterial and venous blood, of the mechanism of respiration and the protrusion of the tongue in feeding, as well as of more purely anatomical matters. With regard to the first named we note that the importance of the septum bulbi in effecting such imperfect separation as occurs has apparently been much exaggerated in Amphibia.

In the case of nerves and muscles, and where necessary other structures, a full synonymy of the terms used by different authors is provided and this should be a boon to future research workers.

There is an introductory historical chapter by Prof. Cole and a bibliography of 840 entries. At the end of each chapter a list of the numbers of all the relevant items in the bibliography is given. It would have been an even better plan if the bibliography itself could have been divided into sections corresponding to the chapters, but this is a comparatively minor point. The book fulfils its purpose admirably and the printing and general get-up are of the high standard which is expected of the Clarendon Press.

B. W. Tucker.

MEDICINE

Practical Bacteriology: An Introductory Course for Students of Agriculture. By Andrew Cunningham, D.Sc. Second edition—Revised and enlarged. [Pp. viii + 203, with 26 figures.] (London and Edinburgh: Oliver & Boyd, Ltd., 1934. 7*s*. 6*d*. net.)

This book is an example of the unusual event of a second edition being notably inferior to the first, although the plans of both editions are similar, and in bulk

the second does not greatly exceed the first. If the first edition had been brought up to date in a few places the result would have been excellent. Much of the new edition is specialised and beyond the scope of a true introductory course; seemingly it would have been better to attempt less and teach more: some of the exercises partake of the nature of recipes. Principles are often (though not always) submerged beneath detail which to the student must appear confusing for lack of sufficient guidance.

Where the book is unpretentious, as in the section on bacteriology of animal diseases, it is very good indeed. In many places, however, the simple has been dropped (as, for example, the instructive section on "Giant Colonies") and what has been put in has not always been a gain. The determination of thermal death-points of bacteria is to be performed by a cumbersome method which would take several weeks, if not an entire term, even if the culture to be tested were to remain unchanged during that time. A technique which demands sterile apparatus for sampling soil is over-refined for the purpose of mere enumeration of micro-organisms; one is diiected to empty the soil sample on to a piece of sterile paper, pick out the stones with sterile forceps, and mix the sample by means of a sterile (sect. 8*a*) metal spatula. This cross-referencing would be splendid if it were consistent, but it is not. Once more, *le mieux a été l'ennemi du bien.*

H. N.

Mental Defect. By LIONEL S. PENROSE, M.A., M.D. [Pp. xii + 183, with 5 plates and 2 figures.] (London: Sidgwick & Jackson, Ltd., 1933. 8*s.* 6*d.* net.)

THIS book is intended for the use of medical or lay persons who desire to obtain information on some of the scientific problems associated with the study of mental deficiency. It is the first of a series of *Text Books of Social Biology* edited by Prof. Lancelot Hogben. The author's special qualifications for this kind of work are well known and his already considerable reputation will be enhanced by the issue of this volume.

Dr. Penrose is the Research Medical Officer of the Royal Eastern Counties Institution for Mental Defectives. In that capacity he has had considerable experience in the administration of the Mental Deficiency Acts and the treatment of patients thereunder: he has also investigated the condition and history of some 513 defectives maintained in the Institution and this book records his experience in that work and the views he has formed as a result.

While the investigation of 513 cases is a formidable task it must be recognised that such a number is not sufficient to base final conclusions on the varied and diverse major problems associated with mental deficiency, particularly when we come to the statistical treatment of the data which, as Dr. Penrose rightly insists, is of considerable importance.

The volume consists of fifteen chapters, an index and a glossary which will be helpful, at least to the lay reader. The first chapter deals with the historical and administrative side of the subject and many readers will no doubt wish that this had been more fully dealt with. There are then six chapters dealing with the methods of investigation applicable to this branch of study, followed by six chapters devoted to the description of the different types of persons who are subject to be dealt with under the Acts, and a final chapter dealing with treatment—radical, palliative and preventive.

The chapters on methods of investigation are both interesting and instruc-

tive; they not only describe the methods of the author but will be of much value to any reader who may be disposed to make similar investigations. They also bring out the pit-falls and difficulties inseparable from this kind of investigation.

In the six chapters descriptive of the type of persons who may be certified under the Acts, some attempt is made to measure the relative importance of heredity and environment in the causation of defectiveness. With regard to the cases actually investigated the author comes to the conclusion that in 62 per cent. of the cases the effect of environment and heredity could not be extricated from one another. He adds that every case is to some extent primary (heredity) and to some extent secondary (environment) if these terms are to be interpreted in their strictest sense. In this the author's findings do not differ from the views expressed by others who have investigated such cases.

The view that mental defectiveness can be definitely classified as either "primary" or "secondary" according as causation is thought to be hereditary or environmental has long since been shown to be wrong. Heredity and environment are not to be contrasted in the field of causation but must be regarded as complementary, with environment as a developing factor. Environment is often a co-efficient acting upon predisposition, more dramatic and clearly observable in the case of insanity than in the case of mental deficiency, but, nevertheless, effective and often acting in such a way as to bring under control a person who, but for the circumstances of his living, may have maintained some sort of place in the normal community. Economic and social maladjustment contributes largely to these problems and the relative importance of nature and nurture cannot easily be measured.

It is therefore important to observe that in this book Dr. Penrose is dealing only with certified patients. The major part of mental deficiency—the uncertified and uncertifiable—is outside the scope of his enquiry. From this point of view it must be remembered that in the majority of cases mental deficiency, even when clearly within the definitions prescribed by statute, is not alone sufficient to secure certification, and that in most cases there must be found one or other of the social disabilities prescribed in section 2 of the Act. In the result, as is well known, relatively few of the mentally defective are in the certified class.

It will thus be seen that if the author's views be accepted as representative of cases in the certified class a much larger group lies outside the scope of his enquiry. It is perhaps the failure to recognise that his investigations are limited to the class of case controlled in institutions that leads Dr. Penrose to refer to "the supposed fecundity of the feeble-minded" and to compare the total amount spent annually on the maintenance of defectives in Institutions with the cost of the fighting forces and to suggest a reduction of the latter for the support of the former. Fecundity has been alleged in relation to the feeble-minded not under control but it has nowhere been suggested that this is a condition of the classes dealt with by Dr. Penrose—the certified mental deficient. The amount mentioned (one and a half million pounds annually) as the cost of the maintenance of the certified defective in institutions loses sight entirely of the enormous cost in finance, in social disorganisation and human misery of the uncontrolled mental deficient not in institutions.

With regard to the 513 cases investigated, Dr. Penrose has endeavoured to determine the number of cases in which inheritance or environment

may be regarded as the origin of defect. His final classification is as follows:

Primary aments (Hereditary)	137	(29%)
Secondary aments (Environment)	47	(9%)
Unclassified (Impossible to disentangle causes)	329	(62%)

Notwithstanding the observations quoted above there is a tendency throughout the book to disparage the views so often expressed by other observers as to the incidence of defectiveness due to hereditary causes. This fact rather suggests that the high proportion of cases unclassified in this table may be due to a too exacting interpretation of the meaning of heredity. This is confirmed by the statement (p. 64) that we must look for inheritance "on mendelian lines" and that "it is necessary, in order to establish the presence of mendelian factors, at least to begin by recognising the *same* conditions in different members of the family." What is intended by this passage is not clear, but if it is intended to suggest that each of the many forms of mental defect is to be regarded as a mendelian character transmissible as such, few students of heredity will agree.

It is common ground that there are many forms of amentia: some not due to inheritance, others inherited in different ways. Of the latter, some suggest the mendelian recessive, some the mendelian dominant, others sex-linked inheritance and others have no relation to any known mendelian rule. Some present the characteristics of disease, others of degeneracy, some the pressure of economic and social maladjustment upon inherently weak material and some the simple result of peculiar parental fusion. May it not be that, except in the relatively few conditions known to be of the acquired variety, many of the manifestations in the whole range of mental disorder—from mild incompetence to insanity—are varied manifestations of a single underlying cause. There are undoubtedly more mentally defective children whose parents are, or have been, or become insane than there are whose parents are mentally defective, and in both of these classes defective children are more numerous than mentally defective children born in families in which no history of mental defect can be found. Is inheritance as a possible factor in these cases to be disregarded because sameness is not a quality in the manifestation of such disorders? Who can say that insanity in one generation and mental defect in the next is not due to a common underlying cause which may be transmitted as a mendelian character. In the final chapter on treatment the author deals, somewhat superficially, with the question of sterilisation. This section is lacking in the considered argument to which, even in the present state of our knowledge, the subject lends itself, and betrays social and political bias. Most people will agree that the legal prohibition of voluntary sterilisation should be removed.

Notwithstanding its limitations this book is a valuable contribution to the literature on the subject, not only because it deals with original research but because it is suggestive of the need for further and more comprehensive research in these matters. There is so much we want to know.

E. J. L.

The Improvement of Sight by Natural Methods. By C. S. Price, M.B.E. [Pp. viii + 232, with 24 figures.] (London: Chapman & Hall, Ltd., 1934. 5s. net.)

It was inevitable that a reaction should arise against the widespread use of spectacles, largely encouraged by the sight-testing opticians. Mr. Price's

book expresses this reaction, but swings the pendulum too far in the opposite direction.

He lays stress on the psychological factor which so often is an important element in aggravating small disabilities of vision, and the methods of treatment described in the book largely influence this factor.

It is unfortunate that, to any but a critical reader, the book may give the impression that this treatment is a cure-all, and applicable to every degree of the conditions described. A critical reading, however, will show that Mr. Price does not, in reality, exclude the wearing of glasses nor resort to an oculist.

In many cases of myopia and squint in children the teaching in this book would be definitely harmful, by delaying the early use of glasses, which are necessary in some cases.

Incidentally Mr. Price does not realise the immense advance which has taken place in the treatment of detachment of the retina by diathermy in the last three years. Thirty per cent. of cases, at least, can now be cured if seen early by a competent operating oculist.

Mr. Price's explanations of common eye troubles are clearly and simply given, and he is evidently not a whole-hearted believer in the theory of Dr. Bates, who started the "Better Eyesight without Glasses" school, and based his treatment on the fallacy that the focussing of the eye is done by the pressure of the external muscles on the eyeball.

Anyone with bad vision can get used to interpreting the blurred images they receive more or less correctly, but it should be for an oculist to decide whether the non-wearing of glasses will be harmful to the eyes or the mentality. If not, the patient can decide whether his vision is good enough for practical purposes, and adopt all or some of the methods described, if he wishes, without risk.

M. L. H.

A Textbook of Hygiene for Training Colleges. By MARGARET AVERY, B.Sc., M.R.San.I. Fifteenth edition. [Pp. xx + 415, with 130 figures.] (London: Methuen & Co., Ltd., 1934. 6*s*.)

THE number of editions through which this book has run since its first appearance in 1919 is sufficient proof of its continued popularity. The present edition has been partly rewritten and considerably rearranged, while items of importance have been added.

The first section of the book is a general introduction to biology, and a rapid excursion through the plant and animal worlds as a preliminary to the study of the human body. This is a welcome improvement upon earlier editions since the majority of students begin the hygiene course without any adequate conception of biological principles and are immediately plunged into the complications of human physiology with insufficient background.

In succeeding chapters, the systems of the human body are described and accounts of the reproductive system and endocrine organs have been added. As both these important topics are dealt with in four pages, the description in this particular must be considered inadequate.

The section upon practical hygiene ("The Conditions of Bodily Health") is in general very good, the information being sufficient and useful; but in the accounts of common ailments of schoolchildren the author attempts to give far more than is necessary or desirable for the Training College students

to whom the book is addressed. For the treatment of diphtheria, we are told that "antitoxin should be injected *at once*," a direction more in place in a medical textbook than in one designed for teachers whose interest in disease should be principally focussed upon recognition of early symptoms rather than therapeutic procedure. The author urges that teachers may do good work by persuading negligent parents to carry out the recommendations of the School Doctor and, to give the teacher the necessary knowledge for such persuasion, some excellent general accounts are given of such subjects as vaccination, infant welfare centres, etc.

The book is plentifully illustrated but some of the simplified anatomical diagrams (as for example that of the inner ear on p. 143) cannot convey very much to the ordinary student.

C. G. E.

PHILOSOPHY AND THE HISTORY OF SCIENCE

Mind and Nature. By Hermann Weyl. [Pp. viii + 100, with 12 figures.] (Philadelphia: University of Pennsylvania Press; London: Humphrey Milford, 1934. 6*s*. 6*d*. net.)

This slender volume contains the substance of five lectures which were delivered by Professor Weyl at Swarthmore College, in the University of Pennsylvania. The purpose of these lectures is to examine the characteristic features of our knowledge of the world as expressed in Mathematical Physics. The plan of the investigation is as follows: Professor Weyl first contrasts the manner in which the so-called secondary qualities appear in immediate experience and the manner in which they appear in our sophisticated theories. He then conducts a similar analysis for the primary qualities of extension and duration, explaining in detail the mode in which space and time enter into Mathematical Physics. The third Lecture gives the essence of Weyl's message regarding the constructive character of scientific concepts and theories of the "representative" character of mathematical symbolism. These methodological considerations are exemplified in the concluding chapters by the special theory of Relativity and the modern Quantum Theory.

The reviewer cannot refrain from making a comparison between Prof. Weyl and Mark Tapley. The Dickensian hero strove to be a philosopher but "cheerfulness would keep breaking through." Weyl also strives to philosophise but in his case it is Mathematics which intrudes like a Pythagorean snake into the philosophical paradise. There is a delightful passage in the first page in which the author, having alluded to the physical problems of colour composition, at once expands his wings and soars away into the realm of Projective Geometry, establishing, as with a shout of triumph, the magnificent result that all the theorems of Projective Geometry as it applies to a convex region have an immediate interpretation and validity in the domain of perceived colours. In fact, the essence of Weyl's message is that the characteristic feature of Mathematical Physics is that it constructs only an isomorphic relation between the entities and relations of mathematical symbols and the entities and relations of the physical world.

This principle may be interpreted in many ways. The minimisers will accept it as a sober and cautious statement of the inevitable inadequacy of all mathematical theorising and will assert with joy that more is given in immediate experience than is ever preserved in mathematical symbolism. The maximisers will assert that the idea of isomorphism implies an unsur-

mountable barrier of knowledge. The author appears to waver between these two extremes. At times he is sounding the praises of the "immediate, alive intuition" as compared with scientific cognition; and at times he relapses into a solipsism which denies a real and separate existence to the external world.

I find the concluding chapters on Relativity and a Quantum Theory somewhat inadequate, especially when regarded as providing material exemplifying the main philosophical and psychological theses of these lectures. Surely it is an inescapable fact that all versions of these theories have been profoundly affected during their period of formation by the general philosophical outlook of their creators. Thus Einstein's Theory shows the influence of Kant and all versions of the Quantum Theory are deeply imbued by dialectical materialism. In these circumstances it is futile to cite scientific theories as evidence or examples of general philosophical principles. The argument runs essentially in a vicious circle.

G. T.

Scientific Organizations in Seventeenth Century France. By HARCOURT BROWN, M.A., Ph.D. [Pp. xxii + 306.] (Baltimore: The Williams & Wilkins Co.; London: Baillière, Tindall & Cox, 1934. 13*s.* 6*d.* net.)

THE researches described in this volume, No. V in the New Series of publications of the History of Science Society, form one of the most important contributions made in recent times to the literature of the history of science. After a painstaking, detailed and unhurried study of masses of original documents, letters and manuscripts, published and unpublished, and other material in London, Oxford, Paris and Leiden, Dr. Brown has been able to present from these rich sources an extraordinarily vivid picture of the scientific movement in 17th century France: and, since much of the material to which he has had access has not previously been made use of, his book can fairly claim to be the most authentic work on this period. Essentially, the book is "a history of some of the private assemblies of the amateurs of natural science in seventeenth century France," of those minor associations whose existence has long been known, but whose work and historical significance has not previously been studied. As Dr. Brown writes: "From them came a multitude of tracts, essays, collected discourses, periodical publications; taken collectively, they are the source of the bulk of the popular scientific literature of seventeenth-century France. The history of these groups lies in the archives and the manuscript collections of the libraries of western Europe. For their own sake, and because they were the heirs of the Platonic tradition of free assembly and corporate activity outside political control, their story seems worth telling." And so he proceeds to tell this hitherto untold story, lengthy quotations being interspersed to "flavour the mass of undistinguished detail with the spice of personalities," describing the services of de Peiresc and the brothers Dupuy, the *Cabinet des frères Dupuy*, Renaudot and Mersenne, the Montmor Academy, the *Compagnie des Sciences et des Arts*, the *conférences* of Justel, the academies of the provinces and the academy of the Abbé Bourdelot. Dr. Brown has also studied very carefully the scientific relations between France and England during this period, and disposes very thoroughly of the tradition that the Royal Society of London was modelled on a French original.

The book is documented throughout and includes a bibliography, a summary of manuscript sources, and an appendix giving some typical examples of unpublished material. Dr. Brown is to be congratulated on the production of a very remarkable book.

D. McKie.

MISCELLANEOUS

Working Dogs. By Elliott Humphrey and Lucien Warner. With a Foreword by Raymond Pearl. [Pp. xiv + 253, with 52 figures.] (Baltimore: The Johns Hopkins Press; London: Humphrey Milford, 1934. 16s. net.)

For the last 10,000 years man and dog have evolved together. And there is good reason to think that during the last part of this period the dog, at least, has degenerated. Those of us who are not acquainted with sheep dogs probably underestimate the dog's possible attainments. For most sporting dogs are highly specialised for a particular task, and other breeds have little chance to display such intellectual faculties as they may possess.

The book before us tells a more cheerful story. At Fortunate Fields, in Switzerland, dogs are being bred and educated as leaders of the blind, and for police and army work. The breed chosen was the German Shepherd dog. The numerous anecdotes given leave no doubt of the energy, fidelity, and intelligence of these animals.

To the biologist perhaps the most interesting data are those given in Chapter X on the inheritance of various traits. For while most geneticists who interest themselves in psychology are concerned with rather crude characters such as "tameness" or capacity for learning a particular maze, the breeders of these dogs are dealing with traits such as "willingness," "intelligence" and "energy" which are analogous to those considered desirable in man. Indeed the character of the ideal dog is very like that of the ideal citizen of Germany or Italy to-day. It is therefore worth noting that selection for sensory acuity, willingness to learn, and so on, has led to a definite falling off in initiative.

While there was strong evidence for the independent inheritance of auditory sensitivity, tactile sensitivity, distrust, willingless to learn, and so on, it is a striking fact that no correlation between parent and offspring as regards intelligence has been found. This may mean that all these dogs have much the same make-up as regards genes determining intelligence, or that intelligence, so far as genetically determined, depends on heterozygosis, in which case it would be futile to select for it. But in any case the fact is of interest to students of eugenics. The differences between the sexes are also of great interest. The males appear to be definitely better at police work and the females at blind guiding, and the females are on the whole definitely more intelligent.

The book is written in a semi-popular style, and the data on breeding are not given in any detail. We may hope that some day a highly trained geneticist may be permitted to work up the records. But even so no geneticist can possibly read this book without a feeling of delight that we are at last learning something of the inheritance of the higher psychological characters.

The general biologist should be glad that here, at last, man is directing evolution along the lines which presumably have been the psychological correlates of the expansion of the mammalian brain since the Cretaceous.

And if, as some students of evolution believe, this process is directed, it may be that the Director will judge mankind not by its science, art or religion, but by such secondary creations as the rose and the dog. If this is the case the authors of this book and Mrs. Eustis, who started the breeding programme, have done something to earn the Director's thanks.

J. B. S. H.

The Preservation of Antiquities. By H. J. PLENDERLEITH, M.C., B.Sc., Ph.D., F.R.S.E. [Pp. viii + 71, with 2 plates and 5 figures.] (London: The Museums Association, 1934. *2s. 6d.*)

THIS is an excellent little book by a specialist published at a low price.

The book is divided into three chapters. The first chapter commences with a short consideration of the conditions of storage of organic materials (leather, textiles, wood, ivory and bone) as affecting the growth of moulds and attacks by insects and explains how these pests may be prevented or combated. The methods of cleaning and preserving the materials are then given.

The second chapter, which in the "Contents" is headed "Siliceous Materials," deals with the cleaning and conservation of stone, earthenware, clay, shale, glass, glaze and enamel, several of which are not siliceous, but this slip is partly rectified later by the chapter itself being entitled "Siliceous Materials, etc."

The third chapter, which treats of metals, commences with a description of the nature of metallic corrosion, notes on both chemical and electrochemical action and practical considerations affecting treatment. The metals and alloys, iron, steel, copper, bronze, silver, electrum, gold, lead, and pewter are then dealt with separately.

No book is, or can be, perfect and a few minor points, that in no way detract from the value of the book, may be mentioned. Thus it is suggested that in the next edition (and that many future editions will be called for is certain) the fact that the vapour of carbon tetrachloride has an anæsthetic action and may have fatal effects, if inhaled in quantity, should be mentioned. Also, neat's-foot oil, although much employed as a dressing for leather, is always very acid, and it is suggested that sperm oil might be used instead, if oil, other than castor oil, is required. In connection with alabaster, mention might be made of the fact that Egyptian alabaster is calcium carbonate (calcite) and not calcium sulphate (gypsum), since this knowledge is of importance in cleaning.

The book is very attractively produced and the author, the Oxford Press and the Museums Association are all to be congratulated.

A. LUCAS.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

The Differential Calculus. By Theodore Chaundy, Student and Tutor of Christ Church, Oxford. Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. (Pp. xiv + 459, with 7 figures.) 35*s*. net.

The Natural Logarithm. By Sir Charles Vernon Boys, A.R.S.M., LL.D., F.R.S., Fellow of the Imperial College. London: Wightman & Co., Ltd., 1935. (Pp. 32, with 6 figures.) 2*s*.

New Pathways in Science. By Sir Arthur Eddington, M.A., D.Sc., LL.D., F.R.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge. Messenger Lectures 1934. Cambridge: at the University Press, 1935. (Pp. x + 333, with 4 plates.) 10*s*. 6*d*. net.

Relativity, Gravitation and World-Structure. By E. A. Milne, M.A., D.Sc., F.R.S., Rouse Ball Professor of Mathematics and Fellow of Wadham College, Oxford. The International Series of Monographs on Physics. Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. (Pp. x + 365, with 4 plates and 21 figures.) 25*s*. net.

Through My Telescope. Astronomy for All. By W. T. Hay, F.R.A.S. With an Introduction by Sir Richard Gregory, Bart., F.R.S., F.R.A.S. London: John Murray, 1935. (Pp. xiv + 128, with 7 plates and 17 figures.) 3*s*. 6*d*. net.

Il Planetario Tascabile Hoepli. By Comte. A. Tosi and Dott. G. L. Andrissi. With instructions for use. Milan: Ulrico Hoepli, 1935. L.20.

A Manual of the Principles of Meteorology. By R. Mountford Deeley, M.Inst.C.E., M.I.Mech.E., F.G.S., F.R.Met.S., F.R.G.S., F.S.G., Late Locomotive Superintendent and Electrical Engineer, Midland Railway Co. London: Charles Griffin & Co., Ltd., 1935. (Pp. xii + 285, with frontispiece, 134 figures, including 3 plates, and 27 tables.) 15*s*. net.

Through the Weather House, or The Wind, the Rain, and six hundred miles above. By R. A. Watson Watt. London: Peter Davies, Ltd., 1935. (Pp. xii + 192, with 8 plates.) 7*s*. 6*d*. net.

Reports on Progress in Physics. London: The Physical Society, 1934. (Pp. iv + 371, with 2 plates and numerous figures.) 12*s*. 6*d*. net.

International Conference on Physics, London 1934. Papers and Discussions. Vol. 1: Nuclear Physics. Vol. 2: The Solid State of Matter. London: The Physical Society, 1935. (Vol. 1: pp. viii + 257, with 1 plate and 83 figures; Vol. 2: pp. viii + 183, with 62 figures.) 10*s*. each.

International Conference on Physics, London 1934. Reports on Symbols, Units and Nomenclature. London: The Physical Society, 1935. (Pp. vi + 40.) *2s. 9d.*

Sound. A Physical Text-Book. By E. G. Richardson, B.A., Ph.D., D.Sc., Lecturer in Physics, Armstrong College, Newcastle-on-Tyne. Second edition. London: Edward Arnold & Co., 1935. (Pp. viii + 319, with 111 figures.) *15s.* net.

A Symposium on Illumination. Edited by C. J. Webber Grieveson, B.Sc., M.A.(Oxon.). With a Foreword by Lt.-Col. Kenelm Edgcumbe, M.Inst.C.E., M.I.E.E. London: Chapman & Hall, Ltd., 1935. (Pp. xvi + 229, with 109 figures, including 20 plates.) *13s. 6d.* net.

The World of Colour. By David Katz, Dr.Phil., formerly Professor of Psychology and Education and Director of the Psychological Laboratory, University of Rostock. Translated from the German by R. B. MacLeod, Ph.D., and C. W. Fox, Ph.D. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1935. (Pp. xvi + 300, with 12 figures, including 1 plate.) *15s.* net.

Electron Emission and Adsorption Phenomena. By J. H. de Boer. Translated from the manuscript by Mrs. H. E. Teves-Acly. The Cambridge Series of Physical Chemistry. Cambridge: at the University Press, 1935. (Pp. xii + 398, with 150 figures.) *21s.* net.

Electrons (+ and −), Protons, Photons, Neutrons, and Cosmic Rays. By Robert Andrews Millikan, Director, Norman Bridge Laboratory of Physics, California Institute of Technology. Cambridge: at the University Press, 1935. (Pp. x + 492, with 98 figures.) *15s.* net.

Fine Structure in Line Spectra and Nuclear Spin. By S. Tolansky, Ph.D., D.I.C., A.Inst.P. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1935. (Pp. viii + 112, with 24 figures.) *3s.* net.

Introduction to Electric Transients. By Edwin B. Kurtz, E.E., Ph.D., Professor and Head of Electrical Engineering Department, and George F. Corcoran, M.S., Associate Professor of Electrical Engineering, the State University of Iowa. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xvi + 335, with 86 figures and 108 oscillograms.) *22s. 6d.* net.

The Principles of Electric Power Transmission by Alternating Currents. By H. Waddicor, B.Sc., A.M.I.E.E. Third edition. London: Chapman & Hall, Ltd., 1935. (Pp. xxii + 449, with 164 figures.) *21s.* net.

Modern Surveying for Civil Engineers. By Harold Frank Birchal, O.B.E., D.F.C., late Chief Construction Engineer to the Kenya and Uganda Railways. London: Chapman & Hall, Ltd., 1935. (Pp. xii + 524, with 14 folding plates and 382 figures, including 12 plates.) *25s.* net.

Practical Solution of Torsional Vibration Problems. With Examples from Marine, Electrical and Automobile Engineering Practice. By W. Ker Wilson, M.Sc.(Eng.), London, Wh.Ex. London: Chapman & Hall, Ltd., 1935. (Pp. xviii + 438, with frontispiece and 106 figures.) *25s.* net.

The Elements of Practical Flying. A Detailed Survey for Students and Air Pilots. By P. W. F. Mills. London: The Technical Press, Ltd., 1935. (Pp. viii + 133, with 6 figures.) *4s. 6d.* net.

A Recording Manometer Having Low Inertia. By G. Allsop and H. Lloyd. Safety in Mines Research Board Paper No. 91. London: H.M. Stationery Office, 1935. (Pp. 20, with 2 plates and 16 figures.) 1*s.* net.

Inorganic and Theoretical Chemistry. By F. Sherwood Taylor, Ph.D., M.A., B.Sc., Assistant Lecturer, Queen Mary College, University of London. Third edition. London: William Heinemann, Ltd., 1935. (Pp. xiv + 832, with 19 plates and 200 figures.) 12*s.* 6*d.*

A Progressive School Chemistry. School Certificate Course. By J. M. Harrison, M.A., Senior Science Master, Bristol Grammar School. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. xii + 343, with 68 figures.) 4*s.* 6*d.*

Optical Rotatory Power. By T. Martin Lowry, C.B.E., M.A., D.Sc., F.R.S., Professor of Physical Chemistry in the University of Cambridge. Textbooks of Physical Chemistry. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. xiv + 483, with 186 figures.) 30*s.* net.

Crystal Chemistry. By Dr. O. Hassel, Lecturer in Physical Chemistry in the University of Oslo. Translated from the German by R. C. Evans, B.A., Ph.D., B.Sc., Demonstrator in Mineralogy and Petrology in the University of Cambridge. London: William Heinemann, Ltd., 1935. (Pp. xii + 94, with 8 figures.) 6*s.*

An Introduction to the Modern Theory of Valency. By J. C. Speakman, M.Sc., Ph.D., Lecturer in Chemistry at the University of Sheffield. London: Edward Arnold & Co., 1935. (Pp. viii + 157.) 4*s.* 6*d.* net.

Physical Chemistry for Students of Biology and Medicine. By David Ingersoll Hitchcock, Ph.D., Associate Professor of Physiology in the Yale University School of Medicine. Second edition. Baltimore: Charles C. Thomas; London: Baillière, Tindall & Cox, 1934. (Pp. xii + 214, with 28 figures.) 12*s.* 6*d.* net.

Colloidal Electrolytes. A General Discussion held by the Faraday Society, September 1934. London: Gurney & Jackson for the Society, 1935. (Pp. iv + 422, with frontispiece and numerous figures.) 18*s.* 6*d.* net.

The Nitrogen System of Compounds. By Edward Curtis Franklin, Stanford University, California. American Chemical Society Monograph Series, No. 68. New York: Reinhold Publishing Corporation, 1935. (Pp. 339, with 26 figures.) $7.50.

Metallurgy. An Elementary Text-Book. By E. L. Rhead, M.Sc.Tech., F.I.C., A.I.M.E., formerly Lecturer on Metallurgy at the Municipal College of Technology and in the University of Manchester. Sixth edition. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. xiv + 382, with 182 figures.) 10*s.* 6*d.* net.

Thorpe's Dictionary of Applied Chemistry. Supplement: Vol. II. N–Z. By Jocelyn Field Thorpe, C.B.E., D.Sc., F.R.S., F.I.C., Professor of Organic Chemistry and Director of Organic Laboratories, Imperial College of Science and Technology, and M. A. Whiteley, O.B.E., D.Sc., F.I.C. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. xx + 727, illustrated.) 60*s.* net.

Shellac. Its Production, Manufacture, Chemistry, Analysis, Commerce and Uses. By Ernest J. Parry, B.Sc.(Lond.), F.I.C., F.C.S., of Gray's Inn,

Barrister-at-Law. London: Sir Isaac Pitman & Sons, Ltd., 1935. (Pp. xii + 240, with frontispiece and 15 figures.) 12*s*. 6*d*. net.

British Chemicals and their Manufacturers. The Official Directory of the Association of British Chemical Manufacturers. London: The Association of British Chemical Manufacturers, 1935. (Pp. 459.) Free to buyers of chemicals on application to the publishers.

Van Nostrand's Chemical Annual. Edited by John C. Olsen, A.M., Ph.D., D.Sc., Professor of Chemical Engineering, Polytechnic Institute, Brooklyn. Assistant Editor, B. Whitney Ferguson, Brooklyn Technical High School. Seventh issue. London: Chapman & Hall, Ltd., 1935. (Pp. xviii + 1029, with frontispiece, charts and graphs.) 25*s*. net.

A German-English Dictionary for Chemists. By Austin M. Patterson, Ph.D., Vice-President and Professor of Chemistry, Antioch College. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xx + 411.) 15*s*. net.

Structural Geology. With Special Reference to Economic Deposits. By Bohuslav Stočes and Charles Henry White. London: Macmillan & Co., Ltd., 1935. (Pp. xvi + 460, with frontispiece and 663 figures.) 25*s*. net.

Earthquakes and Mountains. By Harold Jeffreys, M.A., D.Sc., F.R.S. London: Methuen & Co., Ltd., 1935. (Pp. xii + 183, with 6 plates and 9 figures.) 7*s*. 6*d*. net.

Primitive Land Plants, also known as the Archegoniatæ. By F. O. Bower, Sc.D., LL.D., F.R.S., Emeritus Professor of Botany in the University of Glasgow. London: Macmillan & Co., Ltd., 1935. (Pp. xiv + 658, with frontispiece and 449 figures.) 30*s*. net.

Diseases of the Banana and of the Manila Hemp Plant. By C. W. Wardlaw. Ph.D., D.Sc., F.R.S.E., Pathologist for Banana Research, Imperial College of Tropical Agriculture, Trinidad. London: Macmillan & Co., Ltd., 1935. (Pp. xii + 615, with frontispiece and 295 figures.) 30*s*. net.

An Index to the Genera and Species of the Diatomaceæ and their Synonyms, 1816–1932. Part XXI: Sy–Z, Appendices and Supplement. Compiled by Frederick Wm. Mills, F.L.S., F.R.M.S. London: Wheldon & Wesley, Ltd., 1935. (Pp. 1571–1726, with frontispiece.) 10*s*.

Management of American Forests. By Donald Maxwell Matthews, B.A., M.S.F., Professor of Forest Management, School of Forestry and Conservation, University of Michigan. American Forestry Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xvi + 495, with 22 figures.) 30*s*. net.

Elementary Microtechnique. By H. Alan Peacock, M.Sc., Head of the Biology Department, Cheltenham Grammar School. London: Edward Arnold & Co., 1935. (Pp. viii + 200, with 15 figures.) 5*s*. 6*d*. net.

Cold Spring Harbor Symposia on Quantitative Biology. Vol. II. Cold Spring Harbor, L.I., N.Y.: The Biological Laboratory, 1934. (Pp. xii + 284, illustrated.)

The Wasp. The Genus Vespa in Scotland. By W. B. R. Laidlaw, D.Sc., F.R.E.S. Edinburgh: John Baxter & Son, Ltd., 1934. (Pp. 32, with 3 coloured and 5 black and white plates.) 3*s*.

Lac and the Indian Lac Research Institute. By Dorothy Norris, M.Sc., F.I.C., P. M. Glover, B.Sc., and R. W. Aldis, Ph.D., D.I.C. Nankum, Dt. Ranchi, Bihar and Orissa: Indian Lac Research Institute, 1934. (Pp. vi + 53, with 12 plates.) Rs. 2/8.

Fishes. Their Ways of Life. By Louis Roule, Professor at the National Museum of Natural History in Paris. Translated from the French by Conrad Elphinstone. London: George Routledge & Sons, Ltd., 1935. (Pp. viii + 312, with 52 figures.) 12*s.* 6*d.* net.

Phytoplankton and the Herring. Part I. 1921 to 1932. By R. E. Savage, A.R.C.Sc., D.I.C., Fisheries Laboratory, Lowestoft, and A. C. Hardy, M.A., University College, Hull. Ministry of Agriculture and Fisheries, Fishery Investigations, Series II, Vol. XIV, No. 2. London: H.M. Stationery Office, 1935. (Pp. 73, with 42 figures and 31 tables.) 3*s.* 6*d.* net.

The Hake and the Hake Fishery. Being the Buckland Lectures for 1934. By C. F. Hickling, M.A., Assistant Naturalist on the Staff of the Ministry of Agriculture and Fisheries. London: Edward Arnold & Co., 1935. (Pp. 142, with frontispiece and 13 figures.) 3*s.* 6*d.* net.

School of Biology. By Curt Thesing, M.D. Translated by Eden and Cedar Paul. London: George Routledge & Sons, Ltd., 1935. (Pp. xii + 368, with 91 figures and 1 table.) 15*s.* net.

Epidemics and Crowd-Diseases. An Introduction to the Study of Epidemiology. By Major Greenwood, D.Sc., F.R.C.P., F.R.S., Professor of Epidemiology and Vital Statistics in the University of London. London: Williams & Norgate, Ltd., 1935. (Pp. 409, with 8 graphs and 74 tables.) 21*s.* net.

Respiration. By J. S. Haldane, C.H., M.D., F.R.S., Fellow of New College, Oxford, and J. G. Priestley, M.C., D.M., M.A., Reader in Clinical Physiology in the University of Oxford. Second edition. Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. (Pp. xiv + 493, with 124 figures, including 17 plates.) 30*s.* net.

The Biochemistry of the Eye. By Arlington C. Krause, M.A., Ph.D., M.D., F.A.I.C., Instructor in Ophthalmology, The Johns Hopkins Medical School. The Wilmer Ophthalmological Institute, Monograph No. 2. Baltimore: The Johns Hopkins Press; London: Humphrey Milford, 1934. (Pp. xvi + 264, with 17 figures.) 15*s.* net.

Furneaux's Human Physiology. Revised edition by William A. M. Smart, M.B., B.S., B.Sc., M.R.C.S., L.R.C.P., Senior Demonstrator of Physiology, London Hospital Medical College. Nurses' edition. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. viii + 348, with 210 figures.) 4*s.*

Viruses and Heterogenesis. An Old Problem in a New Form. By Sir Henry H. Dale, C.B.E., M.D., F.R.C.P., F.R.S. Huxley Memorial Lecture, 1935. London: Macmillan & Co., Ltd., 1935. (Pp. 24.) 1*s.* net.

Individual Health. A Technique for the Study of Individual Constitution and its Application to Health. By E. Obermer. Vol. I: Biochemical Technique. By E. Obermer and R. Milton. London: Chapman & Hall, Ltd., 1935. (Pp. xvi + 244, with 4 folding plates, 22 graphs and 40 figures, including 12 plates.) 15*s.* net.

Psychology and Health. By H. Banister, M.Sc., Ph.D., Director of Psychological Studies in St. John's College, Cambridge. Cambridge: at the University Press, 1935. (Pp. viii + 256.) 7*s*. 6*d*. net.

The Schoolboy. A Study of his Nutrition, Physical Development and Health. By G. E. Friend, M.R.C.S., L.R.C.P., Medical Officer of Christ's Hospital, Horsham. With a Foreword by Professor J. C. Drummond, D.Sc., F.I.C. Cambridge: W. Heffer & Sons, Ltd., 1935. (Pp. xii + 128, with 31 figures and 36 tables.) 7*s*. 6*d*. net.

The Two Sources of Morality and Religion. By Henri Bergson. Translated by R. Ashley Audra and Cloudesley Brereton, with the assistance of W. Horsfall Carter. London: Macmillan & Co., Ltd., 1935. (Pp. viii + 278.) 10*s*. net.

Essay on the Foundation of Cognition. By C. Lambek. Translated from the Danish by Agnete Kortsen. Copenhagen: Levin & Munksgaard; London: Williams & Norgate, 1935. (Pp. 104.) 4*s*. 6*d*. net.

Science and the Human Temperament. By Erwin Schrödinger, Professor of Theoretical Physics at the University of Berlin, Temporary Professor at the University of Oxford. Translated and with a Biographical Introduction by James Murphy. With a Foreword by Lord Rutherford of Nelson, O.M., F.R.S. London: George Allen & Unwin, Ltd., 1935. (Pp. 154, with 13 figures.) 7*s*. 6*d*. net.

Origins and Development of Applied Chemistry. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry in the University of London, Queen Mary College. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. xii + 597.) 45*s*. net.

A History of Science, Technology, and Philosophy in the 16th and 17th Centuries. By A. Wolf, Professor and Senator, University of London. With the co-operation of Dr. F. Dannemann and A. Armitage. History of Science Library. London: George Allen & Unwin, Ltd., 1935. (Pp. xxviii + 692, with 316 illustrations, including 67 plates.) 25*s*. net.

From Galileo to Cosmic Rays. A New Look at Physics. By Harvey Brace Lemon, Ph.D., Professor of Physics, the University of Chicago. U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1934. (Pp. xviii + 450, with frontispiece and numerous illustrations.) 17*s*. 6*d*. net.

British Scientists of the Nineteenth Century. By J. G. Crowther. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1935. (Pp. xii + 332, with 12 plates.) 12*s*. 6*d*. net.

Darwinism and its Critics. By Sir Arthur Keith, M.D., F.R.S. The Forum Series, No. 20. London: Watts & Co., 1935. (Pp. viii + 56, with frontispiece.) 1*s*. net.

Publications of the Institute of British Geographers. No. 1: Transactions, and No. 2: The Pastoral Industries of New Zealand. By R. Ogilvie Buchanan. London: George Philip & Son, Ltd., 1935. (Pp. xvi + 99, with 9 graphs and 12 maps.) 7*s*. 6*d*. net.

Publication No. 3 of the Institute of British Geographers. The Changing Sea Level. By Henri Baulig. London: George Philip & Son, Ltd., 1935. (Pp. xii + 46, with 11 figures.) 3*s*. net.

Primitive Society and its Vital Statistics. By Ludwik Krzywicki, Ph.D., Professor of Social History in the University of Warsaw. London: Macmillan & Co., Ltd., 1934. (Pp. xvi + 589.) 20*s*. net.

Sa Torreta. By Margaret A. Murray, D.Lit., F.S.A.(Scot.), F.R.A.I., Fellow of University College, London. With Chapters by T. Balakrishnan Nayar, M.A., and John Cameron, M.D. Cambridge Excavations in Minorca, Part 2. London: Bernard Quaritch, Ltd., 1934. (Pp. 86, with 4 figures and 50 plates.) 20*s*. net.

The Identification of Firearms from Ammunition Fired Therein. With an Analysis of Legal Authorities. By Jack Disbrow Gunther, A.B., LL.B., Member of the New York State Bar, and Charles O. Gunther, M.E., Professor of Mathematics, Stevens Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xxviii + 342, with frontispiece and 148 figures.) 20*s*. net.

The Frustration of Science. By Sir Daniel Hall, F.R.S., J. G. Crowther, J. D. Bernal, Professor V. H. Mottram, Dr. Enid Charles, Dr. P. A. Gorer, and Professor P. M. S. Blackett, F.R.S. With a Foreword by Frederick Soddy, M.A., LL.D., F.R.S. London: George Allen & Unwin, Ltd., 1935. (Pp. 144.) 3*s*. 6*d*. net.

Rats, Lice and History. By Hans Zinsser. London: George Routledge & Sons, Ltd., 1935. (Pp. xii + 301.) 10*s*. 6*d*. net.

Studies in Psycho-Physics. By Jacques J. Cohen. London: John Bale, Sons & Danielsson, Ltd., 1935. (Pp. viii + 194, with 1 figure.) 5*s*. net.

Department of Scientific and Industrial Research. Report for the year 1933–34. London: H.M. Stationery Office, 1935. (Pp. iv + 192.) 3*s*. net.

A First Course in Educational Statistics. By Edna E. Kramer, Ph.D., Assistant Professor of Mathematics, New Jersey State Teachers College. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. x + 212, with 50 figures.) 12*s*. 6*d*. net.

Education in India in 1932–33. Delhi: Manager of Publications, 1935. (Pp. vi + 117.) Rs.2/8 or 4*s*. 6*d*.

SCIENCE PROGRESS

SELECTIONS FROM THE STORY OF PLANT MIGRATION REVEALED BY FOSSILS

By A. C. SEWARD, Sc.D., LL.D., F.R.S.
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Introductory

Twelve years before the publication of the *Origin of Species* Darwin spoke of Geographical Distribution as "that noble subject of which we as yet but dimly see the full bearing." Twenty-nine years later he wrote to Wallace: "What progress palæontology has made during the last twenty years; but if it advances at the same rate in the future our views on the migration and birthplace of the various groups will, I fear, be greatly altered." Encouraged by the Editor of this Journal, I have attempted to give a general account of some of the plant-records of the rocks in illustration of their contribution towards a fuller appreciation of the numerous and baffling problems inseparable from a comprehensive study of Geographical Distribution. My aim is to demonstrate the importance of supplementing a study of plant-distribution in the world as we know it at the present time by taking into account such information as can be extracted from the herbaria of the rocks, particularly those which contain samples of the later phases of vegetation. It is obvious that we cannot understand the present unless we keep before us the fact that the face of the earth, as seen at this or at any other time, is simply a single, transitory manifestation, which endures but for a moment, of a continuous series of events. The tendency to concentrate attention upon the living plant-world without reference to the vanished floras of earlier ages inevitably leads to conclusions that are based on incomplete and therefore misleading premises. By confining attention to a single scene in a drama we lose our sense of proportion and fail to appreciate the significance of what we see. Information supplied by the fragmentary documents preserved in the earth's crust reveals unexpected contrasts between the present and, in a geological

sense, a recent past; it may not provide the key to many problems; it often raises fresh difficulties, but it compels us to take a broader view and enables us to see the modern plant-world in truer perspective and with clearer vision. Palæobotanical evidence provides clues to a possible solution of some of Nature's riddles; it encourages us to look backward in our search for explanations of the facts and anomalies furnished by a study of the present distribution of plants. The documents are not only fragmentary; they are lamentably insufficient, and yet they are well worthy of our attention. As our imagination is quickened the lifeless *disjuncta membra* embedded in upraised sediments of estuaries and deltas become parts of living trees; a remote past is transformed into a living present. As Darwin said: "The noble science of Geology loses glory from the extreme imperfection of the record. The crust of the earth with its embedded remains must not be looked at as a well-filled museum but as a poor collection made at hazard and at rare intervals." The distribution of plants, as Guppy said, is "indissolubly connected with a past of which unfortunately we know very little." Meagre as the botanical records undoubtedly are, they are ever increasing; and, though we can never expect to see clearly the forests of other days, the mists hiding far-off scenes are being partially dispelled.

My present purpose will be served if I am able to show with the help of a few examples selected from the available material that the little we know of the geological history of plants is enough to create a desire to know more. Such facts as are available enable us to follow in some measure the migrations of plants over regions from which they and their descendants have long since disappeared. From the rocks and the fossils they contain it is possible to see dimly the landscapes of an older world; to compare them with the modern expression on the face of nature, and to realise the fascination of contrasting what was and what is, as we reconstruct the changing geological background, the environment, and the composition of successive forests. No attempt is made in this article to present complete series of records; that would involve references to a voluminous literature and would be out of place in a general, illustrative sketch. The examples chosen are intended to be preliminary to a more detailed and thorough enquiry, and it is hoped that they may encourage others to pursue a line of research which is much less unfruitful than is often supposed. It is not surprising that botanists, after a casual inspection of descriptions and illustrations of fossil plants, form the opinion that the scraps of foliage and other remains are so much inferior to specimens taken

from living plants that they are worthless to students accustomed to very different sources of information. On the other hand, despite the fact that palæobotanists have not infrequently allowed enthusiasm to get the better of their sense of proportion—a tendency which they share with colleagues in other branches of science—there is no lack of material awaiting critical investigation which is of great value and of absorbing interest.

The Age and Area Hypothesis

Before passing to the consideration of living and extinct genera and species it is fitting that brief reference should be made to a method of approaching the subject of plant distribution that is based on statistical methods. Several years ago Dr. J. C. Willis, desirous, as he says, of freeing himself "from the trammels of the natural selection theory," took up the study of distribution, in which he had always taken much interest, and reached the conclusion that "Age as an explanation of spread is enormously simpler than natural selection and that it is probably valid is shown by the way in which it can be used for prediction." This means that the greater the spread or, in other words, the larger the area within which groups of plants occur, the greater their age. As he pointed out in his book *Age and Area* (1922) the idea conveyed by the statement—"the older the species is, the more area will it have had time to cover" is not new: his contribution was to carry it much farther than anyone else had done, and stress the importance of applying a statistical method to the interpretation of the facts of geographical distribution. The rule of *Age and Area* is thus stated: "The area occupied at any given time, in any given country, by any group of allied species at least ten in number, depends chiefly, so long as conditions remain reasonably constant, upon the ages of the species of that group in that country, but may be enormously modified by the presence of barriers such as seas, rivers, mountains, changes of climate from one region to the next, or other ecological boundaries, and the like, also by the action of man, and by other causes." Dr. Willis believes that the great majority of endemic species, that is species confined to a more or less isolated locality, *e.g.* on an island or a mountain-chain, are young beginners which have not had time to spread. Mrs. Reid, in a chapter contributed to *Age and Area*, wrote: "It is this fact, that endemic species can frequently be proved to be survivors from a wide-ranging past, which offered to me the greatest stumbling-block to the acceptance of the theory." She does not, however, think that the palæobotanical evidence vitiates the general sound-

ness of Dr. Willis's view. The author of the theory accepts a very small minority of endemics as relicts; they form, he says, only about one per cent. of the total number, and Mrs. Reid, accepting this estimate, adds: "Consequently, though palæobotany is right to hold to its one per cent., it must yield place in the argument to the superior force of numbers." This point of view is open to objection: the number of species that now occupy restricted areas and are known to be relicts is too great to admit of their rejection as an argument against the truth of the *Age and Area* hypothesis: the estimate of 1 per cent. is unquestionably much too low and seriously misleading. It is undeniable and indeed self-evident that if, as Dr. Willis suggests, the factors acting upon dispersal of plants produce in the long run a very uniform effect, then age may be accepted as a measure of dispersal.

On the assumption that factors conditioning dispersal are in the main uniform in their effects, ten genera or species, the number adopted by Dr. Willis as his standard, that have a far-flung distribution (they are what are called wides) would be older than those which have established themselves within a more confined area. If we consider species simply as living members of the plant-world we regard the present without reference to the past, and it is this aspect of *Age and Area* that is germane to my immediate purpose. The theory may fit things as they are, and there can be no doubt that in many instances it does; but my contention is that it fails to visualise stages in the history of the plant-world antecedent to the present. In citing the following examples I admit that I am not following Dr. Willis's rule in taking a group of species as the limit, but that does not invalidate the significance of the facts.

The bracken fern (*Pteridium aquilinum*) affords one of the most remarkable instances of a cosmopolitan plant; it flourishes in the climate of this country, on the lower slopes of Mt. Kenya on the edge of tropical Africa, in South Africa, North and South America, the Fiji islands, Australia and many other parts of the world. It should, therefore, on Dr. Willis's view be an old representative of the family to which it belongs, a vigorous wanderer that has taken full advantage of the time at its disposal. According to the standards set by Dr. Willis it must be old because it has spread over a large proportion of the earth's surface; but is it old as geologists reckon age? The palæobotanical record gives little information of a positive kind; it is chiefly negative. But negative evidence is by no means negligible. In view of the abundance of many other fossil ferns we can with reasonable confidence assert that there are no grounds for regarding *Pteridium* as an old type;

on the contrary it is almost certainly a comparatively recent product of evolution. In view of the abundance in sedimentary rocks of fronds of many other ferns, the marked scarcity of fossils that can be matched with the Bracken is in all probability more than a mere accident. This raises the question: is it safe to assume that the wide geographical distribution of living plants connotes youthful vigour and a relatively recent origin rather than antiquity? While it is true in some instances, as I believe it to be for *Pteridium*, that cosmopolitanism is an expression of comparative modernity, it is certainly by no means always true. The Royal Fern, *Osmunda regalis*, ranges over a large territory and yet it is a representative of a family which stretches back to a remote past. It is, however, noteworthy that the greater number of fossil species of the Osmundaceæ appear to be more closely allied to the genus *Todea* than to *Osmunda*. *Todea*, which has a more restricted distribution, is a characteristic Fern in South Africa, temperate Australia, and New Zealand.

A few ferns will next be considered in support of the contention that genera which now occupy a relatively small area are among the most ancient of their race. The island of Juan Fernandez (rather more than 400 miles off the Chili coast) possesses several endemic ferns, species which occur nowhere else: one of these is *Thyrsopteris elegans*, the sole representative of the genus characterised by large and graceful fronds bearing on the lower arms clusters of spore-capsules in circular cups which replace the ordinary green leaflets and hang like miniature bunches of grapes.

Part of the Geological Table is inserted for convenience of reference to the Periods mentioned in the following pages.

QUATERNARY (or PLEISTOCENE). Present stage passing imperceptibly into the Post-glacial and Glacial (Ice Age) stage.

TERTIARY. Pliocene stage.
Miocene ,,
Oligocene ,,
Eocene ,,

CRETACEOUS. Upper Cretaceous (Chalk, etc.).
Lower Cretaceous (Wealden, etc.).

JURASSIC. The flowering plants were only beginning to play a part in the vegetation of the world.

RHÆTIC. A series of plant-beds, etc., intermediate between Triassic and Jurassic.

TRIASSIC.

PERMIAN, and older formations.

The older formations are not mentioned in the text.

Specimens of fertile fern fronds from the Jurassic rocks of Yorkshire and other parts of the world bear a striking resemblance to those of the Juan Fernandez species, which recalls Darwin's dictum: "Rarity, as Geology tells us, is the precursor to extinction." Two other genera of ferns are of special interest in this connection: the genus *Matonia*, with two or three species, grows abundantly in some parts of the Malay Peninsula, on the mountains of Borneo and on the coast of some of the Malayan islands: it is unknown in other parts of the world. *Matonia*, or at least Ferns having the same unusual form of frond, the same type of spore-capsules and the same exceptional structure of the stem, were members of the earliest Cretaceous and the still older Jurassic floras of Northern Europe. An extinct genus, *Laccopteris*, closely allied to *Matonia*, was almost cosmopolitan in the older Jurassic floras, ranging from Greenland to Indo-China and Australia and over a great part of Europe. The second genus, *Dipteris*, a companion of *Matonia* in Malaya, has a wider though by no means a wide area of distribution; it occurs in Assam, in New Guinea, Central China, Fiji, New Caledonia and in a few other localities. It was represented in Rhætic and Jurassic floras by a very similar Fern, *Dictyophyllum*, which flourished in Eastern Greenland, in Europe, North America, Tonkin and elsewhere. Another fossil Fern, *Hausmannia*, which is even more like *Dipteris*, grew in the Arctic regions, in England and other parts of Europe in the Cretaceous and Jurassic periods. Enough has been said to show that *Matonia* and *Dipteris* are survivals from a group of very ancient Ferns which once occupied a territory far surpassing in size the present distribution area and remote from their present homes. These genera and many others, that are now confined within comparatively narrow regions in Malaya and farther east, are the descendants of plants which in the later stages of the Mesozoic era were among the commoner members of northern hemisphere floras. The same statement is applicable with equal truth to flowering plants of the modern world as contrasted with ancestors in Cretaceous and Tertiary floras. It often happens that plants confined to one part of the world to-day, *e.g.* the southern tropics and sub-tropics, are survivals from the past, carrying us back in imagination to ancestors long dead which lived on a far-off northern continent. Driven by stress of circumstances from their old homes some perished by the way, some gained a refuge where they still remain as impressive, silent and yet eloquent witnesses of the continuity of the past and the present.

These examples are cited primarily in relation to the *Age and*

Area hypothesis; they are selected from a large number of equally or even more striking instances furnished by the rocks from which we learn that the present circumscribed area of distribution is by no means necessarily an indication of youth; it is not infrequently the expression of what may be called senescence, a sign of antiquity.

Ferns

We will now consider other contrasts in distribution without more than a passing reference to the problems which they raise. The family Gleicheniaceæ includes many Ferns which abound in the warmer regions of both the Old and the New World; certain species are among the most familiar plants in the southern tropics and are readily recognised by their regularly forked fronds with divergent arms closely set with narrow, linear, or small, almost circular leaflets bearing spore-capsules of a well-defined pattern. The genus *Gleichenia* no longer occurs in Europe and only in the southernmost part of North America. Its history has been traced through many ages, and there is some evidence of the existence of a similar type of Fern as far back as the late stages of the Palæozoic era. A remarkable fact is that Ferns practically identical with existing species of *Gleichenia*, in the peculiar construction of the frond and in the structure of the spore-capsules and spores, played a prominent part in the Cretaceous vegetation of western Greenland (lat. 70° N.) and in floras of approximately the same age in many parts of Europe. Specimens of *Gleichenia* (*Gleichenites*) have recently been described by Professor Harris in his exceptionally interesting account of an amazingly rich early Jurassic and Rhætic flora from material which he collected in the neighbourhood of Scoresby Sound in East Greenland. Gleicheniaceous Ferns are recorded from the older Cretaceous rocks in Britain and the European continent and there is some evidence pointing to the existence of a member of the family in England in an early Tertiary flora. The sudden display of plant-remains as a rock is split by the blow of a hammer may be welcomed as a satisfactory result of a particular quest; it may also stimulate imagination as one is brought face to face with the inexplicable facts of Nature. The sight of well-preserved fronds of Ferns with forked arms exposed on a slab of shale on the beach of Upernivik Island (71° N. lat.) took me back to a hill-side above Penang in the Malay Peninsula, where living Gleichenias in company with *Dipteris* formed a wonderful tangled carpet of luxuriant growth on the edge of a tropical forest. Thus can we bring to life a scene on an arctic Cretaceous continent and reconstruct, in part at least, the migration of a group of species in

time and space. Millions of years are ignored; ice-bound Greenland becomes a tropical or sub-tropical land; this natural reaction is, however, rather a fantasy than a deduction confirmed on more sober reflection after all the relevant evidence has been weighed.[1]

CONIFERS

Let us now briefly consider some examples furnished by Conifers. The Monterey Cypress (*Cupressus macrocarpa*), which is a familiar cultivated tree in England, lives in splendid isolation as an endemic species in Southern California; the bleached and twisted arms bear the impress of long exposure on the wind-swept granitic headlands facing the Pacific. The Cypress is strangely unlike its descendants or their progeny, raised a century ago from seed planted in English soil, the shapely pyramidal trees of our parks and gardens. These large-coned Cypresses, now confined within a domain not much more than 3 miles long and about 200 yards broad, are survivals, precariously clinging to life, from a remote past; they are certainly not young products of evolution which have lacked time to extend the boundary of their original home. We know the Cypress family formerly occupied a much larger territory: twigs and cones hardly distinguishable from those of the Monterey Cypress have been found in old Tertiary rocks in Northern Ireland, and similar fossils are recorded from the sands and muds of the Bournemouth cliffs. There are few, if any, more astonishing instances of trees that have come almost to the end of their wanderings along routes which we can only partially reconstruct.

Travelling a comparatively short distance north from Carmel Bay and the Monterey Peninsula we come to the southern limit of the interrupted belt of the Redwoods (*Sequoia sempervirens*) rather more than 400 miles in length, which ends a short distance beyond the boundary of northern California. The Redwood belt follows the line of the Coast Range hills bordering the Pacific coast. Farther east across the Central Valley a second species of *Sequoia* (*S. gigantea*), the big tree or mammoth tree of California, still lingers at a few localities on the Sierra Nevada. These two magnificent trees, taller than any others in the northern hemisphere and rivalled only by the Giant Gum trees (*Eucalyptus*) of Australia, appeal to us by their majestic form and even more as the sole living representatives of a genus that was one of the dominant members in the Tertiary forests of Europe and the Far East. Some of the Redwoods have been growing where they stand for over 3,000 years; at Calistoga,

[1] Fuller information on the past history of Ferns was given by the writer in the Hooker lecture published in 1922 (see References, p. 217).

70 miles north of San Francisco, one sees petrified giant stems reaching 8 feet in diameter lying in a bed of volcanic ash derived from some late Tertiary (Pliocene) volcano; and with the stems are cones and leafy shoots indistinguishable from those of the living Redwood. From older Tertiary rocks in Oregon beyond the limits of the present groves abundant specimens of a fossil *Sequoia* have been collected which are referred to a species, *S. Langsdorfii*, differing in no obvious characters from the living *S. sempervirens*. The same fossil species was a common tree in the Miocene floras of Western and Central Europe and in Tertiary forests in Siberia, Manchuria, and Japan; it flourished as far north as Ellesmere Land (about 78° N. lat.) and on the western edge of Tertiary Greenland. Another Tertiary *Sequoia* (*S. Couttsiæ*) is almost identical with the Mammoth tree (*Sequoia gigantea*), which as a cultivated tree is often spoken of as *Wellingtonia*; it also bears witness to the former abundance of the genus in Tertiary floras of the Old World; it grew in the Oligocene forests that have left fragmentary remains in the sediments of a vanished lake on the granitic plateau of Dartmoor at Bovey Tracey in Devonshire. Why, we wonder, is *Sequoia* now confined to a narrow strip of California? The two surviving species find favourable conditions on the parallel mountain ranges near the Pacific Coast, the Redwoods bathed by the mists from cumulus clouds, which in the long dry season roll eastwards from the ocean, while the Mammoth trees, fewer in number and almost verging on extinction, are able to endure the drier climate of the Sierra Nevada.

As the result of geological causes affecting the factors governing climate, particularly rainfall, the Cretaceous and Tertiary Sequoias were threatened with destruction; many succumbed, some few after wandering over wide stretches of the world reached the western sea-board of the New World.

The Redwoods and Mammoth trees in their splendid isolation offer many problems for the exercise of ingenuity and imagination. We think of their age-long ancestry, the vicissitudes which they have encountered during millions of years, and though we can only make guesses at the truth, even a slight knowledge of their past history enormously increases the feeling of awe inspired by their beauty, their strength, and length of years. Before leaving the Sequoias a word must be said on some recent palæobotanical contributions from Professor Chaney of the University of California, who has made an intensive study of Tertiary floras in Oregon in relation to the Redwood forests. One of his latest publications, in collaboration with Miss Sanborn, is on the Eocene Goshen Flora of West Central Oregon: the authors clearly demonstrate that the old

Western American flora bore a much closer resemblance to those of Europe than we find at the present day. They also show that the Goshen flora has a close affinity to the modern vegetation of Mexico and Central America. It is worthy of note that Chaney regards the evidence furnished by the Oregon flora as favourable to the old view of a centrifugal distribution of flowering plants from an Arctic home; several families and genera of the Goshen flora were represented in the Cretaceous vegetation of Alaska and Greenland. It is also apparent that the data supplied by Western American Tertiary floras give strong support to the view put forward many years ago by the Harvard botanist, the late Asa Gray, of a common origin of the vegetation on both sides of the Pacific.

Space does not admit of a fuller consideration of Professor Chaney's researches but attention is called to them because they illustrate in an unusual degree the value of a careful analysis of extinct floras and their comparison with existing floras in other regions, particularly in more southern parts of the same continental region. His results enable us to visualise the wanderings of plants from Arctic to sub-tropical lands and to appreciate how essential it is in dealing with modern plant geography to take into account the facts furnished by floras of the past.

Another Conifer which lives on the Sierra Nevada and the Californian Coast Range hills is also a link with Tertiary floras of the Old World: this is the Incense Cedar, *Libocedrus decurrens*, a species often cultivated in our gardens and characterised by its narrow tapering habit and flattened pairs of small scale-like leaves. The genus is often quoted as an example of discontinuous distribution; it is confined to the western border of the United States of America, western South America, from Chili to Patagonia, New Zealand, New Caledonia—famous as the home of many relicts and endemics—New Guinea, Formosa, and south-western China. As in other instances, its discontinuous geographical distribution is a mark of antiquity and is evidence of partial extinction of a group of species that once occupied a wide and continuous area. Perusal of lists of Tertiary fossils shows that numerous foliage-shoots with flat triangular leaves closely appressed to the stem and arranged in regular alternating pairs have been referred to *Libocedrus*; but in many instances it is impossible to distinguish one kind of *Cupressus*-like twig from another. Unless cones are present, or it is possible to obtain confirmatory evidence from microscopical characters, one cannot confidently assert that the fossils are derived from a *Libocedrus* rather than from a Cypress or a tree such as the *arbor vitæ*. Fortunately at several European localities cones associated with

the vegetative fragments afford proof of generic affinity. It is certain that trees differing in no clearly recognisable features from living species of *Libocedrus* grew in the Tertiary (Miocene) forests of northern Ireland, Germany and Poland. There can be no reasonable doubt that the genus had an extended range in Tertiary and possibly in still older Cretaceous floras, living well within the Arctic Circle and spreading thence, so one believes, over the greater part of Europe and across Asia to the Far East. This Conifer is one of a large company of trees whose present distribution is attributable to changing climate and physical conditions and to the effect of the last Glacial period in destroying the continuity of the west-to-east belt of Tertiary forests.

A similar story might be related of the Chinese Conifer, *Glyptostrobus*, another member of the Cypress family: this tree with a still more restricted range was once European and North American. Admitting that many of the published records of the occurrence of fossil species are valueless, there remain several well-preserved specimens in which the readily recognisable cones of the genus occur in organic connection with foliage-shoots like those of the Chinese tree. A good example of a cone-bearing branch of a Tertiary *Glyptostrobus* may be seen in the Hunterian Museum, Glasgow. The specimen is from a rich plant-bearing bed at Bilin in Bohemia. Other specimens have been described from Tertiary floras in Switzerland, France and elsewhere.

The Umbrella pine of Japan, *Sciadopitys verticillata*, though the fossil records are relatively meagre, is in all probability another ancient Conifer possibly derived from a northern source; there remains only a single species and that has many peculiar features marking it off as an isolated type, "alone among Coniferæ with no obvious affinities or immediate allies," which, "it must be conjectured, came down to us from a remote geological past."[1] The microscopical examination of carbonised leaves in Cretaceous and Tertiary rocks in Greenland and other Arctic countries has furnished evidence of affinity to *Sciadopitys*; and there is also a good deal of probability that detached cone-scales that are common in collections from Arctic and Temperate regions belonged to cones similar to those of the existing tree. We can at least say that Conifers agreeing closely with *Sciadopitys* were represented in northern Cretaceous and Tertiary forests.

There remains another genus of Conifers, *Araucaria*, which merits attention as an example of wanderings from one continent to another

[1] Quoted from Sir Wm. Thiselton-Dyer in Bean's *Trees and Shrubs* Vol. II, p. 507, 1919.

and of the difficulty of discovering the position of starting-points. The most familiar species in cultivation is the species *A. araucana* (= *A. imbricata*), the Monkey-puzzle from the Chilean Andes; a second widely cultivated species is *A. excelsa*, now confined to an island far to the east of Australia, from which the tree derives its name, the Norfolk Island Pine, though it is not a pine in the strict sense. In the vegetation of the world there are now about fifteen species of *Araucaria*, all of them south of the Equator, two in South America, Brazil, Chili and the Argentine, others in eastern Australia and in the islands beyond, including New Caledonia, Norfolk Island and the New Hebrides; there are also several species in New Guinea. Fortunately, the cones of existing Araucarias as well as the structure of the wood and the characters of the foliage-shoots make it possible, with reasonable certainty, to distinguish well-preserved remains of this generic type from those of other fossil Conifers. It is hardly possible to say when Araucarian trees were first represented in the older floras, a question of secondary importance in the present summary. The important point is that as far back as the Jurassic period *Araucaria* (or *Araucarites*) was a forest tree in Europe, in America, India, Graham Land on the edge of the Antarctic Continent, and in many other parts of the world. There is no satisfactory evidence of its presence in Jurassic, Cretaceous or Tertiary floras within the Arctic Circle. Good specimens of cone-scales each with a single seed, or the impression of a seed in the middle—an Araucarian feature—have been described from a Lower Cretaceous flora in South Africa, a continent where the genus no longer exists; also from rocks of the same period in England, France, Holland, Bohemia, Italy, and from several Cretaceous localities in the United States, in Australia and New Zealand. Without considering in more detail Rhætic, Jurassic, and Cretaceous localities which have yielded trustworthy traces of Araucarian trees, it is important to direct attention to the wide distribution of the genus in Tertiary Europe. The Oligocene flora of Bembridge in the Isle of Wight and the Eocene flora of the island of Mull included representatives of this ancient type: a Tertiary species was widespread over a large part of the European continent, in the Tyrol, France, Switzerland, Italy, Dalmatia and elsewhere.

More than ninety years ago blocks of fossil wood having the anatomical characters of an Araucarian Conifer were discovered in water-borne sediment associated with basaltic lava-flows of Tertiary age in the Kerguelen Archipelago. The occurrence of fossil wood is, however, not always proof of the former existence of living trees at the place where the wood occurs. We know that tree stems and

branches may be carried by currents over several hundred miles of ocean. Some of the drift-wood stranded on the west coast of Greenland has its source in the forests of Northern Siberia: a similar derivation from a distant starting-point may be true of fossil wood. In 1931 Dr. de la Rüe, a French geologist, discovered several fossil remains in another part of Kerguelen Land and among them were well preserved foliage-shoots, which it was possible to examine microscopically, also single-seeded pieces of cones practically identical with the corresponding members of a living *Araucaria*. It is therefore clear that trees grew on this remote island group, now desolate and wind-swept, one-sixth of its surface hidden by ice-fields and glaciers, the home of three or four cosmopolitan ferns, including our native Polypody (*Polypodium vulgare*), a lycopod (also a British species) and barely thirty species of flowering plants. Kerguelen is now a treeless land. We are at last in possession of definite evidence of the former existence of an Araucarian forest—how long ago it is impossible to say, but probably before the close of the Tertiary period. The fossil foliage-shoots and seed-scales of cones appear to agree most closely with those of the living species, *Araucaria excelsa*, which is now confined to Norfolk Island more than 3,000 miles from the Kerguelen Archipelago. The resemblance to the existing South American Araucarias is much less close. The discovery made by Dr. de la Rüe has exceptional importance and interest in relation to geographical distribution: how came *Araucaria* to this sub-Antarctic land? Transport by ocean-currents as a means of introduction cannot be seriously entertained; wind and birds cannot perform miracles. In an account of the past and present Kerguelen flora by Miss Conway and myself, published last year (1934) in the *Annals of Botany*, we wrote: "The Kerguelen Archipelago must have been part of a continent; whether the parent mass lies below the southern ocean, or whether the submarine platform with its covering of basalt-sheets is a detached portion which floated away from a continental block is a question to which no definite answer can be given."

As already stated, the remains of foliage-shoots and detached cone-scales, bearing a very close resemblance to those of existing species of *Araucaria*, are widely distributed over the northern hemisphere and in several parts of the southern hemisphere far beyond the present discontinuous range of the genus and family. Araucarian remains have also a wide distribution in time, from the Triassic period upwards. There are some indications of the existence of the Araucarian type of Conifer as far back as the Permian period, but the evidence is unconvincing. Palæobotanical data do not

provide an answer to the question: in what part of the world did this link with an age separated from the present by at least a hundred million years begin its world-wide and protracted journeys? The geological history of *Araucaria*, or such pages of it as it has been possible to decipher, is typical of the history of many other old Gymnosperms; at some stage in the course of evolution we find that a genus, using the term in a wide sense, has occupied a very large proportion of the land-surface of the earth whereas now it precariously lives in refuges that are relatively restricted in area and not infrequently separated one from another by wide stretches of ocean. The past is the key to the present: in the past wide-ranging, cosmopolitan genera; at the present day the same genera are represented by a few species, or even by a single species, which remain as the last links of a chain that once almost encircled the world.

Cycads

Having regard to the fact that references are often made in botanical and geological publications to the former wide distribution of Cycads, it is essential to remember that the Cycads of the present age are very different in the structure of their reproductive apparatus from the extinct plants to which the name Cycads is frequently and inappropriately applied. We know very little of the direct ancestors of the nine genera of Cycads (Sago "palms," etc.) which are for the most part, though by no means exclusively, tropical in their widely scattered habitats. It is no wonder that the innumerable fossil fronds discovered in later Triassic and Jurassic rocks the world over, and at many Cretaceous localities, have been interpreted as evidence of the former occurrence of Cycads that were once cosmopolitan. The resemblance of the fossil fronds and of a large number of beautifully preserved stems to those of the true Cycads is amazingly close, and yet the structure of the flowers, which is known with surprising completeness, indicates a wide gulf in certain features which have a high value as evidence bearing upon the question of relationship of the Triassic-Cretaceous fossils and the living Cycads. During a considerable part of the Mesozoic era it is true to say there existed a group of plants, which played a leading rôle in the world's vegetation, ranging from Arctic regions to the borders of Antarctica and having palm-like fronds and thick stems covered with the stumps of old leaves, constructed on a plan almost precisely the same as that followed in modern Cycads. And yet despite these resemblances the inclusion of the dead and living in the same group is misleading and contrary to the canons of science.

The history of the true Cycads is very imperfectly known; it is indeed hardly known at all. We cannot help thinking that the hosts of extinct plants, which in life must have closely simulated the living genera, while differing widely in their fertile shoots, may have come from Palæozoic ancestors whence perhaps were also derived the apparently more primitive and surviving genera which represent a separate line of development. This is a digression though not entirely irrelevant because of the frequent references in literature that have led to misconception of the actual position.

The Maidenhair Tree

The most impressive example in the plant-kingdom of a survival from bygone ages is one which is too familiar to be described at length; a brief reference to it will suffice. *Ginkgo biloba*, the Maidenhair tree, has in recent years become increasingly familiar as a cultivated introduction; it is often said to be a native of China or Japan, or both, though there is no well-authenticated record of its occurrence in a wild state. It is, as Darwin called it, "a living fossil"; and, as befits its history and its fascination for all who derive pleasure from revivifying the life of other ages, it is tended as an object of veneration in the temple precincts of China and Japan. *Ginkgo*, more than any other tree, links the present world with a succession of geological periods. It is the only representative now living of a group of naked-seeded plants (Ginkgoales, a group of Gymnosperms). Formerly regarded as a member of the Yew family, in 1896 it was made the type of a separate section of Gymnosperms because of the discovery by a Japanese botanist of a primitive attribute, unknown in the Yews or any other Conifers, namely the possession of large and motile sperms. It is the sole survivor of a group which in earlier stages of the world's history included several genera and had a world-wide distribution. There is no proof of the occurrence of Ginkgoalean trees in Palæozoic forests and yet there is some evidence, and by no means negligible evidence, of the existence in early Permian floras of a plant with foliage strikingly similar to that of the Maidenhair tree. More than fifty years ago leaves were discovered in Virginia in beds slightly younger than the Coal Measures which were named *Saportæa* after the famous French palæobotanist, the Marquis of Saporta: about ten years ago leaves of the same form and venation were described by Professor Halle of Stockholm from rocks of approximately the same geological age in Central China. This markedly discontinuous distribution—Virginia and Central China—may be taken as evidence of the antiquity of *Saportæa* even in the Permian period: if, as seems

probable, the fossil genus is allied to *Ginkgo*, it implies the existence of an ancestral stock at some still more distant period of the Palæozoic era. There is ample evidence of the occurrence of near relatives of *Ginkgo* in late Triassic, in Jurassic, and Tertiary floras. The genus *Ginkgo*, or *Ginkgoites* as the fossil leaves may be called, lived in Greenland from the early Cretaceous to well into the Tertiary period: it was a member of a Tertiary flora in Spitsbergen, and a Jurassic flora in Oregon; and in Cretaceous Alaska it was represented by more than one form, and it has been found in Tertiary rocks in northern Canada. The *Ginkgo* family was well represented in the Jurassic vegetation of Franz Josef Land: it had, in fact, a circumpolar distribution. In England members of the group were characteristic trees in Jurassic forests and *Ginkgo* itself survived up to the early part of the Tertiary period, growing on the edge of a lake on the basaltic plateau of the Inner Hebrides. In France it lingered on to the latter part of the Tertiary period. *Ginkgo* and other members of the Ginkgoales were among the commoner plants in Europe throughout the Jurassic and Cretaceous periods: and many records might also be cited from the southern hemisphere, from Afghanistan, Turkestan, Siberia and farther east, and from Australia. This group of naked-seeded plants was almost cosmopolitan. One would like to know the causes of its decline and the gradual dwindling of the territory which it had successfully colonised through many stages of geological history. It is a pleasing fancy to think of the venerable and sacred trees in the temple gardens of China and Japan "dreaming out their old stories to the wind," stories that we can unravel only in part.

Flowering Plants

The past history of flowering plants, though very imperfectly known, furnishes many instances of migration from one part of the world to another, and often gives support to the speculations of some of the older botanists that the Arctic regions were a centre from which new products of evolution spread along divergent routes to more southern homes on both sides of the Atlantic. A few examples will suffice to direct attention to some of the many lines of thought along which the student of ancient floras is impelled by the evidence of fossils.

We will first briefly consider two genera of the *Magnolia* family (Magnoliaceæ), *Magnolia* and the Tulip tree (*Liriodendron*). *Magnolia* now includes about sixty-two species, eighteen American and the rest Asiatic. Some American species live in the south-eastern part of the United States and extend as far north as Southern

Ontario; others are Central American, from Costa Rica to Mexico, Cuba and Haiti. In Asia the genus has a wider distribution, westwards along the Himalayas, South-East Tibet, Upper Burma, through parts of China to Korea, Manchuria, Japan, Assam, Siam, Indo-China and the Malayan region. The genus *Liriodendron* includes two very closely related species; one is confined to a small area in China (*L. chinense*) and the other (*L. tulipifera*) has a much wider range from the South-Eastern United States to Southern Ontario. A great number of fossil leaves from Cretaceous and Tertiary rocks have been described as species of *Magnolia*, but by no means all of them can be accepted as true records. There is, however, no doubt that the genus was represented over a vast territory in the northern hemisphere including the Arctic regions. Evidence is not confined to leaves though many of them are undoubtedly correctly determined. In 1869 Oswald Heer, the pioneer in Arctic Palæobotany, described a cone bearing crowded oval carpels, from a collection brought to England by Mr. Edward Whymper, as a species of *Magnolia*. This specimen is now in the British Museum and is almost certainly an aggregate of *Magnolia* fruits; it is from the Tertiary plant-bearing beds of Western Greenland (lat. 70° N.) and with it occur leaves that can be closely matched with the foliage of recent species. In another collection of Greenland plants Miss Conway and myself recently found a cone-like fossil which we are describing as the fruiting branch of a *Magnolia*; it is practically identical in form and in the structure of the carpels with old flowering branches of some existing species. Mr. Good, in a paper published in the *Annals of Botany* (1925), "On the Past and Present Distribution of the Magnolieæ," gives a brief summary of the fossil records. It is, as he says, fairly certain that *Magnolia* was widely spread in Cretaceous Europe, and on the American continent extended far to the north of its present area: it continued to occupy a large area in both the Old and the New World during part of the Tertiary period and there are traces of its persistence in Europe up to the time of the Ice Age. Mr. Good believes that the present distribution of the Magnolieæ is "the direct result of the enforced migration due to the great climatic changes in the Pleistocene." In a former article in *Science Progress* (July, 1934) reference was made to the occurrence of more than one species of *Magnolia* in the flora of the London Clay.

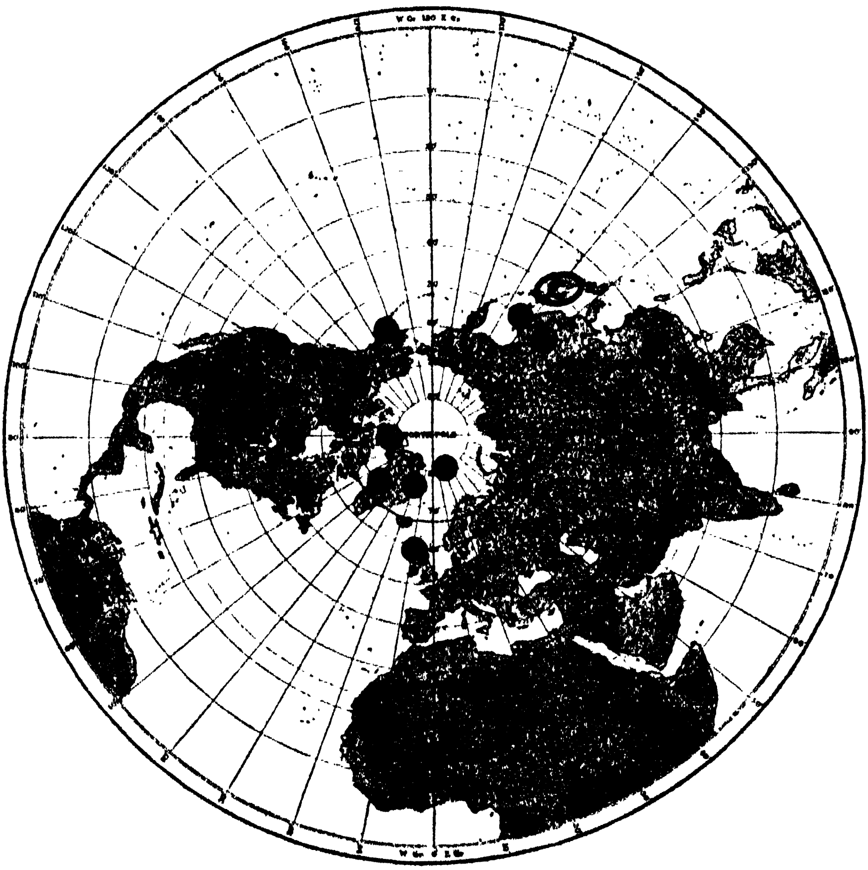
The fossil records of *Liriodendron* tell a similar story. Many of the leaves from Cretaceous and Tertiary rocks referred to this genus are in all probability leaflets of the tropical Leguminous genus *Dalbergia*. Ten years ago I gave reasons for discarding Heer's

supposed *Liriodendron* species from the Cretaceous plant-beds of Greenland. Among the fossils described by Heer from Tertiary beds in North-Western Iceland are a few leaves and fruits which seem to be almost identical with those of the Tulip tree. An examination of the actual specimens in the Copenhagen Museum leads me to regard Heer's identification as probably correct. Imperfect leaves which may belong to a *Liriodendron* are recorded from late Tertiary beds in the Altai mountains: good specimens have been described from Tertiary floras in France and Switzerland, and there are records from several North American localities, both Cretaceous and Tertiary.

Recently a Japanese Palæobotanist, Dr. Endô, described fossil leaves of *Liriodendron* from two districts in Japan, probably Miocene in age. It is noteworthy that seeds of the Tulip tree are stated by Mr. and Mrs. Clement Reid to occur in profusion in the Pliocene delta-deposits on the Dutch-Prussian border, and the same authors describe a head of small carpels which they refer to an extinct species of the same genus. In 1928 Dr. H. von Ihering stated that leaves of *Liriodendron* had been discovered in Upper Cretaceous rocks in Patagonia, but, so far as I know, illustrations of the specimens have not been published. It is worth while to compare with fossil specimens the unusual forms of leaves often seen on shoots springing from the lower part of an old stem, and other variants which can often be found in searching among the foliage. Possibly, as some botanists believe, these abnormalities on a living tree are throw-backs to ancestral forms. A selection of drawings of leaves picked from a Tulip tree is given by Professor E. W. Berry in his book on *Tree Ancestors*. The volume includes a very instructive set of maps illustrating contrasts between past and present areas of distribution, though no doubt if account were taken of more recent discoveries certain modifications would have to be made.

The largest broad-leafed tree in Japan is said to be *Cercidiphyllum japonicum*; it is occasionally cultivated in English gardens. There is a second form in China, *Cercidiphyllum japonicum* var. *sinense*, which lives on the mountains of Szechuan (see Map). The genus has been relegated to a family of its own, the Cercidiphyllaceæ, which is related to the Hammelidaceæ of which the *Liquidambar* and the Witch Hazel are two familiar representatives in cultivation. The leaves of *Cercidiphyllum* are almost circular or broadly ovate; the base is more or less cordate; and the margin may be almost entire or definitely crenulate: the venation is a fairly distinctive feature. Unfortunately no fossil fruits have been discovered, though leaves practically identical with the foliage of the recent

species are abundant in the rocks of several countries. It is one of the great difficulties in palæobotanical work that leaves are usually not preserved in close association with fruits and seeds. The history of our knowledge of the fossil records of *Cercidiphyllum* is briefly related as an example of palæobotanical methods. Nearly seventy years ago Heer read a paper to the Royal Society of Dublin



Map showing the present distribution (oval areas with C) of *Cercidiphyllum* and some of the localities (black patches) in Arctic and Temperate regions where fossils have been found which are believed to be very closely related to the recent species. (Stanford's Stereographical Projection on the plane of the Equator.)

in which he gave a preliminary account of the results of his examination of a collection of Greenland fossil plants: an illustrated and fuller account of this and other collections from Arctic regions was subsequently published in volumes of the *Flora Fossilis Arctica*. He described and illustrated several leaves, differing one from the other only in minor features, as species of *Populus* and compared

them with certain living poplars, *e.g.* a species from the Euphrates. In a later volume he referred some Tertiary leaves from Spitsbergen to the genus *Grewia*, which is mainly tropical and is a member of the family (Tiliaceæ) to which the Lime (*Tilia*) belongs; they are certainly not generically distinct from the supposed poplars. It is interesting to find that in 1877 Heer wrote: "In form, venation, and in the marginal teeth *Grewia crenata* the Arctic species bears a strong resemblance to *Cercidiphyllum japonicum*." Comparison with *Cercidiphyllum* of Heer's *Populus arctica* and similar forms, including the leaves called by him *Grewia crenata*, convinced me that the marked resemblance to the Japanese tree which he noted, but did not implement by adopting an appropriate generic name indicating the striking similarity, is much more than superficial and is evidence of close relationship. At a later date Starkie Gardner expressed the opinion that leaves closely agreeing with Heer's *Populus arctica* should be included in the genus *Bœhmeria*, a member of the nettle family (Urticaceæ). A comparison of the fossil and recent leaves does not lend support to this view. In 1922 Professor E. W. Berry of Johns Hopkins University proposed the generic name *Trochodendroides* for Heer's *Populus arctica* and for similar leaves from a Tertiary flora in Texas. Four years later Berry described other Tertiary leaves from localities near Vancouver in Western Canada as *Trochodendroides arctica* (= Heer's *Populus arctica*). He wrote: "if the reader will compare the accompanying figures (*i.e.* the drawings published in his paper) with the leaves of the existing genera *Tetracentron* and *Cercidiphyllum* he will find that they can be exactly matched." The name *Trochodendroides* was chosen by Berry for fossil leaves which were probably borne by trees of the family Trochodendraceæ. It is a little unfortunate that this name was adopted because it suggests affinity to the Chinese *Trochodendron*, a genus included by some botanists with *Cercidiphyllum* in one family, the Trochodendraceæ. The genus *Tetracentron* is no doubt closely related to *Cercidiphyllum*, its leaves, though rather larger and relatively longer, are very similar: the family Trochodendraceæ is recognised as a family distinct from the Cercidiphyllaceæ. It may be pointed out that leaves of a living species of Pepper, *Piper excelsum* of New Zealand, while they agree closely with the Arctic leaves in size and venation, differ from them in certain details. Though absolute proof is lacking, my belief is that Professor Berry is correct in regarding Heer's *Populus arctica* and *Grewia crenata* as generically the same as *Cercidiphyllum* and indeed very nearly related to the existing species. Some of the Arctic fossils are indistinguishable from *Tetracentron* leaves and it

is by no means always possible to decide whether their affinity is nearer to that genus or to *Cercidiphyllum*. It is perhaps reasonable to offer the opinion that these allied Far Eastern genera once existed side by side in Arctic floras, and that in the Tertiary period the present comparatively slight differences between them had not become definitely established. Valuable confirmatory evidence of the relationship of the Arctic trees to *Cercidiphyllum* and *Tetracentron* has recently been furnished by Dr. Mathiesen of Copenhagen, who discovered that some petrified wood from Tertiary localities in East Greenland, Sabine Island and Cape Dalton (lat. 74° N.), which was found in association with leaves like *Cercidiphyllum*, bears an anatomical resemblance in certain features to the wood of *Cercidiphyllum* and *Tetracentron*.

Trochodendroides arctica, adopting for the present Berry's rather misleading name, had a very wide distribution on the Arctic Continent in the early days of the Tertiary period; it is recorded from Ellesmere Land, Grinnell Land, Greenland, both from the western and the eastern border, Spitsbergen, Sakhalin Island, the Mackenzie River and other localities in Northern and Western Canada (see Map). The same type of leaf has been found in Alaska, Oregon, Nebraska, California and Texas. A short time ago Professor Chaney of the University of California wrote to me as follows: "Your inquiry regarding the occurrence of leaves of *Cercidiphyllum* is of exceedingly great interest. Within the past year Dr. R. W. Brown and I have definitely determined that leaves previously referred to *Grewia crenata* in Miocene floras of Western America are referable to this Asiatic genus."

An examination of exceptionally well-preserved leaves in the large collection of Tertiary plants brought from the Island of Mull by Mr. Starkie Gardner and now in the British Museum convinced me that some of the specimens are undoubtedly generically identical with *Cercidiphyllum* and indistinguishable by any features of taxonomic importance from the Arctic species, which Heer called *Populus arctica*. Records of the same type of leaf might be quoted from more southern localities in Europe. Enough has been said to show that in the earlier part of the Tertiary period forest trees with foliage almost identical with that of the two genera *Cercidiphyllum* and *Tetracentron*, both confined within comparatively narrow boundaries on the mountains of China and Japan, encircled the north polar region and extended well into temperate latitudes in America and Europe. Assuming, as I believe we may, a close relationship of the fossil to the living species, the facts that have been briefly and incompletely stated furnish an exceptionally

remarkable and, to my mind, cogent argument in support of an Arctic origin of genera that now survive in a Far Eastern refuge.

The two kinds of Plane, the Oriental Plane, *Platanus orientalis*, and the so-called London Plane, *P. acerifolia*, are commonly cultivated in England. It should be noted that the name Sycamore, which in England connotes a species of *Acer*, is used in America for the Plane tree. *Platanus acerifolia*, believed to be the result of a cross between the Oriental and the Occidental Plane, is often planted on roads near towns. It is difficult accurately to determine the number of recent species, but there are about half a dozen, all of which, with the exception of the Oriental Plane and the London Plane, are native in eastern and western North America, Mexico and Central America. *Platanus orientalis* is cultivated in Northern India, but its home is in the Near East, in Greece, the Greek islands, the shore of the Adriatic and in Asia Minor (for a map showing the present distribution, see Seward, 1925, in the list of References, p. 217). The most impressive example of the Oriental Plane known to me is in the island of Cos in the Ægean Sea: its gigantic twisted branches spreading from the ruins of a noble trunk produce an impression of great age and almost persuade one to accept the local legend that it was under the shade of this tree that Hippocrates (born in Cos about 460 B.C.) prescribed for his patients. It is impossible to say what its age may be; it carries one's thoughts back to a long line of ancestors reaching far beyond the limits of human history. *Platanus* must have been one of the most widespread genera in Cretaceous forests in Arctic regions, in North America and in Europe. In some of the sedimentary rocks upraised from a Cretaceous and Tertiary estuary in Western Greenland beautifully preserved leaves are abundant and in some places they are associated with the characteristic ball-like clusters of fruits. Similarly in Cretaceous rocks of Bohemia, Germany and other European countries considerably north of the present area of distribution in the Old World the genus is represented by many readily recognisable specimens. Fruits and leaves are recorded also from a Tertiary locality in northern Siberia. Specimens have been obtained from the Tertiary coal-bearing beds of Spitsbergen rivalling in breadth the largest leaves of the Oriental Plane. There are also splendid examples in the British Museum collection from the island of Mull. At a still later stage, in the Pliocene flora of North America and France, the genus was still flourishing several hundred miles west of its present area. Many fossil species have been described and there can be little doubt that in the Cretaceous and Tertiary periods *Platanus* was not only more abundant than it is now, but

was also represented by species exhibiting a greater range in leaf-form than we find in the living descendants. Examination of the leaves on a living tree, including those borne at the base of an old stem, and a comparative study of herbarium specimens makes one despair of any satisfactory delimitation and accurate definition of extinct species. The significant fact is that the present geographical distribution of the genus is far more restricted than the area over which it formerly spread. Another important point is that *Platanus*, like many other Dicotyledons, may very likely have had its original home in polar lands.

One of the most astonishing instances of long-distance travel from polar regions to the tropics is furnished by the fossil records of the Breadfruit tree (*Artocarpus*). The genus, though widely cultivated in the warmer parts of the world, is native in the Indo-Malayan region and in China and does not extend beyond a line drawn through the north-eastern corner of the Peninsula of India to the eastern sea-board. Well preserved leaves almost identical in size and shape with those of the recent species *Artocarpus incisa* and pieces of both male and female inflorescences have been described from Cretaceous rocks in Greenland 300 miles north of the Arctic Circle. There is also satisfactory evidence of the occurrence of the genus in the Miocene forests of Switzerland and in the older Tertiary floras of several parts of North America.

It is contrasts such as this which raise in an acute form the problem of climatic change in the course of geological history. My immediate purpose is not to discuss possible explanations of the disconcerting results of palæobotanical enquiry, but rather to emphasise the desirability of collecting evidence from the rocks in order that we may be in a better position to connect the present with the past. The value of fossil plants as criteria of climatic conditions was briefly considered in my article on the Flora of the London Clay (SCIENCE PROGRESS, July 1934).

Many years ago descriptions were published of supposed fossil representatives in the northern hemisphere of flowering plants belonging to the family Proteaceæ and, as a natural consequence, much discussion followed this provocative pronouncement. It was strenuously denied that any trustworthy evidence had been presented, and there is no doubt that many of the leaves regarded as Proteaceous are incorrectly named and valueless as records: some of them could be referred with greater confidence to genera of other families. The Proteaceæ are now confined to a broad belt in the southern hemisphere stretching from Chili and tropical South America through South Africa and the mountains of tropical Africa

to Australia, New Zealand and New Caledonia. The question is whether this family was ever represented in the Temperate and Arctic floras of the northern hemisphere. A critical examination of the material which has been accumulated since the first announcement of the discovery of northern fossil species is greatly to be desired; this it is hoped to undertake in the future. My belief is that not a few of the specimens from Cretaceous and Tertiary rocks in North America and Europe have been correctly assigned to the Proteaceæ; but this is merely a tentative opinion.

An analogous case for further enquiry is presented by the genus *Eucalyptus*, which includes well over 300 species of Australian trees, some of which can be grown in the warmer districts of this country. There are a very few species in the Malayan region and New Guinea: it is *par excellence* the characteristic tree of the Australian flora and by far the tallest flowering plant in the world, probably in some instances exceeding in height the giant Redwoods of California. Records of *Eucalyptus* leaves and fertile branches from northern hemisphere rocks are numerous and some of them furnish clear evidence of the occurrence of this Australian tree in both Cretaceous and Tertiary floras. But a critical review of the palæobotanical data must be postponed to a later date.

In this connection another member of the *Eucalyptus* family (Myrtaceæ), the genus *Leptospermum*, merits a brief reference. *Leptospermum* is now Malayan and Australian and is a characteristic New Zealand genus. Several years ago specimens of fruits hardly distinguishable from those of recent species were described from a Tertiary locality in south-western Russia and much more recently both fruits and leaves were discovered in northern France. In an unpublished paper Miss Conway and myself have adopted the generic name *Leptospermites* for clusters of fruits from Greenland that are either Cretaceous or Tertiary. In all probability this Myrtaceous genus, now confined to a remote region in the southern hemisphere, had its birthplace in the far north.

Conclusion

In the vegetation of the world there are not a few plants entitled to be called cosmopolitan; some of them exhibit a remarkable range of tolerance and appear to be able to adjust themselves to very different climates and situations. There are other plants living in isolation, isolated because of the narrowness of their spatial boundaries and as members of the plant-kingdom characterised by unusual and distinctive features. One conclusion that emerges from a survey of the world's vegetation at different stages of geological

history, so far as such survey is possible, is that the present areas of distribution often bear very little relation to those revealed by the scanty records of the rocks. Plants which still occupy broad domains may be descended from an ancient stock, but on the other hand they are in many instances comparatively recent entrants on to the world's stage, which are still in the heyday of youth. Plants living within a restricted territory are very often a depauperated rearguard of a much larger company that in earlier days had successfully colonised a more extended region in parts of the world far distant from their present homes.

It would seem that there has been a tendency throughout successive periods of geological history, almost indeed since the earlier ages of the Palæozoic era, for plants to wander over continents through many degrees of latitude, from lands that are now glacial or destitute of trees to the heart of the tropics. The present occurrence of a family, a genus, or a species in one relatively small area, or in two or three discontinuous areas, is the result of partial extinction in a formerly continuous and wide area of occupation. These and many other aspects of plant-distribution can only be appreciated in their full value by supplementing an examination of existing floras by the investigation of the random samples saved by Nature from the forests of other days.

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THE OPTICAL ANISOTROPY OF COLLOIDAL SOLUTIONS

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Optical anisotropy, so characteristic in many colloidal solutions, manifests itself generally in the fact that external forces—mechanical or magnetic forces, etc.—may cause a colloidal solution which is naturally isotropic to become anisotropic towards polarised light; it may become, for instance, double refracting and dichroic [1].

In plane polarised light the vibrations are lying in one plane, and it is mostly obtained by letting normal light pass through a Nicol prism. If two Nicols are put up so that their planes of polarisation are *perpendicular*, no light is able to pass: the visual field is dark and it remains dark if an isotropic substance, water or an aqueous solution, for instance, or a plate of glass, is brought between the nicols. But an anisotropic substance, a plate of a crystal belonging, for instance, to a uniaxial system and cut parallel to the optical axis, lets the visual field brighten up, provided that the optical axis does not lie in one of the planes of polarisation: the plate is double refracting. The brightening up of the visual field between crossed nicols is always distinctive of double refraction.

Quite a number of colloidal solutions show the following behaviour: if one of these sols, for instance an old sol of vanadium pentoxide, V_2O_5, contained in a cell, is brought between crossed nicols, the visual field remains dark; but it brightens up as soon as the fluid is stirred. This is the phenomenon of stream double refraction [2]. Double refraction may also be produced in sols of this kind by bringing the cell with the fluid between the poles of a magnet, the beam of light passing between the poles. This is magnetic double refraction, the first conspicuous phenomenon of this nature discovered by Majorana [3] in 1902 in colloidal solutions of iron-oxide. The electric current also causes double refraction

in sols belonging to this group, but this phenomenon is more complicated than the two mentioned and has not been investigated so thoroughly [4].

One point which is going to be discussed more fully later on may be mentioned now, namely that in all these cases the main effect of the external force consists in arranging the anisometric, *i.e.* rod or plate-like, particles of the sols in a regular way, for instance, parallel to each other.

The quantitative side of this kind of anisotropy may be made clear by the following experiments. If a beam of mono-chromatic light passes through a glass prism, it shows the well-known path first investigated by Newton. If a prism is cut from a double refracting, uniaxial crystal, and if it is cut so that the optical axis is normal to the triangular cross section, two waves of light pass through the crystal having different indexes of refraction; the two beams are polarised normally to each other. The double refraction δ is simply the difference of the two indexes of refraction n_e and n_o:

$$\delta = n_e - n_o; \quad \ldots \quad 1$$

n_e belongs to the so-called extraordinary ray whose vibrations are parallel to the optical axis, n_o to the ordinary ray whose vibrations are normal to the optical axis. Double refraction is called positive, if $n_e > n_o$; negative, if $n_e < n_o$. This experiment is not easily performed: crystals large enough to be cut as a prism are rarely found. But the experiment is not nearly as difficult in the case of stream double refraction. One only needs a tube with a triangular cross section, filled, for instance, with an old sol of V_2O_5. As long as the fluid is at rest, a beam of light passing through the tube behaves as if the tube was filled with water or as if it were a glass prism. But as soon as the liquid flows, the beam is split into two beams which are normally polarised to each other. The indexes of refraction of these two beams may be measured; their difference is the stream double refraction. The optical axis of the flowing liquid is practically parallel to the direction of flow. One is able to decide whether the double refraction is positive or negative, by determining the beam whose vibrations are parallel to this direction. V_2O_5–sol turns out to be positively double refracting.

In order to make similar experiments with magnetic double refraction, the poles of the magnet would have to be above and below the triangular tube.

The methods actually used for measuring the double refraction of colloidal solutions caused by streaming or by a magnetic field

are indirect and more complicated, but also more exact [5]; they need not be discussed here.

The two beams of light passing through the flowing, double refracting liquid may also be absorbed to a different degree, or more correctly, they may be extinguished to a different degree. For extinction may have two causes [6]: it may be due to the true absorption of the particles contained in the sol. This phenomenon is called *dichroism*; it is measured by the difference of the absorption coefficients of the two rays. But extinction may also be in part due to the fact that the so-called Tyndall-light, *i.e.* the percentage of light diffracted and scattered sideways, differs regarding the two beams. This phenomenon is called *dityndallism*. It is not easy to distinguish between dichroism and dityndallism.

The optical anisotropy of colloidal systems comprises a great many possibilities. The external forces may vary: mechanical, magnetic and electric forces have been mentioned already. But that is not all. In concentrated sols there may be a tendency of the particles to become orientated spontaneously, rod-shaped particles forming flame-shaped, double refracting spots, in which they lie parallel to each other; such orientated groups are called *tactoids*, the remainder of the sol, only containing non-orientated, but also rod-shaped particles, is the *atactosol* [7].

Light may also act as an orientating force.—The colloidal systems influenced by light in this way are not sols, it is true, but gels; yet these two kinds of colloidal systems are so closely related that this orientating action of light deserves to be mentioned here. Weigert [8] was the first to discover that, if films of gelatine containing particles of photochloride or of a suitable dyestuff are illuminated by planely polarised light, a dichroic and double refracting spot is produced. The "optical axis" of the dichroism of this spot depends upon the plane of vibration of the polarised light.

In a similar way circularly polarised light, illuminating a layer of photochloride, produces a spot which shows circular dichroism and optical activity, *i.e.* circular double refraction. This was the first case, found by Zocher and Coper [9], where optical activity was called forth by purely physical means.

The anisotropic properties produced in colloidal systems by these external forces have been mentioned already, all three: double refraction, dichroism and dityndallism. They are not independent of each other: dichroism and double refraction are correlated by a rule stated by Zocher [10] and found to hold very generally. The same rule correlates circular dichroism and circular double refraction, *i.e.* optical activity.

Finally, characteristic differences may arise, if cases of optical anisotropy are investigated macroscopically or with a microscope. Macroscopic dityndallism is due to the amount of light diffracted by the colloidal particles and depends upon the direction in which the particles are illuminated. In sols with rod- or plate-like particles dityndallism may show itself in beautiful streaks [11] which appear when the liquid is stirred; these will be discussed more fully later on. The amount of diffracted light is, on the other hand, instrumental in causing the visibility of these particles under the ultramicroscope. Rod-shaped particles, for instance, are only strongly visible, when the illuminating light strikes them normally to their long axis. Using an ultramicroscope, having a favoured direction of the illuminating light, as is the case with the slit ultramicroscope, only those rod-shaped particles are visible which have a suitable position. In the cardioid ultramicroscope, on the other hand, the illuminating light is naturally all-sided; here an instrument, like the azimuth diaphragm [12], becomes valuable, which makes it possible to cut out any direction of the illuminating light.

The recent development of this line of research has been specially successful in two directions. The investigations, mentioned so far, have perhaps not always distinguished strongly enough between the different kinds of forces causing optical anisotropy. A closer investigation has shown that, for instance, magnetic double refraction behaves very differently from stream double refraction in some important points; magnetic double refraction not only depending upon the shape of the particles, but also upon their chemical nature.

Secondly, an old hope was fulfilled which was raised when stream double refraction was first discussed: the double refraction of muscles could be explained to a marked extent on the ground of what was known about the optical anisotropy of other colloidal systems.

II

1. As mentioned above, stream double refraction is due to the fact that the particles, originally being distributed at random in the liquid, when at rest, are orientated, as soon as the liquid flows [13]. This orientation is caused chiefly by the friction between the particles and the surrounding liquid. It is essential that the particles are not spherical; obviously spherical particles cannot be orientated by the movement of a liquid. These forces of friction favour an orientation of rod-shaped particles, for instance, with their long axes parallel to the lines of flow, on the whole. In concentrated

sols forces of attraction between the particles influence orientation too. That the particles are actually orientated by the flow of a liquid, could be seen directly under the ultramicroscope using a V_2O_5–sol whose particles are pronouncedly rod-like [14].

It is remarkable that this orientation of non-spherical particles alone is sufficient to produce a system which is double refracting. The particles may consist of an amorphous substance or be crystals belonging to the regular, not double refracting system of crystals. If they have the shape of rods or lamellæ and are orientated regularly, they produce a system which is double refracting, as was shown by O. Wiener [15]. This is the co-called rod or lamellar double refraction.

Better known and more easily understood is the actually less simple case that the particles are small crystals having an intrinsic double refraction, because they belong to one of the crystal systems naturally showing double refraction. As long as the liquid is at rest, the particles are distributed so irregularly that the double refraction due to each single particle is extinguished on the whole; the colloidal solution, as it is, is not double refracting. But when it flows, layers of orientated particles, having an intrinsic double refraction, are formed, and such a layer would behave like a plate of a crystal. If we are dealing, for instance, with rod-shaped crystals of a uniaxial system lying parallel to the lines of flow, the layer would be like a plate of the crystal, cut parallel to its optical axis.

Pure cases of rod or lamellar double refraction are not found frequently. Lamellar double refraction is shown by aqueous sols of gold and silver [16]. These generally have a weak stream double refraction, although their particles have the normal X-ray spectrum of gold or silver; that is to say, they are crystals belonging to the regular system and they therefore ought not to be double refracting at all. But the crystals in these sols are formed very quickly and not very regularly. Owing to this they are not well-defined cubes or octahedra, as they might be, but lamellæ which cause a lamellar double refraction, when orientated.

Wiegel [17] was able to prove the lamellar shape of the particles of these Ag-sols in a fairly direct way. He worked out a nuclear method of preparing evenly grained Ag-sols with particles of different size. By a quite gradual change of the experimental conditions much larger Ag-particles could be produced which formed a rough suspension; they were visible under the microscope as distinctly plates. Ag-sols prepared in practically the same way, only using somewhat different concentrations of the reacting substances, had particles of colloidal size and were stream double refracting.

X-ray spectroscopy shows that the particles in stream double refracting V_2O_5– and iron oxide-sols are small crystals belonging to crystal systems which have an intrinsic double refraction. This is the main cause of the double refraction of these sols. Yet, rod or lamellar double refraction has to be taken into account to a certain extent too, since the particles have a rod- or plate-like shape.

Hence the total double refraction is the sum of intrinsic and rod double refraction. In the case of stream double refracting sols it is not easy to decide to what extent the one or the other contributes to the total birefringence. Yet, this problem may be solved in the case of double refracting gels, and a case of this kind will be discussed in section III.

This discussion leads to the following results: if a sol shows stream double refraction, its particles are undoubtedly anisometric; they are not spheres or cubes, etc., but rods or plates. One is therefore able to judge of the shape of particles in colloidal solutions by examining them as to their stream double refraction. An apparatus introduced by Zocher [18] is generally used for this purpose: the light is polarised by reflexion on two sets of glass-plates, and analysed by a nicol. The colloidal solution, contained in a cylindrical tube, is brought into the beam of light and rotated there; looking through the nicol, a black cross is seen in a bright field, provided that the sol is stream double refracting. This phenomenon is easily understood. Assuming the particles to be rod-shaped, they are orientated practically in concentric circles. The visual field is brightened up, where the long axis of a particle forms an angle with the directions of polarisation; it remains dark, where this axis is parallel to one of these directions.

Dityndallism allows one to detect deviations from a spherical shape even in a simpler way, provided that the anisotropy of shape is sufficiently pronounced. In old sols of V_2O_5, for instance, beautiful, glossy streaks appear on stirring, as mentioned above; they disappear when the fluid settles to rest again. The explanation of this behaviour is that on stirring currents of different direction are produced in the liquid, the particles always being orientated practically parallel to the lines of flow. The Tyndall-light scattered sideways is of different intensity and of different colour, according to the angle which the long axes of the particles in a given current forms with the direction of the incident beam of light and with the direction of vision. Thus neighbouring currents contrast with each other.

If this method is developed in a more detailed form, one is able to decide, with a high degree of probability, whether one is dealing

with rod-shaped particles or with lamellæ [19]. The amount of Tyndall-light scattered laterally depends largely on the size of the particles; it increases as the second power of the size, as long as the particles are sufficiently small compared with the wave-length of the incident light. If polarised light hits a rod-shaped particle, the direction of vibration lying parallel to the long axis, the amount of light scattered is large. It is very small if the polarised light hits the particle in such a way that the direction of vibration is parallel to one of the short axes of the rod. In the case of plate-like particles the conditions are different: a large amount of light is scattered in two main positions, the direction of vibration lying in the plane of the lamella; but only a small amount is scattered when the direction of vibration is normal to this plane.

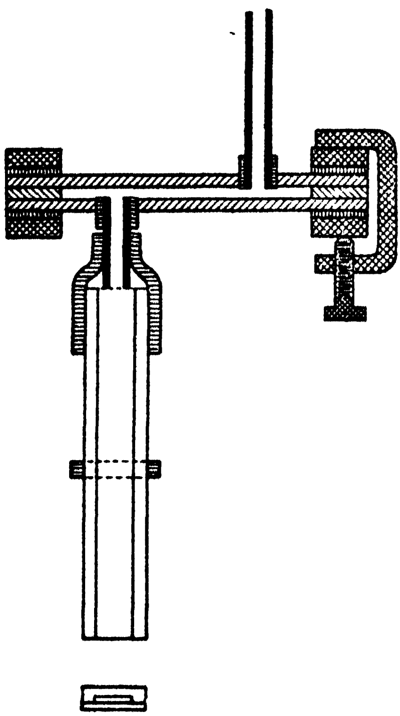

FIG. 1.

It would lead too far to discuss all the subtleties of this phenomenon. Fig. 1 shows an apparatus which may be used for investigating this question and which allows one to change the direction of the incident beam of polarised light and the direction of view at will. A tube with a rectangular cross section may be illuminated from one side or from behind or from above, and the flowing liquid may also be looked at in one of these directions. Finally light of a different plane of polarisation may be used. The intensity of the Tyndall-light and, to a certain extent, its colour too are changed, as soon as the liquid flows; for the particles are orientated by the flowing liquid and a certain percentage of them is brought into a new position toward the beam of incident light. Combining in different ways the characteristic directions mentioned, it is found that the Tyndall-light changes in some cases in a different sense, when the liquid contains rod-shaped particles as it does, when the particles are plate-like. In this way it was proved that particles of old iron oxide sols, prepared by Graham's method, are not rod-

shaped, but lamellæ, whereas those of V_2O_5 sols are most pronouncedly rod-like.

2. In the case of magnetic double refraction [20] the shape of the particles is of great importance too. If the particles are amorphous or are crystals of the regular system, their orientation in a magnetic field mainly depends upon their shape : a cylinder of a homogeneous paramagnetic substance is orientated with its long axis parallel to the lines of force, that of a diamagnetic substance normally to these lines. If the particles are crystals belonging to other crystal systems, however, the susceptibility may be different in one direction from that in another. There is always a tendency of such a crystal to be orientated in a magnetic field with the axis of largest susceptibility parallel to the lines of force. Hence there are two influences competing in regard to orientation of crystals not belonging to the regular system : an external anisotropy owing to the shape of the particle, and an internal anisotropy, caused by the difference in susceptibility, when determined for one or the other axis. It is, for instance, quite possible that in a needle-shaped crystal the susceptibility has its largest value in the direction of a short axis, and the needle might therefore be orientated with its long axis normally to the lines of force, not parallel to them, as would be the case if the external anisotropy prevailed over the internal anisotropy. Obviously a very manifold behaviour of magnetic double refraction may be expected, with a strong influence of the chemical nature and the crystal structure of the particles ; their shape is not the only decisive parameter.

It is easily understood that, whereas a sphere is not orientated in a flowing liquid, it may be orientated in a magnetic field ; it only need be cut from a crystal, whose susceptibility depends upon its crystal structure : the sphere would be orientated in such a way that the axis of maximum susceptibility stands parallel to the lines of force.

Magnetic double refraction actually behaves in a fairly complicated way. There are certainly quite simple cases like that of the V_2O_5–sol. It always shows a positive stream double refraction and a positive magnetic double refraction, independently of the flux density of the magnetic field. Since the small, crystalline particles of this sol are rod-shaped and most likely weakly paramagnetic, this signifies that they are always orientated with their longest axis parallel to the lines of flow and of magnetic force. There also exist sols and suspensions of iron compounds which show a simple behaviour in regard to magnetic double refraction. Hematite very finely powdered may be peptised to a fairly stable

suspension. This has a positive double refraction δ—in an arbitrary unit, except in the case of Fig. 6—which grows steadily with increasing flux density H (in Gauss) and reaches a state of saturation at low flux density values (cf. curve I in Fig. 2). Practically the same curve is found in sols, prepared by hydrolysing iron salts at high temperatures (curve II in Fig. 2). One is justified to assume that they mainly contain iron oxide as so-called α-Fe_2O_3, the substance forming hematite.

Yet, cases of such a simple behaviour were found but a short time ago, when Heller investigated magnetic double refraction of

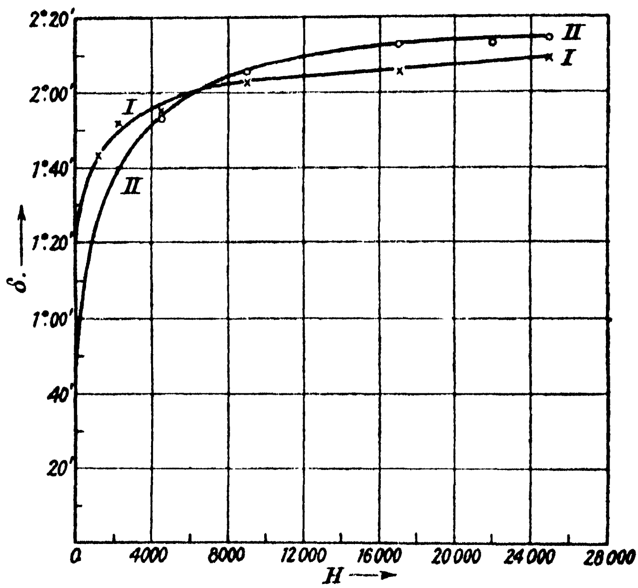

FIG. 2.

sols very thoroughly. All the older experiments by Majorana [21], Schmauss [22], Cotton and Mouton [23] were performed with so-called Graham sols.—They are prepared by dialysing solutions of iron salts such as $FeCl_3$, having a suitable p_H, at room temperature. The sols used in these experiments had mostly been heated mildly for a shorter or longer time.—Their behaviour was discouragingly complicated: the magnetic double refraction was in some cases positive, in others negative, very often it was positive for small values of flux density, inverting to negative values when the flux density grew larger. Cotton and Mouton were at a loss how to explain this extraordinary behaviour; the only possible

explanation seemed to be that these sols might contain particles of different chemical constitution, perhaps both particles of a basic salt and of an oxide or a hydroxide, and their properties as to shape, susceptibility, etc., might be very different.

But Heller showed that this explanation need not be the true one. He found that sols prepared by oxidising $Fe(CO)_5$ with H_2O_2 behaved in the very same way, although they contain undoubtedly exclusively particles of gœthite, a characteristic, crystalline hydroxide, FeO(OH). The X-ray diagram of the particles of these sols shows only lines of this substance. The most plausible explanation is therefore that the complicated behaviour found by Cotton and Mouton is due to an antagonism between internal and external anisotropy as discussed above.

It is not easy to disentangle these rather complicated correlations. They are simplified to a certain extent if homodisperse or evenly grained sols are used instead of the heterodisperse ones containing particles of different size.—Homodisperse sols of gœthite may be prepared by dialysing solutions of suitable iron salts, for instance of iron acetate, at low temperatures. If this hydrolysis proceeds sufficiently slowly, the particles produced are small and fairly equal in size. X-ray analysis proves them to consist of gœthite [24].—These evenly grained sols do not show such a complicated behaviour as do the heterodisperse gœthite sols, just mentioned: the magnetic double refraction is positive at first (curve I in Fig. 3) and increases, as the sol grows older (curve II in Fig. 3); but as soon as the sol has attained a certain age, the double refraction diminishes again: there comes a time when the sol shows no double refraction at all (curve III in Fig. 3), independently of the value of flux density; after that the double refraction turns negative and goes on growing in this inverse sense till a maximum value is reached (curve IV in Fig. 3).

According to Heller the explanation for this behaviour might be as follows: as long as the sol is young, the external anisotropy predominates; the particles are most likely very thin plates—or possibly needles—which are orientated with their long axis in the lines of force; double refraction is positive. In course of time the particles grow, as a rule plates tend to become thicker. Owing to this, the external anisotropy diminishes relatively, whereas the internal anisotropy might make itself felt more strongly. Now, provided that the susceptibility in a direction normal to the longest axis has the highest value, a point may be reached, in course of time, where the internal anisotropy outweighs the external anisotropy; then the particles are tilted over, they are orientated with

their longest axis normally to the lines of force, the double refraction turns negative. When external and internal anisotropy are equally large, they compensate each other, the particles are not oriented any more, no double refraction is observed.

In fairly old sols of gœthite, prepared by oxidising $Fe(CO)_5$, which are heterodisperse and thus contain particles of different size, such complicated curves are found as shown in Fig. 4, curves III and IV: positive double refraction at low values of flux density, negative double refraction at high values. Considering the com-

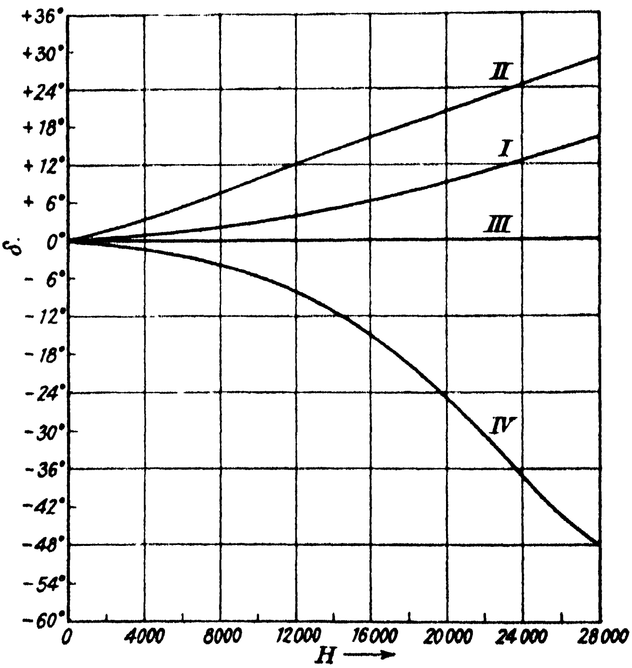

FIG. 3.

petition between external and internal anisotropy this may be explained as follows: when the flux density is relatively small, the orientation of the larger particles is favoured, since they are comparably very thin, *i.e.* they have a strong external anisotropy; their positive double refraction may therefore outweigh the negative double refraction of the smaller particles. At higher values of flux density the smaller particles, which are less anisotropic externally, are orientated to a growing extent, the internal anisotropy makes itself felt more and more, till the negative double refraction, caused by the internal anisotropy, outweighs the positive double refraction.

The most direct experiments showing the truth of these assumptions have not been carried out yet. The following points ought to be tested: are the large particles actually orientated with their longest axis lying parallel to the lines of force? This might perhaps be done using the ultramicroscope. Do the particles really grow in such a way that those axes, which are shorter originally, increase relatively more than the longest axis? In other words: do the plates mainly become thicker? Lastly: is the susceptibility in a small axis really larger than the susceptibility in the longest axis?

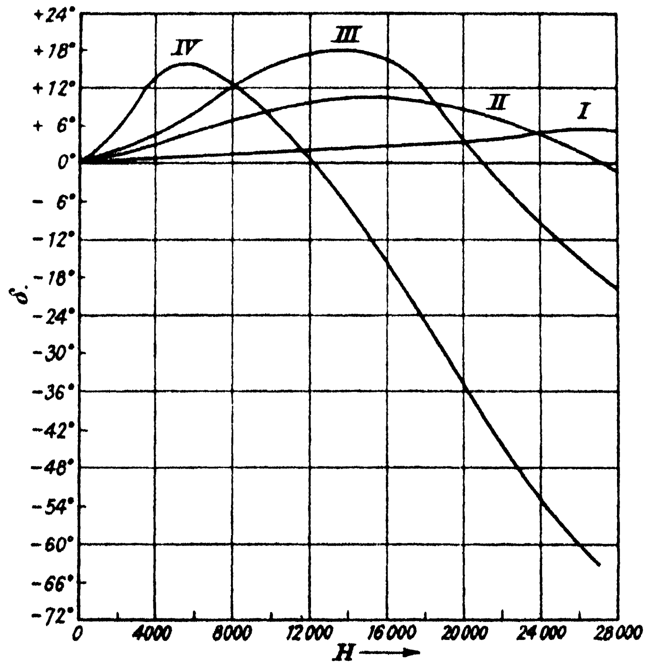

FIG. 4.

But some indirect experiments agree very well with Heller's assumptions. In concentrated and somewhat aged sols of gœthite the larger particles have a tendency to sediment in course of time. If such a sol remains undisturbed in a high cylinder, the liquid near the bottom shows a different behaviour as to magnetic double refraction than the liquid above: the double refraction of the lower portion is strongly positive and reaches a value of saturation at low flux densities. That is just what might be expected with an excess of larger particles, having a strong external anisotropy and being orientated parallel to the lines of force. The upper portion

of the liquid has still a positive double refraction ; but it is weaker than the double refraction of the lower portion. The larger amount of small particles contained in the upper portion causes a stronger negative double refraction, and therefore the positive value in the bulk is reduced.

The following experiment points in the same direction : if a sol of gœthite is stirred vigorously, it is coagulated to a certain extent ; particles are thus produced which have extreme values of external

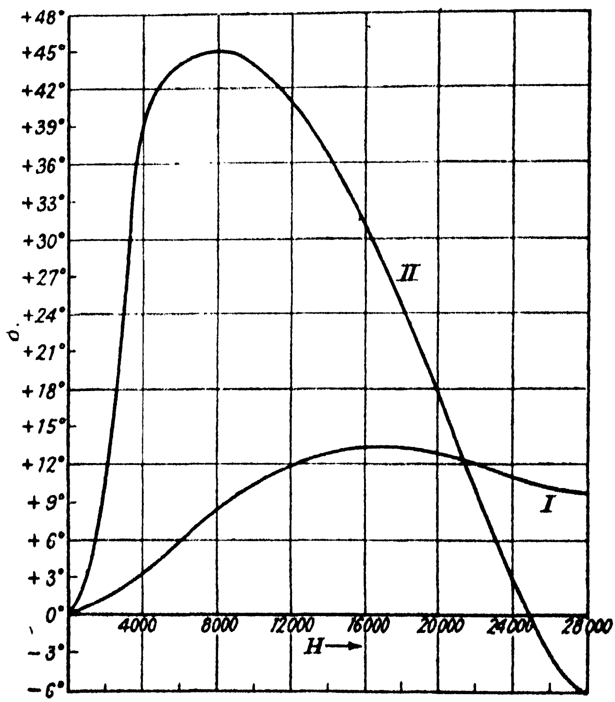

FIG. 5.

anisotropy. This is proved by the fact that the sol shows beautiful streaks on stirring, whereas the original sol, even one which has aged for a fairly long time, does not show them at all. The magnetic double refraction of sols, partly coagulated by stirring, is positive and very strong (curve II in Fig. 5), just what ought to be expected on the assumptions discussed previously. Curve I in Fig. 5 shows the behaviour of δ in the same sol before being stirred.

Sols of hematite and gœthite are not the only distinctive cases. Freshly prepared Graham sols, even concentrated ones, show extremely low values of magnetic double refraction whose increase

with flux density is practically linear. This behaviour may still be observed after the sols have aged for a year or so. They contain most likely a basic iron chloride or several compounds of this kind. Böhm [25] has found that also their X-ray spectrum cannot be identified with the spectrum of any known oxide or hydroxide.

In course of a longer time or on being mildly heated these sols become more strongly double refracting. The birefringence produced in this way behaves as a rule exactly as if it was caused by particles of gœthite. This agrees with the fact that in very old Graham sols, aged twenty years and more, a sediment of a fine powder is frequently found giving the X-ray spectrum of gœthite. Heated sufficiently strongly these sols also might contain hematite particles.

Evidently magnetic double refraction has proved a valuable method for identifying the chemical nature of the particles in sols.

III

The question was discussed fairly soon, whether the special behaviour of the birefringence of muscles might not be correlated in some way or other with the stream double refraction as it is shown by sols with non-spherical particles. But facts did not seem to meet this hope: no protein (or other substance) could be extracted from muscles or be detected in other natural products having marked stream double refraction till a few years ago. Then it was found that there commonly occurs in muscles a protein whose solutions are strongly stream double refracting [26]; its rather exceptional behaviour had made its discovery difficult. It is a globulin, called myosin, which only dissolves in fairly concentrated salt solutions; fresh, chopped muscles have to be treated with a cold 1·2 molar KCl solution of a p_H of about 8 in order to extract the myosin. It is precipitated quantitatively, if the KCl concentration decreases down to 0·1–0·05 molar.

Concentrated solutions of myosin are viscous liquids, showing a strong Tyndall effect; owing to it they look blue in reflected light, golden yellow in transmitted light. They are pronouncedly stream double refracting and behave quite similarly to V_2O_5–sols: on rotating the solution in a cylinder between crossed nicols the black cross appears—the cross of isocline—and the positive double refraction increases strongly with the velocity of flow. In some points there are characteristic differences: the solutions of myosin do not show that tendency to age so distinctive of V_2O_5–sols. Therefore the angle of the cross of isocline is independent of the

age of the sols, and this also applies to the curves showing the relation between birefringence and rate of flow. Obviously the particles in the myosin sols remain unchanged, whereas the micro-crystals in V_2O_5–sols gradually grow in course of time. The shape of the curves, just mentioned, is also different from the shape of the corresponding curves of V_2O_5–sols; these curves do not tend to reach a saturation value at higher rates of flow; in the case of myosin sols the birefringence seems to increase proportional to the rate of flow, as soon as the latter has reached a certain value.

It is most probable that myosin is exclusively the substance which is responsible for the double refraction of muscles. This has been proved chiefly by comparing the behaviour of muscle fibres or, more correctly, of the so-called anisotropic discs in cross-striated muscles with that of a gel prepared from myosin sols [27]. Although this paper deals mainly with anisotropic sols, the anisotropy of this gel deserves to be discussed more fully, since it allows us to distinguish intrinsic double refraction from rod double refraction, a problem not easily solved in the case of sols, as was mentioned previously.

This myosin gel was prepared in the following way: a solution of myosin is squirted into pure water or an aqueous electrolyte solution of suitable concentration; it solidifies forming a thread. In the latter the myosin particles are orientated with their long axes parallel to each other. This is proved by the X-ray diagram, obtained with a bundle of stretched myosin threads. It is a spot-diagram, as is produced when microcrystalline particles are orientated, for instance, with their longest axes parallel to the axis of the thread. This spot-diagram is very similar to that of stretched muscle fibres.

By applying to these myosin threads the method introduced by Ambronn [28], one is able to distinguish intrinsic birefringence from rod double refraction. In the case of rod-shaped particles, orientated parallel to each other, rod double refraction may be calculated according to Wiener's equation [29]:

$$n_o^2 - n_e^2 = -\frac{\phi_1\phi_2(n_1^2 - n_2^2)^2}{\phi_1 n_2^2 + \phi_2 n_1^2 + n_2^2} \quad . \quad . \quad 2$$

Here n_o and n_e are the refractive indexes of the ordinary and the extraordinary ray; n_1 and n_2 those of the particles and the liquid medium between them; ϕ_1 and ϕ_2 are the relative volume concentrations of the particles and the medium of dispersion. Obviously rod double refraction is always positive. It may become zero, if the particles and the liquid medium have the same value

of refractive index. By soaking a birefringent gel containing orientated rod-shaped particles, *i.e.* these myosin threads, with liquids of different refractive index, including the index of the solid particles themselves, one is able to decide whether the birefringence observed is entirely due to rod double refraction or not. In the case of pure rod double refraction birefringence becomes zero as soon as the refractive index of the liquid is equal to that of the particles ; the curve showing double refraction as a function of the refractive index of the liquid medium of dispersion reaches a minimum, when this zero value is attained. If birefringence does not become zero at the minimum of the curve, then its value there represents the intrinsic double refraction of the particles.

Curve I in Fig. 6 represents this curve for stretched myosin threads. Evidently the particles have a marked positive intrinsic double refraction. The way birefringence changes with the refractive index of the liquid shows definitely, on the other hand, that we are dealing with rod double refraction too. But the curve is not strictly symmetrical, as it ought to be according to equation 2. The branch for values of $n_2 > 1{\cdot}576$ agrees with this equation, but the other branch lies too low. The cause seems to be that the liquids with small refractive index, used, do not leave the particles unchanged, they let them swell, etc. ; but one of the main premises of Wiener's equation is that there is no reaction between particles and liquid.

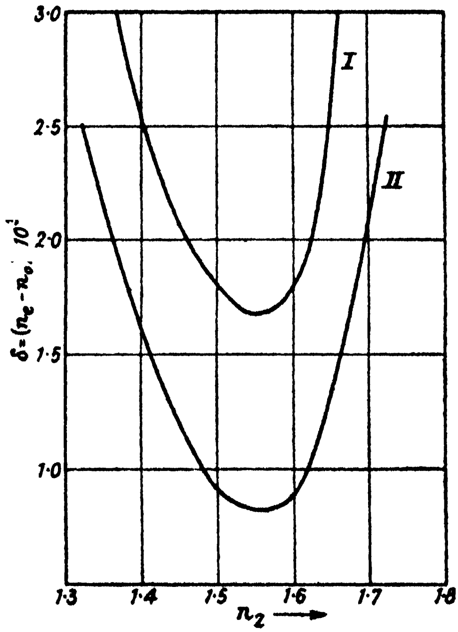

FIG. 6.

The corresponding curve for muscle fibres, curve II in Fig. 6—frog muscles were used—runs parallel to that of the myosin threads ; the total birefringence is again the sum of an intrinsic double refraction of the fibre particles and rod double refraction.—The muscle fibres had been treated with an aqueous solution of formaldehyde, but there was no difference between such fibres and still living ones

as to birefringence.—The absolute values of the double refraction of muscle fibres are 40 per cent. of the birefringence of myosin threads; this applies both to the total double refraction of the original fibres and threads and to their intrinsic double refraction. Assuming the birefringence of the threads to be 100 per cent., all particles consisting of myosin and being orientated, it would be permitted to conclude that the fibres contain 40 per cent. of myosin. In the case of rabbit muscles it was found actually that about 40 per cent. of the total amount of proteins consisted of myosin.

These experiments prove definitely that the myosin particles are rod-shaped. It is not possible to show this in a direct way, for instance, with the aid of an ultramicroscope; under the latter they are not visible at all. This is most likely due to the fact that their dimensions are amicronic—most likely excepting their length—or that the difference in refractive index between particles and medium of dispersion is too small.

It would lead too far to discuss the question, how the very complicated change of double refraction during the contraction of the muscle has to be explained [30].

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THE PHYSICAL LABORATORY AT THE NATIONAL GALLERY

By F. IAN G. RAWLINS, M.Sc. (Cantab.), F.Inst.P.
Scientific Adviser to the Trustees

It is common knowledge that not only scientific testing, but a scientific method of approach has found its way into numerous places where it would have seemed, only a short while ago, somewhat unlikely that it would penetrate. The numerous and varied concerns sponsored, or partially controlled by the Department of Scientific and Industrial Research are cases in point. The latest development is the new installation at the National Gallery for the physical examination of pictures ; a scheme which, with the strong support of the Director, received the official sanction of the Trustees towards the end of last year. The Laboratory is now complete and in working order, and it is probably not too much to say that it represents the nearest approach to the ideal for such investigations that has yet come about. Laboratories for the physical examination of paintings are to be found in connection with museums and institutions in various places : considerable thought was given to the results which these centres had produced before deciding upon the exact form which the new equipment in Trafalgar Square should take. In some important respects however the problems at the National Gallery are peculiar ; one great advantage in planning has been the amount of available space, and another the fact that a large department of photography has long been in existence, the standard of which had reached a high level under the guidance of Mr. W. D. Booker, until his recent retirement. It was natural therefore that the new laboratory should be installed in the proximity of the photographic rooms, and the two sections are now worked as one unit. Photographs of details of pictures and direct enlargements are of great value to students, and the fact that these can be taken and compared with the products of the X-ray and ultra-violet apparatus is of considerable moment. Again, the vast number of pictures available for study encourages the hope that systematic research may flourish and abound as years go on.

PLATE I

↓ X-ray Tube in Cage

Bar and Clamp holding Picture

Frame (*a*)

Screen (*b*)

Easel for the Examination of Pictures.

* The words "in front" and "behind" in the text refer to the sides remote from and nearer to the X-ray tube respectively.

The components (*a*) and (*b*) are those described in the text.

Equipment

A convenient lofty room of appropriate size has been chosen for the physical laboratory, and in it all the plant has been placed. Before describing the apparatus in detail, it may be mentioned that special care has been taken in the decoration and finish of the laboratory to reduce to a minimum the accumulation of dust and dirt; adequate ventilation is maintained and ready access is provided by exceptionally high doors, so that panels of large size can be brought in and out with ease. Box blinds ensure complete darkness for X-ray screening and ultra-violet observations. Special precautions have been taken with all electrical wiring and cables.

A paramount consideration is that of safety. Clearly no precautions are too great both to guard against mechanical damage to the pictures under examination, and to make it practically impossible for anything in the nature of fire to occur. This latter point has been given great weight, since the laboratory is part and parcel of the Gallery, in close contact with its priceless contents. The steps taken with regard to mechanical safety will perhaps best be appreciated when the apparatus is described in detail in the following section.

There are four processes involved, all of an essentially physical character.

(1) X-rays; (2) Ultra-violet; (3) Infra-red; (4) Microscopical. These will be discussed in turn, but before doing so it will be convenient to mention the special easel which is the most novel part of the equipment, and which makes it possible to set up a panel once and for all, to locate any area of it with accuracy, and to subject any part of it to each of the above-mentioned methods of analysis with no further "shunting."

Easel. This consists essentially of a very solid metal chromium plated structure (see Plate I), 10 feet long and 9 feet in height.

Two distinct moving parts are incorporated, (*a*) a frame with backing of sheet aluminium, to which the picture is fixed, and (*b*) a suitable holder for a cadmium tungstate fluorescent screen of exceptionally large size (3 ft. × 2 ft.), provided with lead glass for X-ray protection of the observer.

The frame (*a*) is 6 feet by 4 feet, and is capable of both vertical and horizontal movements, the former by means of counter weights inside the upright pillars of the structure, and the latter by ball-bearing rollers.

Additional weights can be added, to keep the frame in equilibrium when a picture is not present. The fluorescent screen is

given a similar vertical movement, but no horizontal displacement, since the X-ray tube is fixed in a definite position (actually 4 ft. 6 in. from ground level), and the vertical movement is only necessary in order that the picture may be loaded upon the frame carrier, which naturally is behind the member carrying the fluorescent screen. When all is ready, the fluorescent screen is brought to a fiducial mark so that its centre is 4 feet 6 inches above the floor. The relative areas of the picture carrier and screen carrier are such that a picture 6 feet by 4 feet can be explored mosaic-fashion in not more than four movements.

To maintain the painting upon its carrier a stout horizontal bar with suitable clamps and vertical movement grips it gently but firmly at the top, while rests are provided at the bottom of the carrier: as an additional precaution, the horizontal member carries clamps easily attachable, and of a shape to hold the picture should it by any mischance become disengaged from the bar. In order to load a picture, the carrier is brought nearly to ground level, the screen is raised to the top and the whole adjustment rarely takes more than a couple of minutes. Before being brought into use, the easel was tested with a dummy panel of appropriate area and bulk.

For X-ray photography three alternative courses are open. (1) The film is placed upon the back of the screen carrier (which is provided with a series of ruled co-ordinates to assist in obtaining the correct position for the film), or (2) the screen is lowered and the film supported by a separate carrier, or (3) the fluorescent screen image itself can be photographed. Experience shows that, on the whole, (2) is preferable, and this is the method usually adopted. The distance between picture and film is an inch or less, whereas the distance from picture to X-ray tube is seven feet.

It will naturally be asked, what happens when a panel or canvas larger than the dimensions of the carrier (6 ft. by 4 ft.) is to be examined. The method of dealing with such cases (which are not very frequent) is to support the picture upon stout "bankers," and to lash it to the top horizontal member of the easel. It is clear that the determining factor is, in the end, the height of the X-ray tube above the floor (4 ft. 6 in.) which means that an area of which the lesser dimension is nine feet or less can be tackled. Calculation shows that, with due preparation, almost every picture at present in the Gallery can be coped with, so far as dimensions are concerned. Allowance has been made for considerable variation in thickness, and short of an altogether abnormal panel, the easel may be relied upon to function conveniently. Experience has already been

gained with exceptional sizes, and no undue difficulty is expected on this score.

Two considerations have prompted the design of the easel, incorporating the vertical position for the picture under test. The first is safety—no part of the X-ray or other apparatus is in the least likely to drop on a picture—and in the second place all portions of a surface can be explored, which is scarcely possible otherwise.

The horizontal position sometimes adopted has been definitely rejected, both on the score of safety and of convenience in operation.

It has already been indicated that a picture once in position upon the easel can be examined by means of all the types of radiation with which the laboratory is equipped. Only the X-ray unit, now to be described, is a fixture : the other appliances are movable and can be brought up to the easel as required. The special microscope (to be described later) is arranged for both horizontal and vertical working : the usual practice is to investigate small panels horizontally. The travel and stand of this instrument are such as to enable one to take full advantage of the arrangements of the easel when needed.

X-Ray Unit.—The X-ray equipment is of standard design, consisting of tube, transformer and switchboard : suitable diaphragm controls are mounted upon a movable stand, with a view to easy manipulation when screening.

Experience has demonstrated that, for X-ray photography, the best results are obtained by working at 20 kilovolts, or even less, and a current of some 30 m.a. The exposure times are, naturally, somewhat long—about three minutes—but this is no drawback for work of this character. A thermo-syphon cooling device is fitted to the tube.

An important point, and one which usually proves a difficulty in X-raying pictures, is to obtain delicate gradation (corresponding to the layers of paint) in the photograph. So much depends upon this for purposes of interpretation that special care has been given to this question. The use of exceptionally low kilovoltage is helpful, and experiments have been made in which the kilovoltage has been raised for some fraction (say about one-twentieth) of the exposure time, thus bringing out areas of pigment of higher density to advantage. So far, this differential method seems to be decidedly promising.

Much time has been wasted and trouble incurred in the past by overlooking the essential nature of an X-ray photograph of such an object as a panel or canvas. The problem presented to the observer of such a photograph is of a three-dimensional character

projected into one of two dimensions. Otherwise expressed, one is confronted with something like several layers of print superimposed upon each other, and the task is that of disentanglement. These layers correspond of course to the successive layers of wood, gesso and paint of which the painting is composed. The differential method of exposure already referred to promises to be of decided use in stressing those layers corresponding to certain constituents, and in this way making the unravelling process a little easier.

In general, the method is to standardise the conditions, both electrical and photographic, as far as possible and to vary the exposure times as experience may suggest. Neither X-rays nor any other physical tests are infallible guides to the condition or authenticity of a picture, but there is little doubt that the scientific method is capable of providing information of considerable value to the art expert and historian, when the evidence is properly handled and assessed.

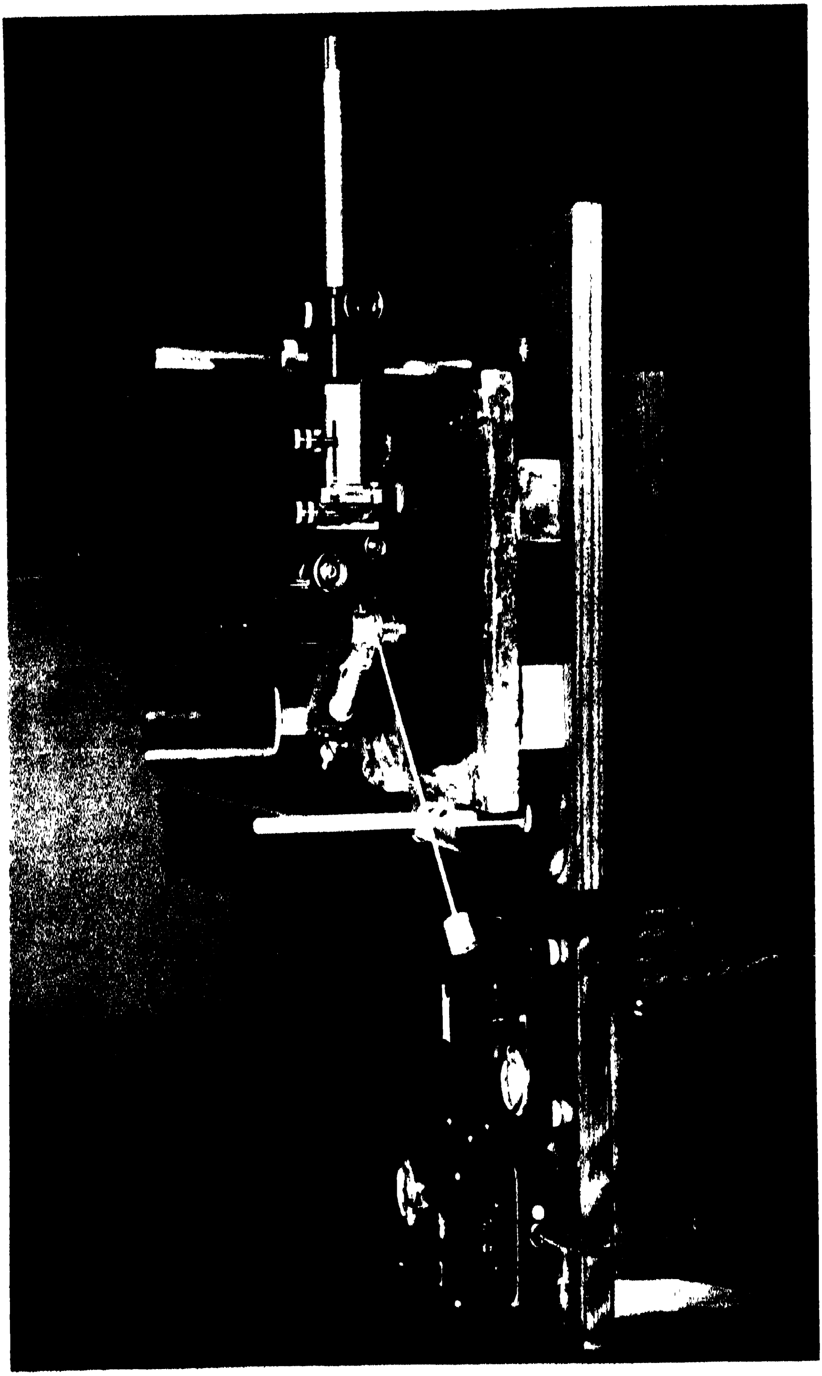
Once it is grasped that an X-ray photograph has, so to speak, "lost" one dimension, then the work of interpretation has at least progressed one step. Careful checking with the surface appearance of the picture itself and with detail ordinary photographs of high quality, are obvious precautions. Fortunately, too, the scientist can co-operate with his colleagues in art history on many points.

For screening, about 5 milliamperes at 35 kilovolts are used for a panel of normal character.

Ultra-violet Analysis.—It is well known that fluorescence of chemical compounds such as pigments is excited by the incidence upon them of ultra-violet radiations: this property has therefore obvious applications for the examination of pictures. Areas which have been repainted can frequently be detected in this way, and considerable help may be given to the cleaner and restorer in telling him exactly how far he needs to go, thus obviating any danger of cleaning beyond the area to be treated. The main difficulty in ultra-violet work is the presence of varnish. This tends to fluoresce itself, and consequently to mask what is beneath. When however the varnish layer is not too old or too discoloured, fair results can be expected.

The apparatus consists of a quartz-mercury lamp of standard design, except that the hood and filter have been arranged to illuminate an exceptionally large area, actually about one yard square. In order to observe the differences brought out by ultra-violet illumination, it is essential to have the room perfectly dark and for the observer to remain in the dark for some ten minutes before beginning operations, in order to get the eye sufficiently sensitive.

PLATE II



Polarising microscope, showing picture under examination.

With a suitable filter however, photographs are easily obtained with reasonably short exposures (about eight minutes with rapid plates), and interesting details are sometimes revealed, or at least made more obvious.

In order to provide a rapid visual comparison, a " Restlight " artificial daylight lamp is fitted on the same pillar as the ultra-violet burner. Thus one can switch over to " daylight " immediately, and since it may be necessary to return to ultra-violet observation, a bulb of low candle power is fitted to the " Restlight," so as to avoid undue disturbance of vision. A transformer and rheostat insure constant intensity from the ultra-violet lamp.

INFRA-RED.—Infra-red photography as far as wavelengths of about one μ is now feasible. A suitable tungsten lamp is available, and the technique follows standard practice. The value of infra-red photography applied to pictures consists in the possibility of penetrating a surface layer of dirt, and perhaps of revealing some detail such as a signature which has apparently become obliterated. Naturally, the character of the surface layer is the determining factor.

MICROSCOPICAL ANALYSIS.—The microscopical examination of a painted surface is usually somewhat unsatisfactory by the usual method of vertical illumination. The difficulty is that much light is scattered, and flare results. Also, if cracks or indentations are present, little is perceived that cannot be seen with the naked eye or a low-power magnifying glass.

These troubles are almost completely overcome by the use of polarised light (using ordinary nicol prisms), combined with an objective system designed for conical, rather than vertical, illumination. Thus a polarising " ultrapak " microscope has been obtained, and has already given definite proof of its usefulness.

From the illustration it will be seen that vertical and horizontal movements of very considerable range are available, and experience shows that the stability is excellent.

Two illuminants are provided, a more powerful one for photomicrography, and a pea-lamp for ordinary visual purposes. Both are carried on an arm attached to the body-tube, rendering the whole apparatus very convenient to use (see Plate II).

The magnifications obtainable are from 19 to 275, and a small attachable camera of the usual pattern suffices for photomicrography.

The value of microscopical methods is only beginning to be taken seriously in this field. Chief amongst the possibilities are (1) the determination of the areas of repaint, (2) the character of crackle, *i.e.* the minute cracks with which the surface of a painting is fre-

quently covered, (3) the nature of cracks of all kinds, whether they are true or spurious, whether empty, or filled up with some material, intentionally or otherwise, and of course whether they are paint cracks at all, or derive from the wood or the varnish, (4) the distribution and nature of pigments. At fairly high magnifications the separate grains are clearly visible: sometimes mere shapeless fragments, but sometimes pseudo-crystalline. The characteristic structure of gold leaf is very pronounced. Much observation and practice is needed, but it is not impossible to foresee the day when a certain studio-technique will be, if not recognised for certain, at least strongly indicated by its microscopic appearance. When it is recalled what a rigid apprenticeship was required of all aspirants to a place in Italian studios before they became master craftsmen, it is not very surprising that the resulting technique may be such as to be capable of more or less exact recognition by controlled optical means such as microscopy. Undoubtedly, too, brushwork is a very individual accomplishment, and problems of authenticity on this account are within range of research of this kind.

A number of ancillary questions are being tackled in the laboratory of the National Gallery. These include the recognition of the woods used for panels (often a valuable criterion in attribution), the nature of varnish (and the problem of its "ageing") and the fading of artist's colours; in fact, almost anything that assists in the preservation and better understanding of pictures.

To conclude, a few words may be added upon the all-important matter of collaboration between art student and scientist.

Doubtless, intuition is the essential possession for the former ("an eye for a picture"): the latter is called upon to make precise when precision is possible, to weigh evidence and to confirm. The physicist should not be content merely to provide data—with humility and diffidence he may express opinions concerning physical possibilities or the reverse. Above all he will resist the temptation to obtain technical triumphs merely because they are satisfactory to himself: photographs or other documents of whatever kind become valueless when they fail to tell the art student anything he did not know before; or when they are merely the outcome of an exaggerated passion for prestige.

Art critic and scientist are perhaps new to finding themselves in double-harness: physics as a fundamental study has achieved many and various successes; it remains to be seen whether an increased knowledge of the nature and development of painting will be numbered amongst its conquests.

THE INTERPRETATION OF ANIMAL BEHAVIOUR

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In the April number of this Journal Dr. Zuckerman [1] has given an interesting exposition of his views on the interpretation of animal behaviour, the topic of a discussion in a joint session of the sections of Zoology and Psychology during the Aberdeen meeting of the British Association for the Advancement of Science in September 1934, in which he took an important part. I am much indebted to the Editors for giving me an opportunity of expounding my views on this point in the same journal, and at the same time to make a few remarks on Dr. Zuckerman's views, the more so as, at the Aberdeen meeting, I was invited to open the discussion, and therefore had no opportunity to reply to his criticisms.

I believe that, with regard to the interpretation of the behaviour of animals, three main tendencies exist, which we may distinguish as the physiological, the behaviouristic and the psychological.[2] The first of them consists in an attempt to analyse the actions of animals into as complete a number as possible of simple reflexes, in the belief that in enumerating the reflexes that built up these actions, the actions themselves are explained. So Szymanski tried to analyse a part of the mating behaviour of the snail *Helix pomatia* into a few simple reflexes, and Peters to analyse the action of a spider catching its prey in its web into five such reflexes. Doflein went

[1] S. Zuckerman, "The Interpretation of Animal Behaviour," Science Progress, 1935, XXIX, 639.

[2] I believe this distinction between three tendencies in the interpretation of animal behaviour is preferable to that of Zuckerman, who only distinguishes a "mechanistic" from a "vitalistic" school. Especially the denomination of this "vitalistic" tendency seems to be rather inappropriate: one can be a vitalist, *i.e.* believe that the phenomena of life cannot be analysed without remainder in co-physical and chemical processes, without admitting psychical phenomena as an explanation of animal action. So Driesch was a vitalist in his biology, but was an objectivist in the explanation of animal behaviour.

even further and believed himself able completely to interpret the behaviour of the ant-lion, making its sand-crater and capturing and killing its prey, by admitting nothing more than the working of three simple reflexes. As to the latter case, it could be shown by other investigators on closer examination that the behaviour of the ant-lion included a number of elements of quite a different nature from simple reflexes, such as spontaneity, purposeful variation and persistence of the action, memory and simple emotions, which make it probable that in the other cases too a closer examination would reveal in the behaviour elements that are of essentially different nature from reflexes. But let us rather put the question in a more essential form : suppose we were able to split up without remainder the behaviour of an animal into a number of simple reflexes, would that give us an interpretation of the behaviour of the animal, satisfying our scientific requirements ?

Of course, I am not referring to the needs of the nerve-physiologist, who in the study of the reception of stimuli by the sense organs, their conduction through the nervous system, and their action on the effectors, finds the sole aim of his investigation. I am speaking here about the man who tries to understand why the animal acts as it does. Let us for the sake of clearness suppose an analysis of this kind were offered as the result of an interpretation of human conduct. We know that our movements, too, contain a number of more or less simple reflexes, and that, *e.g.*, the act of walking requires the functioning of a number of such reflexes. Yet everybody will admit that the enumeration of these reflexes would give only a very poor and unsatisfactory interpretation of, say, our running to catch a train. We feel that by this analysis we at best come into the possession of a number of parts, the link making the parts into one whole being absent. There is something lacking, and we feel we want something of quite a different nature. Is the case different, when we are attempting to interpret the behaviour of animals ?

Dr. Zuckerman did not fail to face this argument of the inadequacy of physiological analysis as a mode of interpreting animal behaviour. His argument against it is, first, that we have no right to admit that "science has already uncovered all the reflexes, tropisms and other basic mechanisms of behaviour that it can ever possibly unfold." This may be true. But a discovery of some dozens of new reflexes will never make up for the "discrepancy between the sum total of the separate items, such as reflexes, that have come out of the investigation, and the primary behaviour material that was put into the investigation." For we feel, as I

said before, that, if we wish really to understand the behaviour of an animal, we need something of quite a different nature from these reflexes and tropisms. And when Dr. Zuckerman remarks that this argument regarding the inadequacy of physiological analysis is based upon a misconception of the intention of scientific investigation, which is directed to the isolating of parts in a system, then we would answer that we can only agree with this, as long as such "isolates" are not merely of a physiological nature, but when psychological phenomena also may be regarded as a possible result of such an analysis. If this is the case, we shall certainly find that after this analysis it will not be impossible "to reconstruct the gross phenomena of experience by the recombination of hypostasised scientific isolates," as was the result of Zuckerman's physiological analysis.

But let us consider the other ways of interpreting the behaviour of animals. They differ from the first mentioned tendencies chiefly in the fact that they do not go so far as to analyse that behaviour into such elements as reflexes, but try to interpret it by means of more complex phenomena. Here again we may distinguish between two antagonistic tendencies. The first, which we may call the behaviouristic interpretation, attempts to do this by describing it in terms of objectively perceptible phenomena, such as the stimuli that act upon the animal and the reaction the animal shows as a response to each stimulus. The other, the psychological one, does not content itself with this one-sided point of view, but thinks it desirable and necessary to take a broader standpoint, and also to take into account phenomena, which we know influence our own actions, so that they cannot be neglected when we attempt to interpret our own behaviour, although they are not directly perceptible to others. These are the so-called subjective or psychical phenomena, subjective because they are bound to ourselves in so far as we are experiencing subjects, psychical because they are immaterial and do not belong to the world of matter. The question now arises whether the former interpretation satisfies our scientific requirements when we set ourselves to understand the behaving animal, and, if not, whether the latter interpretation is possible, or, although preferable, is impossible for a priori reasons, viz., that such subjective phenomena do not exist in animals, or that, if they do exist, they are not knowable to us to such a degree as to be used as elements in a scientific interpretation.

It will be clear that if such an interpretation in terms of subjective phenomena is possible in the case of animals, if it is possible to take into account their perceiving, their feeling and striving when

we try to interpret, say, their flight from a menacing danger, this kind of interpretation will satisfy us more than the one in which these phenomena are neglected. For this again we may look to ourselves. If an observer were to explain our activity when trying to reach the shore after having fallen into the water, as a partly innate, partly acquired "behaviour-mechanism," evoked as a response to the stimulus of the water, we should recognise this as a very unsatisfactory way of interpreting our actions. We should feel that very important and fundamental elements had been neglected, as for example our fears as to the possibility of death, our looking out for a place of safety, and our actual striving to reach this, and we should know that these phenomena had influenced and governed our actions and movements much more than the stimulation by contact with the water alone would do. Therefore it will be clear that we shall also accept such an objective interpretation of the behaviour of animals only when we are forced to believe that the other is impossible. Let us see if such is the case.

As I have previously pointed out, the answer to this problem depends on the question whether such subjective phenomena, as we know within ourselves, do also occur in animals, and, if such is the case, if they are sufficiently knowable to us as to be used as elements for the interpretation of their behaviour. That no subjective phenomena occur in animals at all is a belief that has probably not found many advocates since the days of Descartes; yet some doubt is oft-times expressed if they also occur in the lower organisms. As these subjective phenomena are directly knowable only to the subject experiencing them, and as animals are not able (and probably do not feel any need) to communicate their inner experiences to us, the only way to settle this question is to look for objective signs, for objective criteria, that make it possible to decide whether their behaviour is to be regarded as an expression of subjective phenomena, or is not to be so regarded, in which case it would show an essential similarity with (though perhaps in a more complicated form) the movements we observe in inanimate nature, such as the rolling of a stone or the movements of iron filings towards a magnet.

Dr. Zuckerman is rather pessimistic on this point, and believes that the question whether animals have subjective lives "is one that is impervious to scientific statement and scientific proof." I cannot share his pessimism. As has already been remarked by William James in his *Principles of Psychology*, we find a criterion for the presence of mentality in a phenomenon in the fact that it is directed to a goal, in its "pursuance of future ends." Zuckerman

meets this mark of purposiveness or mentality by the remark, that "the most obviously purposive systems are man-made machines." I believe that he in doing so overlooks the fact that machines are not purposive by their own nature, but are only appropriate constructions, made by man after the example of his own purposive activity, so that in their movements they show only a seeming purposiveness and lack the essential characteristics of real purposive action (*vide infra*). Dr. Zuckerman attaches much value to an apparatus, constructed by Hull and Baernstein,[1] "whose behaviour provides an amazing parallel to the various phenomena of the conditioned reflex." I certainly admire in constructions like this the ingenuity of the builder, who succeeds so well in imitating the phenomena of life in inanimate material, but think it rather naïve to accept these crude analogies as a kind of explanation of biological phenomena. In the same way I cannot accept a doll that "closes" its eyes when it is laid on its back as an "explanation" of the phenomenon of sleep. For that matter, the difference between the imitation and the real thing is too essential.[2]

But we may go further in tracing subjective phenomena in animals. To Professor McDougall[3] we owe the discovery of seven objective criteria that enable us to recognise real behaviour, *i.e.* physical activity in which mental processes of men and animals find expression; in other words, to recognise subjective life in living beings. It seems worth while to mention them here. They are:—first, that of the spontaneity of movement, in opposition to the passiveness of the inanimate world; then, the persistence of the activity, independently of the continuance of the impression that first evoked it, and the cessation of the activity, as soon as a particular goal has been reached; next, the variability and unpredictability of the action, the preparation for a new action, the totality of the action, in which the whole organism is involved (in contrast especially to the reflex, which only involves parts of the body); and, finally, in many cases, the improvement of the action under the influence of experience, acquired on former occasions. By means of these seven criteria we may distinguish behaviour, as an expression of subjective phenomena, from mechanical move-

[1] C. L. Hull and H. D. Baernstein. A mechanical parallel to the conditioned reflex. *Science*, **70**, 1929.

[2] Hull and Baernstein are of another opinion. They hope to organise the phenomena, imitated by their apparatus, into a system which shows nothing less than "true trial-and-error learning with intelligent selection and the elimination of errors, as well as other behaviour ordinarily classed as psychic" (!)

[3] Wm. McDougall, *An Outline of Psychology*, London, 1923.

ment, and where we find them in animal activity we may conclude that some mental life, some subjective phenomena, are at the bottom of this.

On a former occasion,[1] while following this line of thought, I tried to show that we find these seven criteria of McDougall everywhere in animals, when we closely observe their behaviour. I cannot repeat the result of this examination here. I will only point to the fact that even in the Protozoa the marks of spontaneity, of variability, of persistence, etc., so clearly show themselves in their behaviour, especially in hunting and catching their prey, that there is, in my opinion, no reason to doubt that even in these lowest animals we find in their actions an expression of phenomena, essentially akin to, if perhaps qualitatively different from, those that in ourselves we know as perceiving, desiring or feeling. The belief that subjective phenomena are absent in animals or in some of them cannot, therefore, be regarded as an obstacle to our endeavours in admitting subjective elements into our interpretation of their behaviour.

Another objection is that such subjective phenomena, even if they do occur, are not sufficiently knowable by us as to be useful for such an interpretation. This was already the argument of the objectivists in Germany, when at the end of the last century Beer, Bethe and von Uexküll tried to purge sense-physiology of all terms that were clothed with a subjective meaning, and later, in consequence of this endeavour, declared that animal psychology was impossible. The same argument comes up again with the modern Behaviourists, although with some of them, for instance Watson in his later works, such subjective phenomena not only seem to be ignored as unknowable, but even more or less denied. It is rather difficult to meet the argument that such phenomena are not knowable enough to be utilised. If we admit that they are knowable to a certain degree of certitude, what then would be the degree of knowableness required for their usefulness? The minor degree of knowableness of the mental phenomena of our fellow-men, as compared with our own mental experience, will prevent no objectivist from taking them into account, when he wishes to understand the actions of a fellow-man he meets with in the street, and if in the laboratory he refuses to do so, we can only regard this as a theoretical inconsistency. In the same way no objectivist will hesitate to reckon with the subjective state of a dog or a bull, if he feels the want and necessity to interpret its behaviour—for

[1] J. A. Bierens de Haan, *Die tierpsychologische Forschung, ihre Ziele und Wege*, Leipzig, 1935.

instance when the animal suddenly assumes a menacing attitude towards him. If he is consistent, he will not suddenly ignore these subjective phenomena as soon as he has returned to his laboratory and starts studying the behaviour of a similar animal under laboratory conditions.

Now we often hear the objection that the difficulty of knowing the subjective phenomena in animals becomes greater, the more we descend to the lower animals, as the analogies between our own attitudes and expressions diminish because of the greater difference in morphological structure. I believe this argument to be only partially true. For when we, for example, read fear or anger in the attitudes of an animal, it is not by means of an analogy with our own attitudes that we conclude as to this. It is not by analogy of external signs that we draw conclusions as to special emotions or desires or perceptions in a dog or a bird, but by imagining ourselves to be the animal, by conceiving what would be our perceptions and feelings and desires if we were in the animal's place, possessing its nature and psychical structure. The external attitudes and signs play only a secondary rôle in so far as we have learned by experience that a certain attitude is always associated with a special state of mind in the animal. The basis of our interpretation must be a knowledge of the mind of the animal itself. Therefore, the more the animal is known to us, the surer will be our understanding of the inner aspect of its behaviour. And then we must not overlook the fact that most of our knowledge about the subjective phenomena in animals nowadays is obtained, not by simple observation, but by experiment directed to a special question, like a discrimination between different perceptions, or a remembering, or an understanding of the solution of a special problem. With such experiments much surer results are obtained in this respect than by simple observation. So I believe there is no reason to be excessively prudent and too sceptical, and, as we see that we are justified in admitting subjective phenomena in all animals, we may also utilise them for our interpretation of their behaviour. The fact that in former days errors were made, especially with regard to the degree of explicitness with which a subjective phenomenon is experienced by an animal, cannot be denied, but ought not to dishearten us too much!

Dr. Zuckerman raises another objection to the use of subjective phenomena in the interpretation of animal behaviour. According to him their weakness is that they cannot be defined in a non-ambiguous way, while exact knowledge can only be acquired by making use of objectively definable data, which can be stated

without fear of ambiguity. It is certainly true that so concrete a phenomenon as a "table" can be defined much more easily than, say, a feeling of disgust. But, if we limited our scientific interest to phenomena that were easily definable, the field of science would be unduly restricted. So, for example, the concept of "life" is not easy to define; at least we have not yet a definition of "life" that satisfies everybody. Yet we are very well able to recognise life if we meet with it, and are able to perceive it in a living animal, and fail to do so in a lifeless stone. There is no reason, therefore, to give up the study of life, simply because we cannot easily define its object itself. And, in the same way, if perhaps the phenomenon of "fear" cannot be defined "without the fear of ambiguity," yet everybody knows very well in himself what fear really is, and has about fear a more direct and more primary knowledge than say about some quite objectively definable accessory of his wireless set. And certainly Dr. Zuckerman is wrong when he declares that "the re-introduction of the terminology of introspective psychology would be a disastrous step, calculated only to return the subject to its discredited anthropomorphic and anecdotal phase." Dr. Zuckerman in these words gives evidence of a serious lack of knowledge of the prudent and critical work on animal psychology that has appeared in the last few decades, perhaps more on the Continent than in England, and does injustice to the students of that science who have carefully weighed the terms they use in order to escape the error of overestimating the complexity and explicitness of the subjective phenomena they describe in their animals, and who certainly in their experiments, carried out under strictly laboratory conditions, do not give him any right to the incrimination of "anecdotal" methodology!

A final question:—why and to what end do we feel a need to interpret animal behaviour? Why do we not simply let them behave as they like, and why do we want to explain this behaviour?

This question may be answered in different ways. First of all we may try to understand their behaviour for practical reasons, for a better understanding of the possibilities of their actions and a better government of the animals themselves. For a man who has to do with dangerous animals, or one who wishes to compel animals to do his bidding, it will be useful to understand the springs of their actions. Further we may do it for scientific reasons, *e.g.* for a better understanding of our own nature, which to a certain degree shows affinity to that of the animals, and especially for a better understanding of the child, who in several respects stands nearer to the animal than the adult. So our more profound know-

ledge of the instincts of animals has certainly thrown a new light on human conduct. Yet I believe the chief value of our endeavours to interpret animal behaviour lies in another direction. It lies in the fact that it brings us the material for a science, that has as its object just those subjective phenomena that are discovered by an interpretation of the behaviour of the animals in the sense defined above, and has as its aim the knowledge of the psychical constitution of the different animals or types of animals, *i.e.* the whole structure of cognitive, affective and conative elements of their inner life, that react upon each other and evoke each other. A science like this has the right to bear the name of "Animal Psychology," and it is especially as a basis on which to build up this animal psychology, for which the interpretation of their behaviour is our only source of knowledge, that this interpretation finds its value and significance.

But apart from this last significance I hope I have shown that the interpretation of animal behaviour is only complete when we take into account its subjective experience, and that there is no reason to be too sceptical of the possibility of obtaining knowledge of this inner aspect of animal life.

HOW DRUGS ACT

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How drugs act is the main problem of theoretical pharmacology, but like all fundamental questions in natural science it has to be split up into a number of simpler questions before it can be tackled. The careful analysis of the nature of the physiological responses elicited by drugs and the discovery of the sites of drug action are the first matters to be settled and form the subject-matter of classical pharmacology. A later stage of development is concerned with the quantitative study of drug action. This second stage may provide some clue to the kinds of physico-chemical processes involved in drug action ; it also provides a means of comparing the activities of similar drugs and so of correlating the chemical structure and properties of drugs with their physiological properties. The discovery of the atomic groupings and the physico-chemical properties relevant to any given drug response may be expected to lead on to the final problem, the discovery of how the physico-chemical properties of cells are modified by drugs.

Since drugs do not in general alter the character of the physiological activity of tissues, but only the extent of normal activities, an understanding of drug action may be expected to throw light upon the natural physiological mechanisms of tissues. Theoretical pharmacology has in fact already proved to be a powerful weapon in physiological research.

It is unlikely that any general theory of drug action will be found to cover the great variety of physiological responses elicited by drugs. We may expect to find certain general principles governing the reactions of drugs with cells and to be able to classify the different modes of action possible to drugs. Sufficient is already known of the relation between the chemical structure and pharmacological action of certain classes of drugs to predict with reasonable probability the qualitative nature of the action of certain classes of compounds, but the quantitative aspects of drug action remain beyond prediction and in most cases beyond interpretation. Moreover, almost nothing is known of the intimate physico-chemical

mechanisms involved in the action of drugs. This essay attempts to indicate briefly a few of the general aspects of drug action as they appear to a chemist.

The quantitative study of drug action presents considerable practical difficulty and has been characterised by much erroneous theoretical interpretation. The subject has been admirably reviewed in a recent book by A. J. Clark [1] and need not be dealt with in detail here. One or two points, however, call for comment.

One of the most striking features of drug action is the minute amounts of some drugs which are effective in producing physiological responses. This has naturally led to much speculation. It has sometimes been suggested that some living cells may be too small or that the effective number of drug molecules may be too few for the ordinary statistical laws of physical chemistry to be applied with propriety. This is a question of some importance to physiology, since it is a characteristic feature of hormones and vitamins that they achieve striking physiological effects in very minute amounts.

The most reliable estimates of the size of living cells show that, apart from viruses, most cells are enormously larger than even protein molecules. The volume of a small coccus is of the order 10^{10}–10^{11} cubic Å, whereas protein molecules have volumes of the order 10^4 cubic Å, *i.e.* a small coccus is about a million times larger than a protein molecule. The volumes of most cells in animals are much larger than that of small bacteria; *e.g.* a human erythrocyte has a volume of the order 10^{14} cubic Å and the cell of frog's heart ventricle is about twenty times bigger than this. It is clear therefore that drug molecules, which rarely attain the size of protein molecules, are very much smaller than the cells upon which they act. Clark has estimated the number of molecules of certain drugs required to cover cells with a monomolecular layer. Thus the area of the cells in 1 cu. mm. of frog's heart tissue is 6 sq. cm.; the amount of acetylcholine needed to cover this area is 3×10^{-7} gm. or 10^{15} molecules, and the resulting concentration of the drug in the tissue would be only 1 in 3,000.

The assumption that drugs may form monomolecular layers at the surfaces of cells can be shown to be reasonable for certain classes of drugs. For example, the amounts of the higher alcohols fixed by the heart are of the order necessary to cover the heart cells with a monomolecular layer. Ponder [2] calculated that the hæmolytic action of saponin and of sodium oleate on human erythrocytes involved the fixation by the cell of 10^8 molecules of saponin and of 2×10^8 molecules of sodium oleate per cell. These quantities are about sufficient to cover the cell surface with a monomolecular layer.

There are, however, many drugs with highly specific actions which are effective in amounts much smaller than would be sufficient to cover cells with a monomolecular layer.

The estimation of the amounts of potent drugs which are fixed by cells is a matter of some difficulty. Frequently there is no suitable method of chemical analysis available, and in many cases the amounts of drugs fixed by cells are too small for chemical detection. Consequently rather approximate biological methods of estimation must be used. A known volume of drug solution is allowed to react with a certain quantity of cells and the residual unfixed drug in the separated solution estimated by matching up its effect upon a test tissue with standard solutions of the drug. Another method, which involves a knowledge of the effective minimum concentration of the drug when present in excess, is to transfer a small volume of drug solution from one isolated organ to another and so on until the solution fails to produce its typical effect. In this way several authors have estimated the amounts of cardiac glucosides required to arrest the isolated hearts of frogs and other animals. Some of the most potent glucosides, *e.g.* ouabain, arrest the heart by the fixation of about 2γ [1] of glucoside per gramme of heart tissue. The molecular weight of ouabain is 760 and therefore 2γ contains 2×10^{15} molecules. One gramme of frog's ventricle contains 3×10^{8} cells and consequently each cell fixes about 10^{7} molecules. Each molecule will certainly cover less than 500 sq. Å and hence 10^{7} molecules cannot cover more than 5×10^{9} sq. Å. But the area of a frog's heart cell is 2×10^{11} sq. Å and therefore the drug cannot cover more than from 2 to 3 per cent. of the cell surface.

Clark [1] estimated the amounts of several other potent drugs fixed by the tissues upon which they act and found even smaller values; *e.g.* the amount of acetylcholine fixed by the frog's heart during 50 per cent. inhibition was $0{\cdot}02\gamma$ per gramme of moist tissue, and the amount of adrenaline hydrochloride fixed by the rat's uterus during marked inhibition was $0{\cdot}01\gamma$ per gramme of tissue. These figures are maximum values since these two drugs are rapidly inactivated by the tissues. The molecular weight of acetylcholine chloride is $181{\cdot}5$ and therefore $0{\cdot}02\gamma$ contains about 10^{14} molecules. The area covered by this molecule cannot exceed 100 sq. Å and consequently 10^{14} molecules cannot cover more than 10^{16} sq. Å or 1 sq. cm. The area of the cells in 1 gm. of frog's heart is about 6,000 sq. cm. and hence the drug fixed can only cover about $\frac{1}{6000}$th of this surface.

Although estimations of the amounts of drugs fixed by cells

[1] $\gamma = \frac{1}{1000}$ mgm.

are very approximate, these figures prove that potent drugs achieve their effect without forming a monomolecular layer over the whole surface of the cells and suggest that the drugs exert their action upon some specific receptors which constitute an insignificant fraction of the total cell surface.

The view that cells have not a uniform surface but possess specific receptors or "active patches" has proved very valuable in other fields and was introduced by Warburg [3] to account for the results of his classical experiments on the respiration of cells. It is generally agreed that oxygen uptake is confined to specific points on the cell surface and that there are several varieties of receptors. It is probable that narcotics and cyanides, which inhibit respiration, also act at specific, but not necessarily the same, receptors. Quastel [4] has also adopted the hypothesis of specific receptors to account for his results on the oxidative reactions achieved by bacteria.

It has been assumed in the foregoing discussion that drugs act mainly, if not entirely, at the surface of cells and it is interesting to consider some of the evidence for this view.

There are several lines of evidence for the view that drugs behave differently inside and outside the cell. The clearest evidence is derived from the micro-injection of drugs into unicellular organisms; this technique was developed by Chambers [5] and enables one to compare the action of a drug when applied outside the cell and when introduced directly into the cell interior. Chambers found that sodium and potassium chlorides were more toxic to amœbæ on external application, whereas magnesium and calcium chlorides were more toxic on micro-injection. Hiller [6] found that narcotics which paralysed amœbæ immersed in them produced no narcosis on micro-injection. Brinley [7] found that hydrocyanic acid, which paralysed amœbæ immersed in it, was completely innocuous on micro-injection; solutions of hydrogen sulphide behaved similarly. These results clearly show that some drugs exert their specific action on the cell surface of amœbæ and not on the interior of the cell.

Another clear example of a drug acting differently inside and outside cells is provided by Cook's experiments on the action of methylene blue on the isolated frog's heart [8]. This dye has a powerful atropine-like action on the frog's heart and a heart perfused with it is deeply stained. No measurable amount of the dye can be removed by washing out with Ringer, but such washing immediately abolishes the atropine-like action. Hence the pharmacological action can be abolished when the heart is still deeply

stained by the dye. Moreover, the staining process is slower than the onset of the pharmacological action, since the latter can be observed before there is any visible staining of the heart. Hence there appear to be two actions: a rapid and reversible atropine-like action of the dye at the cell surface and a slow and irreversible entry of the drug into the cells, where it has no pharmacological effect.

Numerous other examples might be quoted. Although it would be unwise to assume that all drugs exert their action at the surface of cells, yet it appears certain that many drugs of the most diverse character do act in this way. Moreover, many of the most potent drugs appear to act only at certain specific areas of the surface of cells.

But when we come to consider what sort of chemical or physical action is involved in the reaction of drugs with cell surfaces or with "active patches" on the cell surface, it is remarkable how little evidence is available for forming an opinion. The chief reason for this is our ignorance of the nature of cell membranes.

The interior of cells appears to consist of an aqueous solution of proteins and salts and nearly the whole of the water is "free" in the sense that it can dissolve in a normal manner substances added to it (Hill [9]). The interior also contains a nucleus and may contain a variety of granules. The composition of the salt solution in the interior of cells is usually quite different from that which obtains in the tissue fluids; *e.g.* vertebrate muscle cells contain much more potassium than sodium, but the reverse is true of the fluid which surrounds them. Consequently we must postulate a cell membrane impermeable to the passage of salts. But Clark has shown that potassium can leak out of the cells of the frog's heart and the same author has demonstrated that rubidium can partly replace potassium in the cell interior. Cowan [9*a*] has also shown that the nerves of Maia crabs are permeable to potassium although the nerves normally maintain a potassium concentration thirteen times greater than that in Maia blood. Consequently we must postulate a cell membrane which can become permeable to salts, but which at the same time functions in such a way as to preserve totally different concentrations of salts inside and outside the cell. It is also known that the maintenance of the integrity of the cell involves the continuous expenditure of energy. Our knowledge of physical chemistry is at present totally inadequate to account for a membrane of this description.

The cell membrane is usually thought to consist partly of lipoids and partly of protein. We have already seen that there are reasons

for thinking that it is not a uniform skin enclosing the cell contents, but that it possesses a structure and, in particular, parts of its surface are more sensitive to drugs than others.

There are three possible ways in which drugs may react with cells : (i) by adsorption at the cell surface, (ii) by differential solubility in the cell membrane and (iii) by chemical reaction with some constituent molecules of the cell surface.

It might be thought that a study of the relations between the concentration of drugs and the extent of their actions, and of the time relations of drug action might enable us to distinguish between these three types of process. Unfortunately this expectation has not been realised. Even the simplest drug reactions, *e.g.* the reactions of a drug with a uniform population of single cells, are complex heterogeneous processes and may involve more than one of the three possible mechanisms (adsorption, solution, and chemical reaction). Moreover, it is usually impossible to measure biological activity by any but approximate methods and consequently the quantitative data are not sufficiently accurate to enable us to discriminate between, for example, an adsorption and a solution effect. The prolonged dispute as to whether chemically inert narcotics act by differential solubility (Overton-Meyer theory) or by adsorption (Traube's theory) is a good example of the difficulties inherent in this type of problem. Again, the measurement of temperature coefficients of drug actions provides very little information, partly because the properties of the cell itself are altered by changes of temperature. The real difficulty of all studies of this kind resides in our inability to isolate the particular reaction involving the drug from the rest of the activities of the living cell. All drug reactions probably involve a complex chain of chemical and physical events and we can only observe the nett result.[1]

It is commonly assumed that the physiological result of drug action is due to a modification of the physico-chemical state of the cell membrane. This may be the result of a purely physical process (adsorption or solution) or of a chemical reaction. The action of chemically inert narcotics is probably a physical process whilst the action of some antiseptics, *e.g.* Dakin's solution, chloramine-T, etc., is certainly chemical. But for the great majority of drugs, and those some of the most potent, it is difficult to obtain evidence which would enable us to decide whether their action is due mainly to physical or chemical processes. The type of chemical reaction

[1] For a detailed discussion of the difficulties involved in the interpretation of quantitative pharmacological data, the reader is referred to the book by Clark [1].

usually envisaged is a combination of the drug with some specific molecules in the cell membrane. Many drugs are rapidly acted upon chemically in the tissues (*e.g.* hydrolysis, oxidation, etc.) but usually in the direction of the production of inert or less active substances, and there is no evidence that these chemical changes are an intimate part of the mechanism of the drug's pharmacological action. In many cases such chemical changes are certainly secondary and they often occur at sites in the body remote from the pharmacologically responsive tissues.

Evidence in favour of the combination of drug molecules with some cell constituent is provided by drugs for which there is a large difference in activity between optical enantiomorphs. This subject was thoroughly investigated by Cushny [10]. The most striking examples are provided by the *d*- and *l*-isomers of adrenaline, hyoscyamine and hyoscine. Cushny argued that a sharp differentiation between optical enantiomorphs implied a chemical mechanism for the action of the drug. Each isomer may combine with an asymmetric receptor in the tissues, but the combinations so formed will no longer be enantiomorphous and consequently will differ in their physico-chemical properties. The differences may be slight or may be considerable so that we may expect to find all examples between no differentiation in pharmacological action and complete inactivity of one isomer. Thus for adrenaline, hyoscyamine and hyoscine the lævo isomers are each twice as active as the racemic compounds and hence the dextro isomers have little if any action. For some other drugs, *e.g.* homatropine, cocaine, eucaine, etc., the ratio between the activities of optical isomers is not greater than 2 : 1, and for a number of drugs, *e.g.* coniine, camphor, tetrahydronaphthylamine, etc., there is no difference in activity between optical isomers. Equality of action for optical isomers does not rule out a chemical mechanism, but difference in the activity of enantiomorphs does seem to imply a chemical combination between the drug and some tissue constituent.

Other factors may be involved : *e.g.* Gottlieb [11] proved that *d*-cocaine is more rapidly destroyed in the body than the more powerful *l*-cocaine, but it is doubtful whether this fact is sufficient to explain the difference in activity of these isomers. Moreover, such an explanation cannot be applied generally ; *e.g.* the stronger action of *d*-hyoscyamine on the brain can hardly be attributed to the more rapid destruction of the *l*-isomer, since the latter has the more powerful action at nerve-endings.

Cushny's views are obviously related to the problem of the specificity of enzymes towards stereoisomers. It is an open ques-

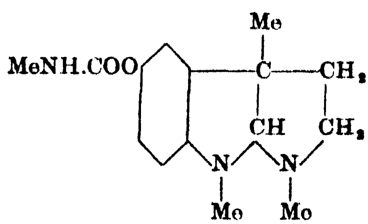
tion whether enzymes combine with substrate molecules or only adsorb them. Perhaps the question has little meaning since it is difficult to draw a sharp distinction between adsorption and reversible chemical combination. Moreover, there appears to be no unambiguous evidence in favour of the differential adsorption of optical enantiomorphs. The partial resolution of racemic dyestuffs by soaking wool in them, the *d*-isomer being preferentially taken up by the wool fibres [12, 12*a*], does not rule out the possibility of chemical combination of the dye with the wool.

The assumption of a chemical mechanism for drug action naturally gives importance to any correlation of the chemical structure and properties of drugs with their physiological effects. Considerable progress has been made on these lines with certain classes of drugs. The whole subject has recently acquired a new significance owing to the development of the humoral theory of the transmission of nervous action.

There have long been known certain classes of drugs which reproduced very faithfully the effects of stimulation of different branches of the autonomic nervous system. This was always difficult to understand, the more so because drugs with similar pharmacological properties were not always closely related chemically. For example, four drugs, acetylcholine (I) a synthetic substance, muscarine (II) a poisonous constituent of *Amanita muscaria*, pilocarpine (IV) the chief alkaloid of jaborandi leaves, and physostigmine or eserine (III) the chief alkaloid of the Calabar bean, all reproduce more or less faithfully the effects of stimulation of the parasympathetic system. A chemical similarity was long suspected between the first two, but otherwise there was no obvious reason for expecting similarity in the actions of these four drugs.

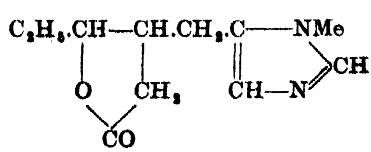
$CH_3.CO.O.CH_2.CH_2.NMe_3\}OH$

(I)



(III)

(II)



(IV)

This example is only a specially striking one of the general arbitrariness of pharmacology which cannot fail to impress any student of the subject who comes to it from the physical sciences.

The discovery of hormones, which may be regarded as endogenous drugs, did something to remove this arbitrary character of pharmacology and gave a greater interest to the whole problem of drug action. The development of the neurohumoral theory has carried this process a stage further.

It is not possible to recount the historical development of the neurohumoral theory here, nor is it necessary since the subject has been recently reviewed by one of its chief exponents, Sir Henry Dale, in his Linacre lecture [13], and by its originator, Otto Loewi, in his David Ferrier lecture to the Royal Society [14]. A brief account of the theory will suffice to make clear some of its implications for any theory of drug action.

In 1921 Loewi demonstrated that stimulation of the vagus to an isolated frog's heart liberated into the perfusion fluid a substance which was capable of reproducing in a second heart the effect of vagal stimulation. Loewi called this substance "vagus-stoff" and was able to discover a number of its properties. He found that atropine (which abolishes the inhibition due to vagal stimulation) annulled the action of the vagus-stoff but did not prevent its liberation. The vagus-stoff was rapidly destroyed by an esterase present in heart muscle, but its activity could be restored by acetylation of the inactivated material, and the action of this esterase was prevented by very small amounts of eserine. In all these properties the vagus-stoff behaved exactly like acetylcholine.

Since Loewi's original experiments it has been clearly demonstrated that stimulation of other parasympathetic nerves is always associated with the liberation of a chemical transmitter indistinguishable from Loewi's vagus-stoff and from acetylcholine. In 1929 Dale and Dudley isolated acetylcholine for the first time from an animal source, *viz.* horse's spleen, and by taking advantage of the inhibition of the esterase by eserine, acetylcholine has been shown to occur in many other tissues.

There seems to-day no reason to doubt that the peripheral effects of parasympathetic nerves are due to the liberation of acetylcholine at the nerve endings. The same is true also of the few fibres, belonging anatomically to the sympathetic branch of the autonomic system, which have long been known to respond to "parasympathetic drugs."

Loewi also demonstrated that stimulation of sympathetic fibres to the frog's heart liberated a different transmitter, which had an

accelerator, adrenaline-like, effect upon a second heart. Further development of this observation, largely due to Cannon and his collaborators, has made it certain that stimulation of sympathetic fibres liberates at the nerve ending a chemical transmitter closely resembling adrenaline in chemical and pharmacological properties, but its identity with adrenaline is not yet fully established.

Finally it has been shown by Feldberg and Gaddum that the transmission of nerve impulses across sympathetic ganglion cells is also accompanied by the liberation of acetylcholine, but the function of acetylcholine at this site remains in dispute.

The chemical theory of the transmission of autonomic nervous effects is now well established and Dale has introduced the terms "cholinergic" and "adrenergic" to distinguish the two types of humoral transmission.

It is natural to extend the humoral theory to the whole realm of nervous action and to suppose that wherever a nerve impulse is transmitted across a synapse (*e.g.* in the central nervous system) or a nerve ending (*e.g.* in voluntary muscles) a chemical transmitter will be found. The work of Dale and his collaborators is indeed making it highly probable that the transmission of nervous excitation to voluntary muscle is a cholinergic process and it seems probable that future work will reveal that the transfer of excitation at all cytoneural junctions is achieved by chemical transmitters.

Loewi has drawn a useful distinction between the chemical transmitters of nervous effects and the true hormones which are liberated at sites remote from the scene of their action and circulate in the blood-stream. He regards the chemical transmitters or neurohormones as the means of initiating action in the body and the true hormones as the body's means of maintaining states. A similar distinction might also be made for drugs, and it is clear that any general theory of drug action must relate it to the normal humoral mechanisms of the body.

Drugs which act at nerve endings provide a good instance in which this correlation with humoral effects can be studied. For example, there are several "parasympathetic drugs" besides acetylcholine which are known to act at the peripheral nerve endings. Some of these, *e.g.* muscarine, eserine and pilocarpine, etc., reproduce the effects of parasympathetic stimulation, whilst others and especially atropine paralyse all parasympathetic terminations.

For stimulant "parasympathetic drugs" several possibilities arise. (i) The drug may be acting as a humoral substitute in virtue of more or less close chemical resemblance to the normal transmitter. (ii) The drug may prevent the normal destruction of the

transmitter and consequently lead to apparent stimulation. (iii) The drug may induce the liberation of the natural transmitter, and (iv) the drug may produce its physiological effect by some direct action upon the muscle or gland quite unrelated to the humoral mechanism.

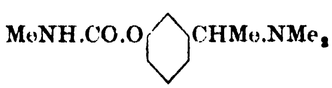
Many esters of choline besides the acetate stimulate parasympathetic nerve endings. Reid Hunt has studied a large number of quaternary ammonium salts of the general formula $(CH_3)_3NR\}Cl$ (where R may be an alkyl group or a short carbon chain carrying an ester, amide or ether group) many of which also have acetylcholine-like properties. Muscarine has recently been shown by Kögl, Duisberg and Erxleben [15] to have the formula (II) related to choline, and it shows a remarkably pure stimulation of parasympathetic nerve endings. It seems probable that these choline esters and quaternary ammonium salts are behaving as humoral substitutes. If we assume that there exists at the nerve ending a specialised receptor mechanism designed to respond to acetylcholine it is reasonable to suppose that other esters of choline and even other quaternary salts with some formal resemblance in structure to choline may act on this receptor mechanism in the same way as, but less efficiently than, acetylcholine. The closer the approximation to the acetylcholine structure the more active the drug will become, but none of the choline esters except pyruvylcholine approaches the activity in high dilutions of the natural transmitter. We meet in this field a degree of specificity which is unknown in chemistry. It is impossible to explain why one particular ester of choline should so enormously exceed other esters in intensity of action. The physico-chemical properties of esters derived from choline and a homologous series of fatty acids will not show quantitative irregularities at all comparable with those which obtain for their pharmacological activities. For this reason several authors have assumed some sort of optimum fit between the drug and the structure upon which it acts.

The facile assumption that drugs which resemble natural hormones in their action are behaving as humoral substitutes must, however, be used with caution. The animal body has open to it more than one way of achieving some gross physiological result and consequently it is important to discover whether the supposed humoral substitute is acting at exactly the same site as the natural hormone. It is also difficult to know what weight to put upon qualitative differences in the action of drugs otherwise closely mimicking the action of a hormone. Such differences may be due to the absence of particular atomic groupings which are present in

the hormone and responsible for some features of its action, but the inexactitude of the correspondence between drug and hormone may be due to a more fundamental difference, *viz.* to the introduction of some new physiological response not characteristic of the hormone.

There are also drugs which imitate more or less closely a natural hormone but which it is difficult to believe on chemical grounds can be acting as humoral substitutes. Examples among "parasympathetic drugs" are eserine (III) and pilocarpine (IV). Inspection of their structural formulæ (p. 259) reveals very little resemblance to acetylcholine.

The purely parasympathetic action of eserine appears from Loewi's work to be due to the inhibition of the esterase responsible for the hydrolysis, and hence the inactivation, of acetylcholine. Stedman [16] has shown that the inhibition of liver esterases by eserine is associated in quite a precise way with definite elements of chemical structure and he has prepared a series of synthetic urethanes which resemble eserine in this inhibition of esterases and in pharmacological properties. One of these urethanes, called miotine (V), was carefully investigated by White and Stedman [17] and found to reproduce all the well-known pharmacological prop-



(V)

erties of eserine. It is, of course, not certain that all the pharmacological properties of eserine and of Stedman's synthetic urethanes can be attributed to the inhibition of choline-esterase, but this is probably true of their peripheral parasympathetic effects. Eserine appears to be the first drug acting on nerve endings for the pharmacological action of which it is possible to give a simple and consistent explanation. Stedman's studies on the inhibition of esterases by synthetic urethanes also make it probable that this action is due to a preferential adsorption of the urethane on the esterase, which is, however, unable to hydrolyse it.

Pilocarpine differs in some respects from acetylcholine. Its most pronounced actions are upon the salivary and sweat glands, which it stimulates to secretion. It has no inhibitory action upon choline-esterase. Its action may be due to a direct action on the tissues it stimulates or to a stimulation of the neurones to liberate acetylcholine. Secker [18] claims to have demonstrated that pilocarpine does induce the liberation of acetylcholine in the salivary gland of the cat.

Burn [19] has also attempted to account for the action of tyramine and ephedrine on the circulation by assuming that at least part of their effect is due to the stimulation of the nerve endings to liberate the sympathetic transmitter. But that some drugs may act by stimulating the neurones to liberate the natural transmitter does not appear to have yet been clearly established.

Another difficult case is provided by atropine which paralyses all the peripheral cholinergic nerve endings, although, as Loewi has shown, it does not prevent the liberation of the chemical transmitter. On Cushny's view a chemical mechanism is probably involved since *d*- and *l*-hyoscyamine differ widely in the intensity of their action. The extensive studies of Jowett and Pyman [20] and others have shown what are the structural desiderata for any appreciable degree of atropine-like action, but unfortunately a knowledge of the atomic groupings necessary for this particular pharmacological action throws no light upon its mechanism.

Enough has probably been said to indicate the way in which Loewi's neurohumoral theory has opened up a new approach to the study of the effects of drugs on nervous mechanisms. It seems reasonable to hope that if the theory can be extended to the central nervous system some insight may be gained into the action of many other powerful drugs. It must be remembered, however, that even if some drugs can be classified in terms of their effects on the normal humoral mechanisms of the body, the problem still remains how the chemical transmitters themselves initiate physiological change.

It has already been mentioned that the correlation of the chemical structure and pharmacological action of drugs has frequently enabled us to associate definite atomic groupings in the drug molecules with particular physiological effects. There is a large class of local anæsthetics which are all benzoic or *p*-amino-benzoic esters of alcohols which contain a secondary or tertiary amino group separated from the hydroxyl by a short carbon chain. Esters of such alcohols with aliphatic acids have no local anæsthetic properties. Many other examples could be given, but the greatest difficulty in such studies is to explain the quantitative irregularities in activity associated with small changes of structure.

There are groups of drugs in which progressive changes in structure can be associated with fairly regular changes in activity; *e.g.* the progressive increase in antiseptic potency (as measured by phenol coefficients) of alkyl-resorcinols as the alkyl group is lengthened up to *n*-hexyl and the progressive decline in antiseptic potency as the alkyl chain is lengthened beyond six carbon atoms. It seems probable that a study of the physico-chemical properties of

such a homologous series would reveal a competition between properties favourable to and disadvantageous to the antiseptic potency; lengthening the chain may at first enhance the favourable properties, but later the disadvantageous properties may outweigh the favourable ones, hexylresorcinol achieving an optimal combination of the two sets of properties.

With many drugs, however, the quantitative irregularities in activity are so considerable that it seems unlikely that the problem can be approached on these lines. The example of choline esters, in which the acetic ester far exceeds in activity all other esters, has already been mentioned. Another striking example also occurs in the choline series. Many esters of choline and other quaternary ammonium salts of the general formula $Me_3N—R\}Cl$ have been shown to have "parasympathetic properties," *e.g.* they inhibit the heart, but replacement of the three methyl groups by ethyl or larger alkyl groups entirely removes all action on parasympathetic nerve endings.

Similarly only quaternary salts with three methyl groups on nitrogen produce the acetylcholine-like contracture of frog's voluntary muscle.

A somewhat different but equally puzzling example is provided by quaternary ammonium salts which Crum Brown and Fraser [21] discovered had the common pharmacological property of paralysing motor nerve endings in voluntary muscle. This property is an extremely general one for onium salts. The activity is largely independent of chemical structure and appears to be purely an ionic effect [22]. A few ions, however, are anomalous; their pharmacological activity is much lower than normal and in some respects is qualitatively different. Thus millimolar solutions of tetramethyl-, tetrapropyl- and tetrabutylammonium salts paralyse an isolated frog's nerve-sartorius in a few minutes, but tetraethylammonium salts have no effect at this concentration and even at ten millimols per litre only achieve complete paralysis after several hours. This striking difference is not due to some peculiar property of ethyl groups, since with arsonium salts the tetramethyl member is the anomalous one and tetraethylarsonium is as active as tetramethylammonium [22]. There seem to be no grounds in the physicochemical properties of such onium salts to account for these striking irregularities in pharmacological behaviour.

It will be noted that none of the examples quoted above involves any configurational factors. The differences between active and inactive substances are simply those between members of a homologous series. It seems to the author that this kind of specificity

can only be explained by assuming such a degree of fit between the drug molecule and the receptive tissue that the actual size of the drug molecule or of particular groups in it may be decisive in determining the physiological activity.

The attribution of pharmacological significance to the unique size of drug molecules may appear strange at first sight, but it is really an extension of well-established ideas. The organic chemist has long recognised that the size of atomic groups may have a profound influence on the properties and reactivity of molecules. There is an extensive literature on steric hindrance which deals with the effect of the size of groups and the subject has recently acquired greater precision since it has been possible to estimate the diameters of atoms fairly accurately.

There is therefore no reason in organic chemistry to regard the size of molecules as necessarily irrelevant to pharmacology. The size factor is really an extension of the conception of fit implicit in Fischer's lock and key simile. Moreover, the assumption that the tissue upon which a drug acts may contain a structure which only drug molecules of a certain size, shape and configuration will fit has physical analogies in adsorption [23]; *e.g.* methylene blue is adsorbed by diamond but not by graphite, succinic acid is adsorbed by graphite but not by diamond (Nellensteyn [23*a*]). The only difference in the adsorbing surface is the spacing of the carbon atoms.

The conception of fit between drug molecules and the tissues on which they act appears to the author to be fundamental to any general theory of how drugs act. Drugs may show very diverse degrees of specificity; for example, narcotics show a low degree of specificity but natural hormones and many alkaloids are highly specific. It is among highly specific drugs that the conception of fit appears to be inevitable.

A very high degree of specificity is shown by the relations between antigens and antibodies in immunology. The fascinating studies of Landsteiner on artificial antigens have shown that the degree of fitting of determinant groups required for the antigen-antibody reaction may be similar to the degree of fit required for two substances to form mixed crystals (Marrack [24]). In this work the nature of the determinant groups was known, but in pharmacology nothing is known of the nature of the receptors on which drugs are supposed to act. Consequently the hypothesis of fit between drug and tissues is at present infertile, and the most urgent need in pharmacology if we are to understand how drugs act is more knowledge of the physico-chemical properties and intimate structure of the tissues on which they act.

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SYMBOLS, UNITS, AND NOMENCLATURE

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THE General Assembly of the International Union of Pure and Applied Physics met in London on October 5, 1934, and, among other things, unanimously approved of Reports presented by Sir R. T. Glazebrook as President of the Commission on Symbols, Units, and Nomenclature, appointed by the Union at the Brussels Assembly in 1931. The Commission has not only collaborated with other bodies such as the Standards Committee of the International Electrotechnical Commission and the Committee of the British Association on Electrical Units, but, during the Electrical Congress in Paris in 1932, took the opportunity of holding an informal international conference. The Reports presented by Sir R. T. Glazebrook were based on the results of these meetings and conferences. They have recently been published as a 40-page pamphlet by The Physical Society (price 2*s.* 9*d.*).

The first report is on the "Standard Thermal Unit" and may be summarised as follows: (1) the Unit of Heat is the Joule $= 10^7$ ergs; (2) the Gramme-Calorie is the heat required to raise 1 gramme of air-free water from 14·5° to 15·5° international scale of temperature at one atmosphere. 1 gramme-calorie $= 4{\cdot}18_4$ joules; 1 international watt-second $= 1{\cdot}0003$ joules. This last figure is accepted as a compromise between the U.S. Bureau of Standards 1·00031 and the N.P.L. 1·00025. The international watt-second involves the international ampere, which is not exactly 0·1 c.g.s. unit, and the international volt, which is not exactly 10^8 c.g.s. units; consequently the international watt-second is not exactly 10^7 ergs, whereas the joule is taken as being exactly 10^7 ergs.

The second report is on Electrical and Magnetic Units and deals with matters on which there is considerable diversity of opinion. There are three main points on which divergent views are held; they are (1) whether the factor $4\pi/10$ should be retained in the expression for the magnetomotive force in practical units, or whether one ampere-turn should be taken as the practical unit; (2) whether

B and H should be regarded as quantities of the same or different kinds; and (3) whether the system of units should be based on the force between unit poles or on the force between current-carrying conductors; a third alternative is mentioned, based on the magnetic flux.

The Report recommends that the $4\pi/10$ be left where it is until there is a preponderance of opinion in favour of a change. Although the practical unit of m.m.f. *par excellence* would appear to be the ampere-turn, the commission is probably acting wisely in leaving matters alone for the present. Although the Report leaves the second question open, the table of recommendations is based on the assumption that B and H are not of the same kind, the unit of B being the gauss and that of H the oersted. This again is a very wise decision since it is much less confusing to have two names for two things which some people regard as being one and the same thing than to have only one name for two things which some people regard as entirely different. The point at issue may be made clear by means of an analogy. When a tensile force is applied to a test specimen it produces a strain at every point throughout the material. Now the usual conception is that the applied force produces a stress at every point in the material, the total applied force being distributed over the cross-section and giving a stress of a certain amount of force per unit area. The relation between this stress and the strain produced at each point depends on a property of the material. The same line of argument may be employed when a m.m.f. is applied to a magnetic circuit by means of a current-carrying coil. One may picture the m.m.f. as distributed around the path giving a magnetising force H at every point of a certain amount of m.m.f. (or ampere-turns) per unit length of path, and causing at each point a magnetic induction B related to the magnetising force H by some property μ of the material. It is, of course, open to anyone to say that the stress in the mechanical case and the magnetising force in the magnetic case are fictions, and that the only realities are the applied load or the ampere-turns and the resulting strain or magnetic induction. To one who regards H as the cause of B, permeability is not a mere number but, even in a vacuum, has a value μ_0 representing a magnetic property of space. The increased value of B in iron is presumably due to the addition of the internal molecular magnetomotive forces to the externally applied m.m.f., thus giving an increased effective permeability which is really the product of μ_0 and a numerical coefficient which may be called the relative permeability.

The definitions given in the table which the Commission recom-

mend to the Union are based on E and I. The formula $d\Phi/dt = -E$ is taken as the defining equation of the magnetic flux Φ and, as is stated, this is certainly defining it in terms of that by which it is measured for practical purposes. The unit of magnetic flux is the Maxwell. The magnetic induction or flux density B then follows from $\int BdS = \Phi$; its unit is the gauss. Magnetomotive force is defined as $F = 4\pi NI$, the unit being the gilbert, which is one oersted-cm. The magnetising force H, or, as it is called in the Report, the intensity of magnetising field, is defined by the formula $\int H \cos \varepsilon \, dl = F = 4\pi NI$, and the unit is the oersted.

At the Paris meeting of the I.E.C. in October 1933 two alternative methods of defining B were adopted, *viz.*, either by the mechanical force on an element of current-carrying conductor placed at the point, or by the electromotive force induced in an elementary circuit surrounding the point. The S.U.N. Commission have only embodied the latter method in their recommendation.

This second Report has six appendices, the first being a reprint of a circular issued in 1931, surveying the problems facing the Commission and giving a number of references showing the apparent inconsistencies in Maxwell's attitude to the relation between B and H. He sometimes refers to them as if one was cause and the other effect, and sometimes as if they were one and the same thing. The second appendix is a very satisfactory discussion of the three alternative methods already referred to as a basis on which to build up the electromagnetic quantities. Of these, the classical method based on the force between two magnetic poles (Coulomb) compares unfavourably with that based on the force between current-carrying conductors (Ampère) in that it is less fundamental and more obscure, especially when the effect of varying the medium is considered. The third alternative, which is free from theoretical abstractions like unit poles and elements of current, is to define the unit quantity of electricity by electrolysis; this gives the unit current, and, from the unit of energy, the unit potential difference and e.m.f. Magnetic flux can then be defined as recommended by the Commission. The so-called absolute c.g.s. system is similarly based on the length of a platinum-iridium rod, a mass of platinum, and the second of mean solar time.

The sixth appendix explains the m.k.s.Ω system advocated since 1901 by Giorgi. It is now generally recognised that the Victorian idea of finding a purely mechanical explanation of electromagnetic phenomena is no longer tenable and that electrical quan-

tities cannot be defined in terms of length, mass, and time. It was due to arbitrary assumptions made in order to avoid the introduction of a fourth fundamental that various systems were evolved in which the same quantity appeared to have different dimensions and that generations of students have gone forth with the idea that electrical resistance was in some mysterious way of the nature of a velocity, or alternatively, of the nature of the reciprocal of a velocity, depending on which column one consulted in a table of dimensions. The Giorgi system not only removes all this confusion by using a fourth fundamental, but, by using the metre and kilogramme instead of the cm. and gramme, gives units which have a convenient magnitude for most purposes without disturbing the established practical electrical units. There is some difference of opinion whether the fourth fundamental should be the resistance of a column of mercury of given dimensions, or the quantity of electricity depositing a given mass of silver, or some other electrical quantity.

The third Report deals with thermodynamic symbols, the fourth with the future work of the Commission, including a recommendation that they be authorised to continue their work. The fifth Report is in French and is contributed by Professor Abraham of Paris (not the Abraham of the well-known German text-book). It deals with the B and H question ; he regards them as magnitudes of the same nature.

The sixth and final section is a mathematical investigation based on Ampère's formula for the force between two elements of current ids and $i'ds'$ separated by a distance r. The reason for including this section is not clear, especially as a footnote states that a similar result is given in Mascart and Joubert. The statement that " each elementary force [on ds′] acts at right angles to the plane containing r and ds " is surely wrong, and a little editing would have avoided flying in the face of the Commission and of common usage by referring to B as the magnetic intensity.

DO BIRDS ATTACK BUTTERFLIES?

A New View and a Critical Analysis of the Published Evidence

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1. A CRITICAL EXAMINATION OF THE EVIDENCE FOR ATTACKS BY BIRDS ON BUTTERFLIES

THE total published evidence for attacks by birds on butterflies can only be regarded as scanty, for the amount of observation devoted to these two most attractive classes of animals by entomologists, ornithologists, and naturalists generally is enormous [*e.g.* *vide* 1, 2, 3, 6, 9, 10 ii, 12, 13, 14, 15, 16, 17, 18, 19]. As Marshall wrote in 1909, "heaps more evidence" is wanted; for "it is curious that not one of those excellent observers, Wallace, Bates, Fritz Müller or Belt, has given us a single record of attack in which either bird or butterfly was identified" [8, p. 383]. Nor can I regard the more recent examples, carefully collected from as many parts of the world as possible, as other than exceptional so far as healthy flying butterflies are concerned. Thus, Mr. Collenette has described frequent onslaughts in Matto Grosso, Brazil [5]; but Mr. Schaus, "from an experience of many years in the forests of Central America," considered that such attacks hardly ever occurred, and Punnett held in 1915 that the absence of records supports this view [11, p. 112]. The paucity of definite evidence for tropical America applies also to Malaya; yet the reverse might be expected if the current views on mimicry in butterflies were correct.

Students of the History of Science are familiar with the idea that otherwise intelligent men have often fallen into the error of imagining that abnormal or even impossible events may occur in remote regions (*e.g.* 9, Chap. IV). It appears that this way of thinking does not easily die out. Thus Scudder, who was only familiar with North America, where he found attacks upon butterflies uncommon, "readily admits that such occurrences are probably much more frequent in tropical countries" [8, p. 331]. Now that more is known about tropical natural history the mystery is sometimes shifted into tropical forests [4, p. 21]. So for England

the abundant negative evidence accumulated from the days of White of Selborne is discounted, emphasis is laid on the occasional assaults that undoubtedly occur, and we are invited to believe, for instance, that kestrels habitually prey upon butterflies on the wing [cf. 8].

As regards the tropics, I think all competent naturalists would admit that birds are active in the early morning and the evening, or the night for some species, but that they are remarkably inactive during the midday heat ; but the latter is the period during which the majority of butterflies are on the wing.

Negative evidence is certainly less cogent than positive evidence. But when the positive cases are few and the amount of negative evidence is very large, the utmost induction that we are justified in making is that positive cases occur sometimes. Negative evidence may be of two kinds. If people are not looking out specially for a phenomenon, lack of observation thereof is rightly considered to be of little value. But if many observant naturalists have watched birds and insects for years and report the absence or rarity of an event, that event is most unlikely to be a common one. Negative evidence is most important and useful in biology, in which knowledge depends so largely upon observation, not upon easily repeatable experiment. To take one familiar illustration, a number of well-authenticated observations has shown beyond doubt that tigers do sometimes attack and devour human beings. Yet it is believed by all students of tigers that normally a healthy tiger will not attack mankind of its own accord. The basis for this belief about the normal behaviour of tigers, which are certainly quite capable of attacking coolies in or near the jungle, is based upon what writers on mimicry in butterflies describe as negative evidence. The number of occasions upon which persons have wandered unscathed in places where tigers occur is enormously large ; secondly, tigers are known, by positive observations, to feed usually upon other mammals, such as pig and deer. Similarly we depend upon induction from the structure and habits of tigers and upon a large amount of negative evidence for the belief that tigers are not habitually herbivorous ; though doubtless, like domestic cats, they may occasionally eat a little grass as medicine, while there seems no reason to query the widespread belief in Malaya that they are very fond of the durian fruit. So the great weight of evidence for the belief that birds seldom attack flying butterflies is not to be lightly dismissed because it is "negative."

The presence of lepidopterous scales in the stomachs and excreta of certain birds has often been adduced as an argument to show

that they eat butterflies. But, as Mr. N. C. E. Miller states, except for the androconia found on certain male butterflies it is difficult to distinguish the scales of butterflies from those of moths; and the necessary examination with a high power microscope does not seem to have been attempted in many of the cases mentioned. So, as there seems no reason to dispute Professor Poulton's statement that birds prefer moths to butterflies as food [10, p. 183], the uncertain argument drawn from the scales found in some avian pellets or crops becomes even less definite than it was before.

Finally, it is undeniable that many biologists approach this question with minds already profoundly influenced by two important factors—a belief, or an inclination to believe, in "Darwinism," *i.e.* evolution through natural selection, and a wish to find an easy explanation for the resemblances found among a number of butterflies as regards their upper wing colours, such as the ordinary mimicry theory supplies. Though some of these resemblances have been exaggerated, the existence of others cannot be denied, at least as far as appearance to human vision is concerned. Any observations which agree with these beliefs, beliefs which are apriori as far as attacks by birds on butterflies are concerned, are therefore noted carefully, and often quoted *ad nauseam*, while evidence to the contrary is discounted. This criticism is still valid for the new and interesting cases of assaults by birds that continue to be published; the large amount of negative evidence, from Malaya for instance, does not receive attention.

2. Conclusions

The conclusions, very different from the beliefs which I held when I commenced this study, to be drawn from an examination of the published evidence upon this subject are therefore as follows:

I. Attacks upon butterflies at rest are fairly frequent, but difficult to observe as sessile butterflies are often well concealed. These attacks are made sometimes by birds, also by lizards, spiders, monkeys, and various other insectivorous or omnivorous animals. The Neo-Lamarckian view, that adult butterflies are hardly subject to attack at all, is therefore incorrect. Poulton's generalisation that "all well-concealed forms are good for food, and are eagerly chased and devoured by insectivorous animals, while unpalatable forms are conspicuously coloured" (10, p. 41) applies well to butterflies at rest, though it indicates that the majority of butterflies in flight are unattractive to the only creatures capable of attacking them, *i.e.* birds. Many of the devices quoted by Carpenter and Ford apply to butterflies at rest, not in flight. The moving "tails"

on the hind wings of such "Hairstreak" butterflies as *Eooxilides tharis* seem obviously designed to protect these insects from serious injury while at rest, when the posterior end of the wings looks like the head; this advantage is not apparent in the flying butterfly.

Many reports of attacks upon butterflies refer explicitly to butterflies at rest. The usual beliefs about concealing (cryptic), warning (sematic) and mimicking (pseudosematic) colours apply to butterflies at rest, not to butterflies on the wing because they are without effective enemies or selective agents until they become feeble, badly damaged, or old. The characteristics of butterflies at rest are found in a more marked degree among females than among males; the former fly less and run special risks during oviposition, which is performed while the butterfly is more or less at rest.

II. Attacks upon flying butterflies are very rare. They can seldom be made except by birds, and are often unsuccessful. Most insectivorous birds, except Drongos, are incapable of capturing uninjured butterflies during flight. Other attacks may be made for fun by young birds, just as a kitten will "attack" a leaf blown along by the wind; or by birds which are unusually hungry or have a hungry brood at home, when all is grist that may come to the mill.

III. Butterflies as a class change their appearance more completely in the positions of flight and rest than any other animals do. The exceptions, such as *Hestia* and *Papilio agamemnon*, do not invalidate this rule. Consequently a rigid distinction should be drawn between attacks upon butterflies at rest and butterflies in flight; but this has not been done up to the present, and assaults upon sitting butterflies by birds and even by other animals have been misused in support of theories about the colours displayed by these insects during flight.

IV. Hence the current theory of Mimicry as applied to the upper wing colours of butterflies is unsound. The numerous attempts to prove that birds attack flying butterflies sufficiently often to be selective agents are unsuccessful, and Professor MacBride is justified in saying that "I do not for a moment suggest that the phenomena grouped together as 'mimicry' have been fully explained, but enough has been said to show that the loose teleological explanation put forward by Bates and Müller and accepted by the majority of Neo-Darwinians breaks down when subjected to detailed criticism" [7; cf. 12, Chap. XI].

3. Final Observations

Nothing is attempted here in criticism of the general theory of Mimicry. It appears to be well established in many remarkable

cases, notably those in which dangerous insects such as wasps are imitated by beetles, and may apply to the *under wing* colours of certain butterflies [*e.g.* 4, pp. 22, 69].

I have confined my arguments to the enquiry as to whether flying butterflies are attacked by birds in any but a few occasional instances or not, and to the bearing of this upon the usual beliefs about mimicry of some butterflies by others. At present I do not attempt to put forward an alternative explanation for the upper surface resemblances found between certain different species of butterflies, principally among the females.

We are told that those who refuse the mimicry hypothesis must propound another one to cover the facts [4, p. 124 ; cf. Poulton, 10, p. 231]. But it is a fallacy to accept a theory merely because there appears to be no better one available at the moment. Almost any notion may be useful as a working hypothesis which evokes further study and observation, but it should be recognised as merely provisional until it is found to accord with all the facts, negative as well as positive. The cocksure attitude of many scientists has received many rude shocks during the present century, and it is better to admit ignorance than to strain an explanation which is plausible for some only out of a large number of apparently connected phenomena. That explanation to a greater or less degree is a function of science is generally admitted [20, p. 293] ; but it is difficult in any given case to say when the primary, descriptive, function of science has been adequately performed ; and without sufficient observation, classification, and description attempts at explanation will be premature and probably erroneous.

This appears to be the position as regards the colours of butterflies, though the ideas of Wallace, Poulton, Punnett and others should prove helpful for future work. Meanwhile, more observations upon wild butterflies and wild birds are needed ; and it is probable that far more knowledge is required about the carry over into the imago stage of properties developed during the longer and more vulnerable larval and pupal periods through which every butterfly has to pass.

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RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., F.R.S.E., Royal Observatory, Edinburgh.

The Atmospheres of the Planets.—In his address as retiring president of the American Association for the Advancement of Science, reprinted as a supplement to *Nature*, 1935, Feb. 9, Dr. H. N. Russell took as his subject the nature and constituents of the planetary atmospheres. The existence of atmospheres on the majority of the planets has been demonstrated by direct telescopic observation ; the varying cloud formations on Jupiter, the alternate growth and shrinkage of the polar caps on Mars, an extension of the horns during the crescent phase of Venus when the planet is within 1° of the sun, occasional and transient white spots on Saturn, may be taken as practically conclusive evidence. On the other hand, there is no sign of either refraction or twilight on the Moon, and Mercury also has been generally supposed to have no atmosphere, though recent observations with the 33-inch refractor at Meudon by E. M. Antoniadi (*Journal of the British Astron. Assoc.*, **45**, No. 6) are claimed to indicate the presence of clouds composed of very minute particles of sand or dust suspended in a very rarefied atmosphere.

The information afforded by the spectroscope concerning the composition of the atmospheres is unfortunately subject to severe limitation, for hydrogen, nitrogen, helium, neon and argon show no selective absorption in the region of the spectrum which is accessible to study. Further, the gases of the earth's atmosphere show very powerful absorption, the effect of the thin layer of ozone being specially troublesome, for it cuts off the whole spectrum short of wave length 2900 Å. In the infra-red the wide bands due to water vapour make things almost equally difficult, and the most interesting parts of all celestial spectra are at present completely blotted out. The terrestrial absorption lines of oxygen and water vapour in the visible part of the spectrum hide, line for line, the absorption by these gases in a planet's atmosphere. This difficulty can be partially circumvented by taking advantage of the line displacements produced by the Doppler effect when the planet is

approaching or receding. The greatest available shift is not sufficient to separate completely the planetary and terrestrial lines, but microphotometer measures of the contours of the blends are able to reveal any slight asymmetry which may be present. Using this method at Mount Wilson, St. John and Nicholson found no trace of oxygen on Venus and in 1934 Adams and Dunham came to the same conclusion regarding Mars. On either planet an amount equal to one thousandth part of that above an equal area on the earth could certainly have been detected. As regards water vapour, though the test is less certain, the quantity on both Mars and Venus must be small. These observations are likely to be repeated in the near future, using red sensitive plates and stronger absorption lines.

In this connection, the question has been raised whether, since all the observations on planetary atmospheres except in the case of the earth must necessarily be made from the outside, the light absorbed by the gases may not be scattered back towards the observer so strongly as to obliterate its characteristic absorption lines in the resultant spectrum. This suggestion has been subjected to an ingenious test at Mount Wilson by T. Dunham (*Annual Report of the Director of Mount Wilson Observatory*, 1934). Although it is obviously impossible to observe the earth's atmosphere directly from outer space, it can be done indirectly by observing the feebly illuminated portion of the moon within the horns of the bright crescent a little before or after the time of new moon. The spectrum is that of sunlight reflected from the earth to the otherwise dark side of the moon and then back again to earth. This sunlight therefore passes three times through the earth's atmosphere, whereas the sunlight reflected from the bright portion of the moon traverses it only once. The difference between the spectra of the two portions of the moon must represent the spectrum of the earth's atmosphere as it would appear to an external observer on the moon. Dunham photographed the spectrum of the earth-shine in the region of the B band of oxygen with a spectrograph of two prisms, the image being formed on the slit by a lens of 90 inches focal length fed by a small coelostat with aluminised mirrors newly coated to reduce scattered light. Photometric comparisons of spectra of the earth-shine with those of the bright moon showed a strengthening of the oxygen band in the former by an amount corresponding almost exactly to what would be expected in a spectrum of the earth photographed from the moon. It appears therefore that it is quite possible to detect the presence of oxygen in a planetary atmosphere by external spectroscopic observation, and that it is

safe to infer that the absence of oxygen lines in the spectra of Mars and Venus does really demonstrate the absence of that element from their atmospheres.

While the constitution of the atmosphere of Mars remains a mystery, bands due to carbon dioxide have been discovered by Adams and Dunham in the spectrum of Venus. Recent investigations by Adel and Slipher, in which light was passed through 45 metres of carbon dioxide at a pressure of 47 atmospheres and the bands found to be considerably weaker than on Venus, indicate that the planet must possess an amount of the gas equivalent to a layer two miles thick at standard pressure and temperature. On the earth there is but little carbon dioxide, and even at sunset the amount in the path of the sun's rays is only equal to thirty feet under standard conditions, so it is not surprising that the bands in question do not appear in the solar spectrum. If, as has been suggested, the supply of free oxygen on earth is due to the action of vegetation on an original atmosphere largely composed of carbon dioxide, the carbon being retained by the plant and the oxygen liberated, then Venus seems to resemble the earth at the stage before life made its appearance. But, as we are still ignorant of the way in which life originated on earth, it is idle to make any conjectures as to the future development of Venus.

In the spectra of the outer planets there are strong bands in the red and infra-red, which were first identified by R. Wildt as due to ammonia and methane. Further and more conclusive evidence has been provided by T. Dunham (*Publ. Ast. Soc. of the Pacific*, XLV, 200, and XLVI, 231). He has definitely identified no less than 69 ammonia and 18 methane lines, and estimates the quantity of ammonia gas above the clouds of Jupiter as equivalent to a layer 10 metres thick under standard conditions of temperature and pressure. The investigation has been carried still further by Adel and Slipher, who by using a 45 metre path under a pressure of 40 atmospheres got enough methane gas in the way of the light to produce bands intermediate in intensity between those in Jupiter and Saturn, and by this means identified practically all the planetary bands. From the published data it appears that the amount of methane above the visible surface of Jupiter must be of the order of one mile atmosphere, and there must be considerably more on Uranus and still more on Neptune.

In his address Russell goes on to consider the probable course of the chemical changes on the major planets. After the solidification of the core the surrounding gases would presumably include hydrogen, helium, oxygen, carbon and nitrogen, together with

smaller quantities of the inert gases, of sulphur, and of the halogens. Under conditions of low temperature and high pressure carbon dioxide and hydrogen would tend to form themselves into methane and water, while the reaction between hydrogen and the higher hydrocarbons would also be towards the formation of methane. Nitrogen and hydrogen would combine to form ammonia. As the planet gradually cooled it would therefore become surrounded by an extensive atmosphere of hydrogen, methane, ammonia and water vapour above an ocean strongly alkaline with ammonia. Still further cooling would freeze the water entirely out of the atmosphere, leaving only hydrogen, methane and ammonia, the last being on the point of precipitation. For Jupiter Dunham has calculated − 120° C. as the minimum possible temperature, for otherwise the quantity of ammonia observed in the atmosphere would be partially condensed under its own weight. He thinks that the gas may be in a condition of equilibrium with the solid phase, and that the clouds may actually consist in part of crystals of ammonia. On Saturn, where the surface gravity is considerably less and the ammonia bands are fainter, the minimum temperature is estimated as 10° or 15° lower than on Jupiter, and this difference is confirmed by the radiometric observations.

Uranus and Neptune are presumably much colder, with temperatures sufficiently low to freeze out the ammonia completely from their atmospheres, leaving them composed of hydrogen and methane, and clear to great depths. This probably accounts for the strength of the methane bands in their spectra, and for the consequent green colour of their discs. On Neptune the methane must be very near condensation point, for the radiation from the sun is insufficient at that distance to maintain a mean temperature higher than − 220° C., which is definitely below the condensation limit. Either the remaining internal heat of the planet or the blanketing effect of the methane envelope itself may be responsible for keeping the latter in its gaseous form, but the margin of safety must be small.

Professor Russell's survey shows clearly how the problems of the planetary atmospheres, which a few years ago seemed hopelessly perplexing, have now, thanks to the workers at Mount Wilson and at Flagstaff, advanced far towards a satisfactory solution.

E. M. Antoniadi has earned an outstanding position as a visual planetary observer, and special interest therefore attaches to his monograph " La Planète Mercure " (Paris, Gauthier-Villars), a work of 72 pages illustrated by 36 figures and 3 plates. This is a summary of what little is known about Mercury, special emphasis being

naturally given to the author's own observations with the large refractor at Meudon. He considers that the period of the planet's rotation on its axis has now been definitely proved equal to its period of revolution round the sun, and that observation is therefore restricted to the one illuminated hemisphere, increased by the large libration in longitude of 23°·7 on either side of the mean position. This libration is caused by the marked eccentricity of the planet's orbit. A map of the surface is given, divided into light and darker areas to which suitable names have been assigned, and whose boundaries are claimed to be substantially stable, while brighter spots are also visible. The albedo of Mercury is very nearly the same as that of the moon, and indicates for both bodies a surface covered with lava, basalt or volcanic ash. This has been strikingly confirmed by polarisation observations conducted by M. Lyot at Pic du Midi. Antoniadi has observed on Mercury certain phenomena which he ascribes to clouds of fine dust. These clouds are much more frequent and more opaque than the clouds of Mars; they seldom occur in the central regions of the disc, often show rapid changes in form and sometimes obscure wide regions for several days. It is suggested that Mercury possesses a highly rarefied atmosphere, too tenuous to be seen during transits or to be detected by the spectroscope, but capable of supporting enormous clouds of exceedingly fine dust, which are blown about by high speed convection currents caused by the powerful radiation of the sun on the illuminated hemisphere.

The Sun.—The lately published *Annual Report of the Director of the Mount Wilson Observatory* is a record of progress in many fields, but the sun naturally claims the first place in the observing programme. The appearance of a new cycle of sunspots during the past year has brought again into prominence a remarkable phenomenon discovered by Hale in 1912—the reversal in the sign of the magnetic field associated with the spots which accompanies their appearance in high latitudes, marking the beginning of the new cycle. But while the average length of the sunspot period is 11·2 years, twice this interval is required for the magnetic field in the spots to reverse its sign and then return again to its original direction. Only one complete magnetic cycle had therefore been observed since the discovery of this phenomenon, and not until its confirmation from the new spot cycle could the period of 22 to 23 years be definitely accepted. It is now clear that in solar theory this magnetic cycle must be regarded as of an importance comparable to that of sunspot activity itself. It is also suggested that the intensity of the sun's ultra-violet radiation may have a con-

nection with the spot cycle. For several years the former showed a decline, while since 1932 it has increased, but it cannot yet be definitely stated that this approximate coincidence with the sunspot minimum has real significance. A new investigation consists of visual measurements with a photoelectric amplifier of the contours and intensities of the solar spectrum lines. By this means information can be gathered regarding the conditions under which each line is formed and the abundance of the element producing it.

The results obtained by Humason regarding the expanding nebulosity emitted by Nova Persei No. 2, 1901, are of special interest in view of the recent observations of Nova Herculis, 1934. In the former star the emission lines of the spectrum were found to be single at the outer edge of the shell of nebulosity but double elsewhere owing to the Doppler effect. It seems that the material in the part of the shell nearest to us is approaching and that furthest away is receding, the relative velocity in the line of sight approximating to 2,500 km. per sec. The Doppler effect is evidently responsible in the spectra of all novæ for the widening of the bright bands and for the displacements of the absorption lines which bound them on the violet edge, and the " expanding shell of gas " theory seems now well established.

A new astronomical publication *Southern Stars* issued monthly by the New Zealand Astronomical Society and edited by I. L. Thomsen was first issued last November. It is evidently to be conducted on the lines of the *Journal of the British Astronomical Association*, and we wish it a career of equal distinction and usefulness.

PHYSICS. By L. F. Bates, Ph.D., D.Sc., F.Inst.P., University College, London.

The Value of *e*.—Physicists have for some time regarded with concern the various suggestions, which have appeared sporadically, that e, the charge on an electron, is not equal to $4{\cdot}770 \times 10^{-10}$ e.s. units, the famous Millikan value. The problem has on several occasions been briefly mentioned in these articles, and it is sufficient now to quote the earlier works of Bäcklin (*Dissertation, Upsala University Årsskrift*, 1928), Bearden (*Phys. Rev.*, **33**, 1088, 1929 and **37**, 1210, 1931) and Cork (*Phys. Rev.*, **35**, 128 and 1456, 1930) which have direct bearing on the subject, and that of Robinson, Andrews and Irons (*Proc. Roy. Soc.*, A, **143**, 48, 1933) which, though less direct, to some extent supports the general conclusions.

Consequently, the appearance of a very clear account of some interesting precision measurements by Bäcklin (*Zeit. für Phys.*, **93**,

450, 1935) must not pass unnoticed. Bäcklin has determined the wavelength of the aluminium $K_{\alpha_{1,2}}$ X-ray line in absolute measure, using a plane reflection grating. A narrow beam of the radiation was allowed to fall at grazing incidence upon the surface of a right-angled glass prism—or, more accurately, a rectangular block of specially polished glass—on which lines had been ruled parallel to, and right up to, one edge of the glass. The beam leaving the grating was strictly limited by a very narrow slit set parallel to the edge just described. A photographic plate was so arranged perpendicularly to the direct beam that the latter, the directly reflected beam and the diffracted beam all produced their several images thereon.

The value of the wavelength was deduced from the well-known equation

$$n \,.\, \lambda = 2 \,.\, d \,.\, \sin\left(\frac{2\phi + \theta}{2}\right) . \sin \theta/2,$$

where ϕ is the angle of grazing incidence, θ is the angle of diffraction and n, λ and d have their usual meanings. In these experiments, however, the angle between the direct ray and the reflected ray, and the angle between the direct ray and the diffracted ray were actually measured, all these rays being assumed to pass through the mid-point of the limiting slit. If the last two angles are respectively ϕ^1 and θ^1, then the above equation becomes

$$n\lambda^1 = 2\ d \,.\, \sin\left(\frac{2\ \phi^1 + \theta^1}{2}\right) . \sin \theta^1/2,$$

where $\lambda = \lambda^1 \left(1 + \frac{2s}{r} . \frac{10800}{\pi(\phi^1 + \theta^1)}\right)$, and where $2s$ is the width of the limiting slit measured in thousandths of a millimetre, r is the distance between the spectrometer slit—*i.e.* the slit through which the X-rays pass before their passage to the glass block and limiting slit—and the latter, and ϕ^1 and θ^1 are expressed in minutes of arc.

The value of $2s$ was found by placing graphite on the cathode of the X-ray tube so that the very soft K_{α} line of carbon provided a beautiful series of diffraction fringes of the limiting slit on the photographic plate. The grating constant d was found by mounting the grating on a precision spectrometer and measuring the copper green line 5153·25 in the first and fifth order spectra. Clearly, great precision could only be obtained by measuring the distance between the limiting slit and the photographic plate with great accuracy; this was done by a special measuring device standardised

by a new metre comparator. The final result obtained for the aluminium $K_{\alpha 1, 2}$ line was $\lambda = 8{\cdot}3395$ Å with a mean error of $\pm{\cdot}0010$Å. With this value of the wavelength, the lattice constant of calcspar was found to be 3·0356 Å, which gives a value $6{\cdot}02 \times 10^{23}$ for Avogadro's constant, and $4{\cdot}805 \times 10^{-10}$ e.s. units for e—these two constants presumably having a mean error of about 0·075 per cent. It might be thought then that the problem is settled.

Unfortunately, these neat experiments have not extricated us from our difficulties, for a new determination of the electronic charge by E. Schopper (*Zeit. für Phys*, **93**, 1, 1935), carried out in Regener's laboratory with an improved form of the early Rutherford and Geiger method, has given the value $4{\cdot}768 \pm 0{\cdot}005 \times 10^{-10}$ e.s. units, with a probable error stated to be 0·1 per cent. In this determination the charge carried by a known number of α-particles was measured. Now, statistical fluctuations could only be avoided in such experiments by counting at least one million particles, which was done in this instance by means of a Geiger point counter in combination with an amplifier and counter of the Wynn-Williams type. The charge was satisfactorily measured by a single-string electrometer of the type used by Regener in his cosmic ray investigations.

An idea of the measurements may perhaps best be gained by supposing that a limited stream of α-rays from a polonium deposit on platinum foil passes across an evacuated chamber, to be received by a Faraday cylinder connected to an electrometer. The majority of the α-particles are captured by the receiver, but some small fraction is permitted to pass through a hole in the receiver. The latter particles then continue on their way across the chamber to fall upon a very thin mica window which covers a gas-filled counter. The number able to enter the counter is further limited by a diaphragm placed before it.

The accuracy of the measurements thus depend upon the exactness with which the fraction passing through the receiver can be estimated; the probable error is stated to be about 0·02 per cent. For finding the capacity of the electrometer system a rod which formed the central electrode of a cylindrical condenser was attached. The capacity of the attachment could be increased by known amounts by adding a further length of rod and outer cylinder, the initial and final capacities being compared by a method due to Clay (*Zeit. für Phys.*, **78**, 250, 1932). Corrections had to be made for the small ionisation currents, polarisation of the insulators used in the electrometer, δ-rays and reversal or change of charge of the incident α-rays, the final result being that quoted above.

How then are the new sets of experimental results to be reconciled? Two suggestions may be put forward. First, it is not unreasonable to suppose that the behaviour of the ruled grating in X-ray diffraction is not completely understood. However, it appears impossible to specify in what way the theory can be elaborated, and the second suggestion must be considered. This is contained in a letter to *Nature*, **135**, 825, 1935, contributed by Bond, who suggests that the factor 136/137 is involved. Eddington deduced on theoretical grounds that $h \,.\, c/2\pi e^2$ was equal to 137. Assuming this to be correct, and that the ratio of the mass of the proton to the mass of the electron is 1847·6, and taking the known values of the Faraday and the Rydberg constant, Bond estimates the value of e to be $4{\cdot}775 \pm 0{\cdot}0004 \times 10^{-10}$ e.s. units. He suggests that the crystal-grating X-ray estimate of e is really an estimate of $\frac{137}{136}(4{\cdot}775) \times 10^{-10}$, *i.e.* $4{\cdot}810 \times 10^{-10}$ e.s. units. He shows that the latter value may be obtained by assuming $h\,c/2\pi e^2 = 137$, but using $e/m = 1{\cdot}757 \times 10^7$ in the Rydberg-Bohr equation. These figures are very striking and perhaps make us think that we are nearer a solution of the problem, but to some physicists the introduction of the mystic 137/136 may be equivalent to the statement that the theory of the grating is not properly understood.

THE MASSES OF THE LIGHT ELEMENTS.—A paper likely to appeal very much to all who enjoy playing with mass numbers is that published by Oliphant, Kempton and Rutherford (*Proc. Roy. Soc.*, **150**, 241, 1935) on some nuclear transformations of beryllium and boron and their incidence on the masses of the light elements. It is known that when beryllium is bombarded by protons, α-rays of range about 3 cm. are liberated in small numbers. These are generally attributed to the presence of some impurity which is sufficiently wide-spread to give similar effects with most other substances. In addition, however, it would be expected from a knowledge of the behaviour of lithium and boron, that a nuclear transformation of the type

$$ {}_4\mathrm{Be}^9 + {}_1\mathrm{H}^1 \longrightarrow {}_3\mathrm{Li}^6 + {}_2\mathrm{He}^4 + \mathrm{E}_1 \quad . \quad . \quad . \quad (1) $$

should be possible, resulting in the emission of α-particles of range about 2·4 cm. This range is computed from the available energy E_1 in the above equation, which is calculated from the mass changes as provided by the careful results of Aston and of Bainbridge. Actually, it is found that two groups of particles of almost exactly the same range, 7·4 mm., are present, but one is singly charged and the other doubly charged. Deflections produced during passage through electric and magnetic fields, separate or combined, show that the singly charged particles must be H^2, while the doubly

charged particles are α-particles He^4. The following equation represents the process in which a H^2 particle is emitted

$$ {}_4Be^9 + {}_1H^1 \longrightarrow {}_4Be^8 + {}_1H^2 + E_2 \quad . \quad . \quad . \quad (2) $$

The results obtained by bombarding beryllium with heavy hydrogen are much more complicated, for there are emitted groups of particles of ranges about 1, 1·6, 3, 8, 14 and 26 cm. Records of the oscillograph connected to the detection chamber are compared with the records produced by α-particles from polonium, and show that the 1- and 3-cm. particles are doubly charged, the others singly charged. The numbers of particles in the 1- and 3·0-cm. groups are approximately the same, suggesting that they arise from a single transformation which is represented by

$$ {}_4Be^9 + {}_1H^2 \longrightarrow {}_3Li^7 + {}_2He^4 + E_3 \quad . \quad . \quad . \quad (3) $$

although previous mass data would give a value for E_3 corresponding to a range of 6·2 cm. for the α-particles instead of 3·0 cm.

The groups with ranges of 1·6 cm. and 14 cm. are thought to arise from the bombardment of absorbed heavy hydrogen by the heavy hydrogen ions, according to the equation

$$ {}_1H^2 + {}_1H^2 \longrightarrow {}_1H^3 + {}_1H^1 \quad . \quad . \quad . \quad . \quad (4) $$

The 26 cm. group is only to be explained on the assumption that a new isotope of beryllium Be^{10} is formed as follows

$$ {}_4Be^9 + {}_1H^2 \longrightarrow {}_4Be^{10} + {}_1H^1 + E_4, $$

where E_4 has the value 0·0050 mass units. The 8 cm. group is thought to arise from the transformation

$$ {}_4Be^9 + {}_1H^2 \longrightarrow {}_4Be^8 + {}_1H^3 + E_5 \quad . \quad . \quad . \quad (5) $$

In addition to these groups neutrons are also emitted, some arising from the transformation of absorbed heavy hydrogen and others from the reaction

$$ {}_4Be^9 + {}_1H^2 \longrightarrow {}_5B^{10} + {}_0n^1 + E_6 \quad . \quad . \quad . \quad (6) $$

where the experimental value of the neutron energy may be taken to lie between 4·5 and 10 million electron volts.

Turning now to the experiments with boron, it is found that the main transformation produced by proton bombardment may be represented by

$$ {}_5B^{11} + {}_1H^1 \longrightarrow 3\,{}_2He^4 + E_7 \quad . \quad . \quad . \quad . \quad (7) $$

If Aston's values of the masses are used in this equation the calculated energy is 11·4 million electron volts, whereas experiment gives about 3 million as the energy of each α-particle, *i.e.* a total of 9 million for the whole process. However, Kirchner (*Phys. Zeit.*, **34**, 897, 1933) found a new group of α-particles of range 4·5 cm.,

whose existence has now been confirmed at Cambridge. They form about 1·4 per cent. of the total number of α-particles, and it is suggested that the following equation represents their mode of formation

$$_5Be^{11} + {_1H^1} \longrightarrow {_4Be^8} + {_2He^4} + E_8 \quad . \quad . \quad . \quad (8)$$

but the mass of Be^8 derived from this equation on the basis of Aston's mass data would not agree with the value deduced from equation (2). We see then that in these transformations differences of 2 to 4·5 million electron volts arise, and these lie outside the range of experimental error both in the mass and transformation experiments.

When an explanation of these descrepancies was sought, it was noticed that in Aston's and Bainbridge's work the masses of H^1, H^2, Li^6 and Li^7 were determined directly or indirectly against He^4, whereas those of Be^9, B^{10} and B^{11} were measured by comparison with O^{16}, either directly or through C^{12}, the mass differences being computed in terms of H^1. Consequently, if there exists any error in the standard mass of H^1 used in these comparisons introduced by an error in the original determination of the $He^4 - O^{16}$ mass-ratio, a cumulative error will be brought into all the other masses. Now, this mass ratio was the first to be determined with Aston's new mass spectrograph, and, as a basis of discussion, Oliphant, Kempton and Rutherford therefore assumed that for some reason unknown—possibly because of the polarisation of the electric field—and irrespective of the ordinary distributed errors of measurement—an error denoted by $4x$ was made in the mass of $_2He^4$. A search through the original papers showed how this error would enter into the mass determinations of the other elements. Thus errors of $+ 6x$, $+ 7x$ and $- 5x$ would occur in the cases of $_3Li^6$, $_3Li^7$ and Be^9 respectively, and it was found that if x was taken equal to 0·000314 mass units, corresponding to an error of one part in four thousand in Aston's measurements, then all the above discrepancies could be removed.

If this explanation is accepted, then the work described above means that a new table of mass data for the lighter elements must be put into use, and two new isotopes of beryllium of masses 8 and 10 must be recognised ; on the new scale the masses of these three isotopes are about 8·007, 9·014 and 10·015 units. The experimental evidence so far collected indicates that all three are stable and it may be surmised that the discovery of the 8 and 10 isotopes will speedily result from a re-examination of beryllium from various natural sources.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

The Climate of London.—There is no other region in the British Isles, and but few in other parts of the World, for which such detailed climatological statistics are available in a small area as within the county of London. This fact, together with the magnitude of the population that experiences London's climate, are two reasons why it is worthy of particular study.

The climate of London when considered in its relationship to world climatology is essentially that not only of the British Isles generally but of most of Western Europe, and when the world distribution of this type of climate, as defined, say, by Köppen and Geiger, is examined, it can be seen that apart from Western Europe, it occurs over a very small proportion only of the earth's land surface. The main characteristics, the small range of monthly mean temperature and absence of a dry season, are the characteristics of ocean climates in middle latitudes, and extend so far inland over Europe only because of the strength and persistence of the south-westerly winds that prevail between the nearly permanent area of high pressure near the Azores and the equally persistent low pressure around Iceland. There are vast areas of similar climate over the oceans of both the northern and southern hemispheres.

Having defined it in its broadest aspect, we can consider the different elements separately, and the account can be completed by some comparisons with the climates of other parts of the British Isles.

Precipitation.—Although by far the greater part of the precipitation of London, even on the northern heights, is in the form of rain, the heavy falls of snow that occasionally occur in winter have an important influence on the incidence of spells of exceptionally severe frost, as will be seen in the next section.

There has been a sufficient number of rain gauges for the past fifty or sixty years to enable a comparison to be made of the average annual fall in different parts of London, and this has recently been done by Dr. J. Glasspoole, of the Meteorological Office, London.[1] He finds that in terms of the standard period 1881–1915, largely used in such comparisons in this country, the average annual fall for the County of London is about 23·7 inches. Some of his remarks on the local variations may be quoted. He says: "The average annual rainfall increases fairly steadily over the County of London from just less than 21 in. in the extreme east, near the main river, to rather more than 25 in. in parts of the high land in the extreme

[1] *British Rainfall 1933*, p. 266.

south and just over 26 in. in the extreme north. The range over the whole map is from 20·8 in. at the Northern Outfall Sewage Works, to the east of Beckton, to 27 in. at Bushey Heath in the extreme north-west. The relation of the annual rainfall to the configuration of the land is well marked. The region with less than 22 in. is confined to the neighbourhood of the main river below the Isle of Dogs and to part of the area drained by the river Roding."

The figures in the following table give some idea of the average seasonal variation, as well as an indication of the upper limit for the totals in individual months that may be expected over a period as long as a century.

AVERAGE AND MAXIMUM MONTHLY FALLS AT GREENWICH (1815–1914)

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Maximum Fall (inches) . .	4·35	4·03	4·05	4·35	4·37	6·07	6·75	5·38	5·54	7·65	6·00	6·02
Year . . .	1877	1866	1851	1829	1865	1903	1888	1878	1896	1880	1852	1914
Average . .	1·79	1·53	1·59	1·61	1·90	2·00	2·38	2·30	2·16	2·68	2·30	2·02

The driest year in this series was 1864, with 16·38 in., and the wettest 1903, with 35·54 in., the average being 24·26 in.

The contrast between the normal rainfall of the driest part of the year and that of the wettest is greater than these figures suggest. The figures for the average daily fall suggest that, when "chance" irregularities are smoothed out, the mean daily fall towards the end of October is about two and a half times as great as the mean daily fall late in March. The existence of a maximum in autumn is shared by a large part of the coastal regions of North-West Europe, but not by any means by all parts of the British Isles, for there are on the one hand high level stations that get much of their rainfall from the forced ascent of the westerly and south-westerly winds, and have their maximum in winter when these winds are on an average strongest, and on the other, places, for example in East Anglia, that do not normally receive a great deal of rain when such winds prevail but are more affected by summer thunderstorms, and so have a summer maximum. It is doubtful whether the marked maximum for London late in October can be adequately explained, still less a secondary minimum that appears to be present in September, for a number of factors are probably responsible and the general circulation of the atmosphere may very well be involved.

This account of the precipitation would be incomplete without some statistics relating to the occurrence of snow, hail and thunder.

The figures in the following table are taken from *The Book of Normals* (Section IV*b*) of the Meteorological Office.

FREQUENCY OF DAYS AT KEW ON WHICH SNOW OR HAIL FELL, SNOW WAS LYING IN THE MORNING, THUNDER WAS HEARD, AND OF DAYS WITH ·01 IN. OR MORE OF PRECIPITATION

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
Snow, 1881–1915 .	3·3	3·1	3·4	1·0	0·1	—	—	—	—	—	0·6	1·9
Snow lying, 1912–20	1·0	1·6	0·7	0·2	—	—	—	—	—	—	0·3	0·4
Hail, 1881–1915 .	0·6	0·5	1·2	0·9	0·9	0·4	0·2	0·2	0·1	0·2	0·2	0·3
Thunder, 1881–1915	0·1	0·1	0·7	1·1	2·2	2·3	3·0	3·0	1·1	0·6	0·1	0·1
Days with ·01 in. or more precipitation, 1881–1915 . .	16	13	14	13	12	12	12	13	12	17	16	17

Temperature.—The figures in the following table show the essential features of the average annual march of temperature in London.

MONTHLY AVERAGES OF TEMPERATURE FOR GREENWICH, KEW AND CAMDEN SQUARE (1901–30)

	Jan.	Feb.	Mar.	April.	May.	June	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
Greenwich	40·1	40·3	43·3	47·3	54·7	59·2	63·3	62·5	57·8	51·1	43·2	40·8	50·3
Kew . .	40·4	40·7	43·2	47·1	54·5	59·0	62·8	61·7	57·3	50·9	43·5	41·4	50·2
Camden Square .	40·1	40·5	43·7	48·3	56·3	60·7	64·3	63·2	58·8	51·7	43·7	41·1	51·0

Strictly homogeneous figures are not available for enough places or for long enough periods to make it possible to compare the distribution of mean temperature throughout the county of London, and apart from the fact that on the northern heights the normal decrease with elevation at the rate of about 1° in 300 feet operates to give a mean temperature more than a degree lower than that of places near the level of the river, little can be said about the slight differences that no doubt exist between different parts of London. It is certain, however, that where buildings are most numerous it tends to be warmer both in winter and summer, for in winter the artificial heating of the houses has an effect, and in summer the buildings are more heated by the sun than are the open regions covered with trees or grass, and the air becomes the more heated in that the buildings reduce the ventilating effect of the wind.

Extreme values of temperature in the eighty years 1841–1921 at Greenwich range from 4° F. on January 8, 1841, to 100° F. on August 9, 1911. It appears to be almost a necessity to have the

ground snow-covered if temperature is to fall more than about fifteen to twenty degrees below the freezing-point in London, and in nearly all prolonged spells of severe frost the ground is snow-covered. The reason for this appears to be that so long as there is no snow on the ground heat passes outwards from the ground by conduction when the air is very cold, and the further the temperature falls the bigger is the slope of temperature and consequent transfer of heat, for the ground cools comparatively slowly. There is in this way a powerful check upon excessive lowering of air temperature such as takes place at night when the air is calm and very dry and the sky is clear. Snow, especially if newly fallen, is a very bad conductor of heat, and tends to check this outward flow of heat from the ground. In addition, the whiteness of snow is proof that it largely reflects sunlight, whether direct or diffused, from clouds, and solar radiation falling on the snow-covered ground during the daytime is nearly all reflected back through the atmosphere, whereas with bare ground it would be absorbed and would raise the temperature of the surface of the ground and in this way the air in contact with the ground. The upper extremes of temperature are generally due to the combined action of various causes. It appears to be necessary, in order that temperature shall rise above 90 degrees, to have the air in summer drawn from hotter regions, as for example will happen when southerly winds over the Mediterranean bring air from the Sahara to Southern Europe and when this air drifts north-westwards to England. If, in addition, the sky is practically cloudless and the upper atmosphere very dry, the heat may become almost tropical by the afternoon.

It may be noted that the reading of 100° F. at Greenwich was not equalled in any other part of London, and would probably not quite have been attained at Greenwich had the thermometers there been housed in a screen of the standard pattern. So far as can be judged a temperature of 100° F. represents very nearly the upper extreme that is to be expected in London, but there seems no reason why temperature should not fall below 0° F. under favourable circumstances, as has happened in some other parts of England where the winter is not normally any colder than it is in London.

Sunshine.—The following table shows the average monthly and annual totals of bright sunshine in various parts of London. The measurements are in all cases made with the aid of the Campbell-Stokes sunshine recorder, which focusses the sun's rays by means of a glass sphere on to a card and scorches the card so long as the sunshine is strong enough.

AVERAGE SUNSHINE IN HOURS, 1901–30

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year.
Bunhill Row .	16	34	76	124	183	186	180	170	127	73	25	9	1203
Greenwich .	40	60	102	143	202	202	198	189	151	98	50	31	1466
Regent's Park	28	44	81	128	196	194	191	173	124	77	38	21	1295
Kew . . .	44	61	103	149	203	199	195	183	145	93	53	37	1465
Westminster .	27	45	86	136	194	194	188	178	135	83	34	21	1321

These figures cannot quite be accepted at their face value, for they are affected by various factors which prevent them from being strictly intercomparable, among which must be included the slight loss of record due to obstruction of the sun during certain times of the day at some of the observing stations. One point of difference between the averages is not due to any such causes, namely the superiority of the outer suburbs in winter over places near to the centre of London—an effect due to smoke. It has been shown incidentally that this effect has been smaller in recent years than was the case in the later decades of last century, when sunshine recorders were first used. This seems to be almost certainly due to the growth of London and the diminished use of coal in domestic grates. The smoke pall in the earlier years was denser and was concentrated within a smaller area, and in those years Kew and Greenwich were rural neighbourhoods. Now these last have become urban, and consequently rather more smoky, while the central parts of London are less smoky.

Relative Humidity.—More refined methods of measuring the humidity and an increased number of stations keeping continuous, or at least hourly, records would be necessary for a study to be made of slight local differences of humidity, such for instance as may possibly exist between the regions in the north where the soil is clay, and in the south where it is more often a porous sand or gravel that readily dries up in summer. It seems fairly certain that the regions in which there is practically no bare soil or grass, but only well drained tarred or paved roads and flagged pavement, are drier than the parks or outer suburbs, and the distribution of fogs in recent years supports this view. There is often fog in the morning in the outer suburbs, when the centre of London has none, and in a period of widespread fog there is often more fog in, and near to, the Parks than in other parts of the centre of London. This effect may, however, be partly due to the greater warmth of the more built-up areas.

From the records of the photographic dry and wet bulb thermograph that has been in operation for a long period at Kew, hourly

averages of relative humidity have been computed for the period 1886–1910. The relative humidity, which is defined as the ratio of the actual pressure of water vapour at a given time to the maximum possible at the temperature at that time, has been calculated on tables made out by Glaisher many years ago that have been used in the official publications of the Meteorological Office until 1926. The values so obtained differ little from those given by the more recent official tables. The diurnal variation in each month is sufficiently well shown by the figures for the odd hours 1 h., 3 h., etc., given in the following table:

MEAN RELATIVE HUMIDITY, 1886–1910, AT KEW

Month.	1 h.	3 h.	5 h.	7 h.	9 h.	11 h.	13 h.	15 h.	17 h.	19 h.	21 h.	23 h.	Mean.
Jan. . .	86·6	86·8	86·5	87·0	86·4	82·9	79·7	79·6	82·7	84·6	85·4	86·1	84·7
Feb. . .	84·7	85·2	85·4	85·4	84·0	78·5	74·6	73·7	77·1	81·2	83·2	84·3	81·6
March .	85·4	86·6	86·8	86·4	81·0	73·1	68·4	67·0	69·9	76·8	81·2	84·3	79·1
April. .	84·4	86·1	86·9	83·8	75·3	66·6	62·3	61·0	62·9	69·9	77·4	81·9	75·0
May . .	84·6	86·8	86·8	81·1	71·1	65·0	60·9	59·5	60·7	66·6	75·6	81·1	73·4
June. .	84·3	87·1	85·7	79·7	71·3	64·8	60·4	58·6	59·8	65·5	74·9	80·7	72·9
July . .	85·1	87·2	87·1	80·8	70·7	63·4	59·4	57·8	59·2	65·1	75·9	81·7	72·9
Aug. .	86·8	88·5	89·0	84·9	74·3	65·4	61·0	59·7	61·7	70·4	79·5	84·1	75·6
Sept.. .	88·4	89·6	90·0	88·5	80·0	70·7	65·4	64·1	68·1	78·0	83·4	86·4	79·5
Oct. . .	89·9	90·6	91·1	90·6	85·9	78·2	73·3	72·8	79·1	85·2	87·6	89·1	84·6
Nov.. .	89·2	89·7	89·4	89·6	87·9	83·6	79·4	79·2	84·0	86·3	87·7	88·7	86·4
Dec. . .	87·4	87·6	87·8	87·4	87·1	84·4	81·8	81·9	85·0	86·2	86·8	87·3	86·1
Year. .	86·4	87·7	87·7	85·4	79·6	73·1	68·9	67·9	70·9	76·3	81·6	84·6	79·3

It will be observed that, generally speaking, relative humidity is lowest when temperature is highest and vice versa. Thus in all months the relative humidity is highest in the early morning and lowest in the early afternoon, and it is much lower in June and July than in the winter months. The driest time in the year on an average is at about 15 h. G.M.T. in July and the dampest at about 5 h. G.M.T. in October. As a rule the vapour pressure does not change much in the course of the day, and the large diurnal variation of relative humidity in summer is simply an expression of the large increase in the capacity of the air for water vapour with a rising temperature. In spite of the fact that the air normally contains far more water vapour in summer than in winter, it is in summer that the lowest values of relative humidity are to be found; they occur when a very high temperature is reached as a result of prolonged sunshine in dry weather. At such times the humidity may fall below 20 per cent., as happened on July 10, 1934, when the phenomenally low value of 16 was recorded at Kew.

Changes of Climate.—It is an open question whether the climate of London is undergoing any lasting change other than the slight

rise of mean temperature that is to be expected from the increased area covered by buildings. Severe frosts have been notably scarce since the run of cold winters from 1890 to 1896, but there is evidence that even longer periods of immunity have occurred, one of these occupying the middle part of the last century. It is a curious fact that the mean temperatures of all the districts into which the British Isles are divided for official purposes have been above the average in 1932, 1933 and 1934.

BIOCHEMISTRY. By W. O. Kermack, M.A., D.Sc., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

The Biochemistry of Muscular Exercise.—What actually happens in our muscles when we lift a weight or run a race? The attempt to answer this question has led, during recent years, to much interesting and fruitful research, the object of which is to discover the relations between the actual performance of muscular work and the chemical process of combustion, that is, the oxidation of organic compounds from which the energy is ultimately derived. The animal body, in many ways, resembles an engine, more especially an internal combustion engine, but as compared with the engine of the ordinary motor-car there is one striking difference. The latter consumes oxygen—and fuel—only whilst it is running. When stopped nothing more happens. But it is a matter of common experience that hard muscular exercise, carried on for even a short time, is followed during rest by a period of intense breathing. It thus appears that the process of combustion, which supplies the energy, takes place to some extent after the exercise has been completed. It is as if the motor was provided with an accumulator which supplied the energy or part of the energy for driving it and which, when the car was stopped, was gradually recharged by the engine which would then have to run for some time and consume oxygen. Thus immediately after the exercise is completed the body may be regarded as being in a state of oxygen deficiency and oxygen has to be supplied to bring it back to its normal resting condition. Professor A. V. Hill has called this the phenomenon of oxygen debt and an interesting account of this conception in relation to athletic performances was given in his presidential address to section I of the British Association in 1925 (*Rep. of the B.A.*, 1925, 156).

At that time it was known that the anaerobic contraction of isolated muscles was accompanied by the formation of lactic acid and that lactic acid appeared in the muscles and the blood of the living animal simultaneously with the development of the oxygen

debt. It was therefore generally assumed that the contraction of muscle was accomplished in the first place at the expense of the anaerobic reaction

$$\text{carbohydrate} \longrightarrow \text{lactic acid}$$

and that the excess oxygen subsequently utilised by the body was required to oxidise completely part of the lactic acid ($\frac{1}{5}$ to $\frac{1}{10}$), part of the energy so produced effecting the resynthesis of glycogen from the rest of the lactic acid. But recently it has become clear that the relation between the production of lactic acid and muscular contraction is not so direct as had then been thought. Some of the evidence which leads to this conclusion was discussed in one of these articles (SCIENCE PROGRESS, 1934, **28**, 682). Margeria, Edwards and Dill (*Amer. J. Physiol.*, 1933, **106**, 689) have recently approached the problem from a different point of view and their results are of considerable interest and importance.

These authors have carried out a series of observations on a suitable subject, actually a trained athlete, before, during and after the performance of spells of exercise on a treadmill. The duration of the exercise was usually 10 minutes and the intensity was graded by varying the speed of the mill. From the observations the oxygen debt incurred at the end of the exercise could be calculated and a graph is given in which the blood lactic acid is plotted against the oxygen debt. It becomes clear from this graph that there is little or no increase in blood lactic acid when the oxygen debt is less than 3–4 litres. On the other hand when the oxygen debt exceeds 6 litres the increase of blood lactic acid is proportional to the increase of oxygen debt. Now Margeria, Edwards and Dill produce a considerable volume of evidence which indicates that under the conditions of the experiment no substantial error will be committed by assuming that the blood lactic acid is in equilibrium with that in the muscles and tissues generally, that is to say, that the increase in blood lactic acid is a measure of the lactic acid produced during a spell of exercise. The obvious conclusion is that, during the contraction of the first 3–4 litres of oxygen debt, some mechanism is involved which does not produce lactic acid. On the other hand, once the oxygen debt has exceeded 5–6 litres any increased oxygen debt is incurred entirely through a mechanism which results in the formation of lactic acid, presumably the well-known anaerobic production of lactic acid from carbohydrate. Thus there are two types of debt designated by these authors as the alactacid and the lactacid debts respectively. The first comes into play almost exclusively when small oxygen debts are involved,

but is limited in its capacity to 3–4 litres. The lactacid debt, on the other hand, may, in the case of trained athletes, amount to 12–15 litres. Naturally the exact figures depend on the size of the man as well as on the type of exercise.

Margeria, Edwards and Dill proceed to analyse the rate of repayment of the oxygen debt, that is to say, the rate of consumption of excess oxygen by the resting subject after exercise. It had previously been shown by Hill that the payment of small oxygen debts goes on at a relatively much faster rate than the payment of larger oxygen debts. It now appears that, in the case of debts involving the lactacid as well as the alactacid mechanism, the curve of repayment is a composite one. After the first few minutes it is essentially exponential in type and runs parallel to the disappearance of lactic acid from the blood. There seems little doubt that the two correspond and that this slow repayment of oxygen debt which is not complete for 1–2 hours corresponds to the removal of lactic acid from the body. If this curve of lactacid debt is extrapolated backwards it is found that it only accounts for part of the repayment which takes place immediately after exercise. The residue clearly corresponds to the alactacid debt. It amounts, at the most, to about 3–4 litres and in the case of small total oxygen debts it is the only type involved. Its payment is practically complete in about 3 minutes. Reverting to the previous analogy, one might imagine that a very large condenser was placed in parallel with the accumulator. When a large call is made on the current the condenser would partly discharge itself. But its capacity is limited and so, for more prolonged or larger quantities of current, the accumulator comes into play. Both are replenished by working the motor, but the recharge of the condenser can be done much more quickly than that of the accumulator which can only be recharged at a relatively slow rate.

In coming to the above conclusions Margeria, Edwards and Dill find it necessary to discuss at some length a number of questions, subsidiary to the main issue but interesting and important in themselves. For instance, the calculation of the oxygen debt incurred after exercise is complicated by the fact that the resting metabolism several hours after exercise is still above the pre-exercise level, and it appears preferable to take this and not the latter as the base line. Though this and other disturbances may introduce a certain element of doubt as to the exact magnitudes of the lactacid and alactacid debts it seems fairly certain that the general picture is a true one.

Confirmatory evidence is adduced in a subsequent paper (*Amer. J. Physiol.*, 1935, **108**, 341) which attacks the same problem from

a somewhat different angle. Here an untrained subject performs repeated spells of intense work with a rest of about 6 minutes between each spell. Evidently, on the above theory, the alactacid mechanism should be completely restored during the resting period. The increase of blood lactic acid produced by the period of exercise is plotted against the time of the exercise. A little consideration shows that, according to the theory, this increase should be zero for some time after the beginning of the period but should then rise at a constant rate. As each spell of exercise is carried on to exhaustion the initial period of no rise is always passed so that the observations should fit on to a straight line. Actually they find that this is so, the equation for the rise in lactic acid (Δ L.A.) being

$$\Delta \text{ L.A.} = -69 + 4{\cdot}33\,t.$$

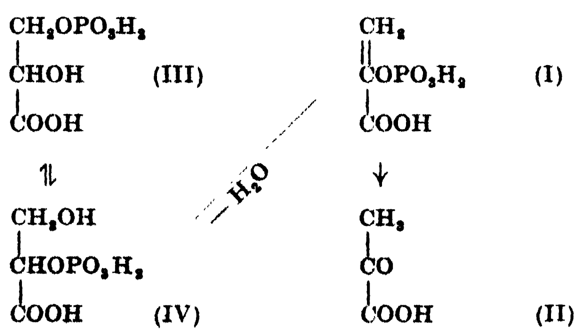
The time $t = 69/4{\cdot}33 = 15$ seconds corresponds to the period during which the alactacid mechanism alone is in action and calculation shows that this leads to an alactacid debt of about 3 litres, in agreement with that arrived at by the analysis of the curves of oxygen uptake.

The question naturally arises as to the nature of the alactacid mechanism. To anyone familiar with the results which have been obtained during the last few years from the study of isolated muscle, for example the results of Clark, Stewart, Eggleton, etc., on the isolated frog's heart briefly described in a previous article (SCIENCE PROGRESS, 1934, **28**, 682), the most obvious suggestion must be that the alactacid mechanism is to be identified with the breakdown of phosphagen into its components, creatine and phosphoric acid. The authors naturally considered this possibility and show that the amount of phosphagen present in the muscles is of the right order to account for the alactacid oxygen debt. The idea is that when the muscles contract, the phosphagen breaks up, liberating thereby sufficient energy to carry out the work of contraction. If the muscle then rests, resynthesis of the phosphagen at once begins at the expense, ultimately, of the oxidation of the ordinary body fuel —carbohydrate, fat or protein. If, however, the exercise is continued, further breakdown of phosphagen occurs and resynthesis proceeds more and more quickly as the concentration of creatine and phosphate increases, until an equilibrium is established in which a considerable proportion of the phosphagen is dissociated. At the cessation of exercise the free creatine and phosphate rapidly recombine and the alactacid oxygen debt, which has been incurred, is paid off.

When we come to inquire as to the exact relations between the phosphagen mechanism on the one hand and the process of car-

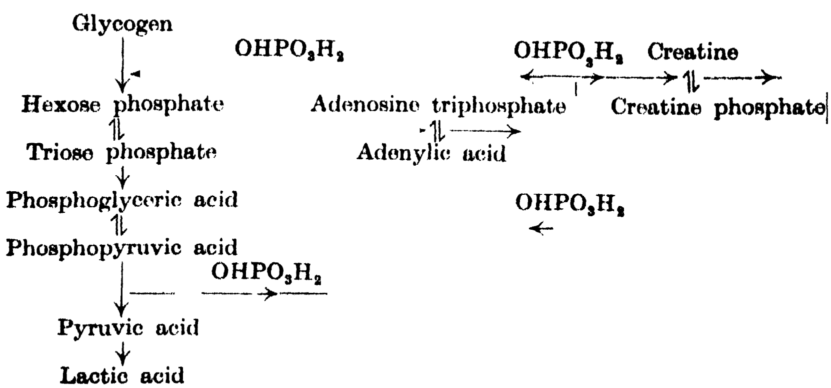
bohydrate breakdown on the other, we find that we are presented with a very complicated jig-saw puzzle. It is all the more complicated because we are not even sure which pieces belong to the one puzzle, but, during the last few months, some very important observations have been made which seem likely to help in the fitting together of the pieces.

The discovery of phospho-pyruvic acid by Meyerhof and Lohmann is of especial significance (*Nature*, 1933, **132**, 337). In a recent article of this series a brief account is given of the scheme put forward by Embden as a description of the chemical stages involved in the breakdown of carbohydrate into lactic acid. A brilliant series of papers by Meyerhof and his school, published since Embden put forward his theory, have completely substantiated the main features of the scheme and have added much to our knowledge of the details of the steps involved. The essential idea is that carbohydrate, after being phosphorylated with the formation of a hexose mono- and di-phosphate and finally of a triose monophosphate, is then converted into phosphoglyceric acid (III) as the result of oxidation. The phosphoglyceric acid is then converted into pyruvic acid and phosphoric acid, the pyruvic acid ultimately acting as the oxidising agent involved in the previous stage and itself being reduced to lactic acid. (It is probable that this mutual oxidation and reduction takes place through the intermediate action of the system $CH_2OH{\cdot}CHOH{\cdot}CH_2OPO_3H_2 \leftrightarrows$ triosephosphate.) Now, at first sight the jump from phosphoglyceric acid (III) to pyruvic acid (II) and phosphoric acid appears rather a big one, but it has been found by Meyerhof and Lohmann (*Biochem. Zeit.*, 1934, **273**, 60) that if phosphoglyceric acid (III) is acted upon by a suitable enzyme in the absence of co-enzyme (that is to say, adenylic acid and magnesium) the product is not phosphoric acid and pyruvic acid but phosphopyruvic acid. This discovery was soon supplemented by another, namely, that in presence of muscle



enzyme the β-phosphoglyceric acid (III) rapidly comes into equilibrium with the α-phosphoglyceric acid (IV). Thus it seems clear that the transition from β-phosphoglyceric acid to the pyruvic acid and phosphoric acid takes place by easy stages through α-phosphoglyceric acid and phosphopyruvic acid.

But the interest in phosphopyruvic acid does not end here. The fact that this compound loses its phosphorus only in presence of adenylic acid brings it at once into close relationship with creatine phosphate, for it has been shown by Lohmann (*Biochem. Zeit.*, 1934, **271**, 264) that adenylic acid also plays a rôle in the dephosphorylation of creatine phosphate. In fact, he comes to the conclusion that creatine phosphate does not break down directly to creatine and phosphoric acid but that it first reacts with adenylic acid to form adenosine triphosphoric acid and that the latter then breaks down to adenylic acid and phosphoric acid (or possibly by interaction with a molecule of glucose to a molecule of hexose phosphate). This idea that phosphopyruvic acid and adenylic acid form a link between carbohydrate breakdown and the synthesis and decomposition of phosphagen has very recently been brilliantly confirmed by Meyerhof and Lohmann (*Naturwissenschaften*, 1935, **23**, 337) who have actually synthesised phosphagen on a considerable scale by allowing phosphopyruvic acid to react with creatine in presence of a suitable muscle enzyme, but only when adenylic acid was present. The facts at present known seem to be consistent with the following scheme which, of course, is merely to be regarded as a working hypothesis.



It will be seen that the phosphagen synthesis appears as a side branch and that the rest of the scheme would remain self-consistent even when this was cut off. Now this is very interesting because

it has long been suspected that carbohydrate breakdown by yeast, for example, is closely analogous to that which takes place in muscle. Yeast contains no creatine phosphate but it does contain an adenylic acid (very similar to muscle adenylic acid). In fact, it looks as if the same scheme in most of its essentials would continue to hold, the main differences being, apart from the phosphagen side-branch, the presence in yeast of a decarboxylating enzyme which removes CO_2 from pyruvic acid before reduction. Thus the ultimate products are CO_2 and ethyl alcohol instead of lactic acid.

The above scheme is, of course, only a condensed outline of the more important features of the Embden-Meyerhof theory. Further details will be found in Meyerhof (*Ann. de l'Inst. Pasteur*, 1934, **53**, 566; *Ergebniss. der Enzymeforschung*, 1935; cf. also Parnas, *Klin. Woch.*, 1935, **14**, 1017).

Attention may however be specially directed to one aspect of the process of glycolysis which has been brought out by the work of the last two years (cf. Meyerhof, *Naturwiss.*, 1935, **23**, 490). This is the very common occurrence in the sequence of reactions of what might be called series of "enzymatic equilibria." An example has already been given. In presence of muscle enzyme α-phosphoglyceric acid is in a real reversible equilibrium with β-phosphoglyceric acid which in turn is likewise in equilibrium with phosphopyruvic acid. A similar series of enzymatic equilibria exist in the early stages of hexose breakdown, for example, between hexose diphosphate and dihydroxyacetone phosphate and between dihydroxyacetone phosphate and glyceric aldehyde phosphate. The stages involving oxidation or reduction, the stages which seem to be inhibited by iodoacetate, have not so far been shown to be of this enzymatic type.

The provisional scheme would indicate that the compound which most nearly connects with the mechanism of muscular contraction is creatine phosphate. An interesting review of recent work dealing with this compound and the closely related arginine phosphate found in invertebrates is given by Eggleton (*Ergebnisse der Enzymforschung*, 1934). Though the breakdown of creatine phosphate takes place in close association with the actual muscular contraction, it does not seem possible at present to say that this reaction is in fact the proximate source of the energy expended by the muscle as external work. Lundsgaard has shown that part of the phosphagen is actually decomposed after and not during the physical contraction of the muscle. This suggests that at least one more chemical stage may exist after creatine phosphate, but it must be remembered that we are still in very great ignorance as to the

details of the molecular structures which are brought into play in the physical process of contraction. The examination of muscle tissue by X-ray analysis which has recently been begun by Astbury will ultimately, we may hope, shed light on this question. Meanwhile the suggestion put forward by Ritchie (*J. Physiol.*, 1933, **78**, 322) should be borne in mind. It refers to the possibility that the natural unstimulated state of the muscle may be a contracted one and that it may be for the process of extension that chemical energy is required. Very crudely put, this theory suggests that the extended muscle is like a stretched elastic string which contracts immediately the constraints on it are removed and that the immediate effect of the chemical processes is to extend the muscle after contraction and keep it extended.

It will, in any case, be realised that the complete picture of all the numerous and interlocking mechanisms, physical and chemical, which enable the animal organism to convert chemical energy into mechanical work—and that with a surprisingly high efficiency—must necessarily in its final form be a highly complicated one. Considerable progress in its investigation has already been achieved. One set of pieces of the jig-saw puzzle seems to have been put together successfully. Other fragments, no doubt, have still to be discovered. Some of the pieces at present known seem to have no relation to the rest. Such, for example, are the considerable quantities of carnosine (β-alanyl histidine) and anserine (methyl carnosine) which together or separately seem to exist in relatively large quantities in muscles (cf. Wright Wilson, *J. Biol. Chem.*, 1933, **100**, Proc. cvi.). So the puzzle is still far from being solved. But this only makes the present situation all the more interesting, and the remarkable advances made during the past few years give ground for hope of equally rapid progress within the near future.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., The University, Glasgow.

Sedimentation and Sedimentary Rocks.—The grain-size of mixed sediments is determined and described by so very different methods and units that comparisons become extraordinarily difficult. Professor P. Niggli, in a paper entitled "Die Charakterisierung der klastischen Sedimente nach der Kornzusammensetzung" (*Schweiz. Min. Petr. Mitt.*, XV, 1935, 31–8), has essayed the task of standardising grain-size determinations with what appears to be considerable success.

Excellent ripple-mark has been detected in a quartzite belonging to the Archæan of the Grand Canyon of the Colorado by J. H.

Maxson and I. Campbell (*Amer. Journ. Sci.*, XXVIII, 1934, 298–303). This is the oldest ripple-mark so far found in North America. It is of the type which is characteristic of the sandy floors of lakes.

According to A. O. Woodward (*Amer. Journ. Sci.*, XXIX, 1935, 518–25) regular rhomboidal ripple-marks of a reticular pattern are primarily the result of wave interference in rapidly flowing shallow water. The direction of the current bisects the acute angle of the rhombs. This type of ripple-mark is chiefly found in intertidal regions and their chances of preservation are therefore slight.

Many of the impressions on sedimentary rock surfaces which have been described by Kindle as " pit and mound " structures, as well as forms described as rain-prints and desiccation-cracks, are regarded by H. Jüngst (*Geol. Rundschau*, XXV, 1934, 312–25) as due to dehydration of colloidal matter in sediments, a process to which he has given the name *synaresis*. Numerous problematical structures in sedimentary rocks are probably effects of synaresis.

The origin of ferruginous concretions in lake sediments is reviewed by W. Ohle (*Geol. Rundschau*, XXV, 1934, 225–47). They are regarded as due mainly to direct bacterial action, although a purely physico-chemical explanation of the process is possible. The ferruginous-calcareous concretions of the Grossen Plöner lake are ascribed by Ohle to root action of littoral plants (*ibid.*, 281–95).

Very large spiral bodies from the Wealden beds near Hastings were described in 1922 by B. B. Woodward as the casts of a gigantic gastropod, *Dinocochlea ingens*. These forms are now interpreted by H. D. Thomas (*Proc. Geol. Assoc.*, XLVI, 1935, 1–17) as spiral concretions. Dr. Thomas describes many similar forms from other formations and localities.

The report by L. H. Matthews on " The Marine Deposits of the Patagonian Continental Shelf " (*Discovery Reports*, IX, 1934, 175–206) describes the sea-floor accumulations from the standpoint of providing data as to the habits of the bottom-living fauna of the region. The characters and distribution of twenty-nine types of deposit, falling into six groups, are described. The probable effects of currents in segregating finer materials towards the north are discussed.

E. Neaverson's report on " The Sea-Floor Deposits. I. General Characters and Distribution " (*Discovery Reports*, IX, 1934, 295–350) records the general characters and distribution of the sea-floor deposits over a wide region in the western Antarctic and south Atlantic Oceans prior to the samples being used for a detailed mineralogical analysis. Flocculent matter which must play a large

part in the transport of terrigenous material to considerable depths, was abundant in the diatomaceous muds.

Another memoir dealing with marine deposits and segregation of certain sedimentary types by current action is O. Pratje's "Die Schlickgebiete der Deutschen Bucht und die Beziehungen zwischen Strömung und Sediment" (*Geol. Rundschau*, XXV, 1934, 145–60).

On December 15–16, 1933, a vast quantity of yellowish dust fell during a snowstorm over the New England area of the United States. This material has been investigated by L. R. Page and R. W. Chapman (*Amer. Journ. Sci.*, XXVIII, 1934, 228–97). Diatoms were abundant in many samples, as well as rounded and polished grains of quartz. Observations of the storm-track and the evidence of the composition of the dust makes it probable that it came from arid regions in the south-western United States.

The fine-grained buff-coloured subsoil in the vicinity of Boston, Mass., is interpreted on field and petrographic data by H. T. U. Smith and H. J. Fraser as a loess deposit (*Amer. Journ. Sci.*, XXX, 1935, 16–32). Its mechanical composition matches that of wind-borne dust and loess. The grains are angular and fresh, and are mostly quartz and feldspar. The deposit is believed to have been laid down before the complete retreat of the last ice sheet from New England.

While G. B. Barbour's paper "Recent Observations on the Loess of North China" (*Geog. Journ.*, LXXXVI, 1935, 54–64) is mainly stratigraphical, numerous petrographical observations are included, especially a series of chemical analyses presented in bar-graph form, and mechanical analyses. He distinguishes three groups of deposits which, in descending order, are: Malan loess (true æolian dust-deposits); banded loessic loams in which darker layers represent interruptions of deposition with soil formation; and red clays referred to the Pontian stage of the Pliocene.

The problem of the formation of loess is also briefly discussed by H. Winter ("Zur Frage der Lössbildung," *Zeitschr. d. Deutsch. Geol. Ges.*, **86**, 1934, 637–44).

In his paper on "Arkose Deposits in the Humid Tropics. A Study of Sedimentation in Southern Mexico," P. D. Krynine (*Amer. Journ. Sci.*, XXIX, 1935, 353–63) shows that arkose deposits of surprising freshness are being formed in a climatic region which, according to accepted ideas, should be characterised by deeply weathered and decomposed sediments. The conditions for this are extremely heavy rainfall in a region of rugged topography, in which erosion is powerful and mechanical disintegration completely overshadows chemical decay.

R. B. Young calls attention to the occurrence of stromatolitic or algal limestones in the Campbell Rand Series of Griqualand West (*Trans. Geol. Soc. S. Afr.*, XXXV, 1933, 29–36).

In his paper on "Conditions of Deposition of the Dolomite Series" the same author (*ibid.*, XXXVI, 1934, 121–35) demonstrates that the rocks of this series can be divided into groups indicative of varying conditions of deposition within a general shallow-water environment. Intraformational conglomerates, oolites, ripple-marks, etc., testify to a shallow-water phase, while stromatolitic and laminated dolomites indicate a deeper-water phase of sedimentation.

On the basis of study of a laterite from French West Africa and an Eocene lacustrine limestone from Bouxwiller (Alsace) J. de Lapparent (*Bull. Serv. Carte géol. d'Alsace et de Lorraine*, 2, fasc. 2, 1935, 99–105) advances some considerations on the origin of pisolithic rocks in general. He distinguishes three classes of ooliths and pisoliths: 1, pseudo-ooliths; 2, the typical ooliths; and 3, a "perlitic" type.

Professor de Lapparent has also recorded the presence of boehmite (AlO_2H) and diaspore crystals in the bauxitic fireclays of North Ayrshire (*C.R. Acad. Sci., Paris*, **199**, 1934, 1629–31).

With the aid of chemical analyses M. S. Krishnan describes the lateritisation of khondalite from several Indian localities (*Rec. Geol. Surv. India*, LXVIII, Pt. 4, 1934, 392–9). Khondalites are crystalline schists consisting of quartz, feldspars, garnet, sillimanite and graphite. On lateritisation the feldspars are kaolinised, the garnets limonitised, but the more resistant sillimanite seems to be replaced at a later stage by colloidal hydrated oxide of alumina, while quartz is gradually eliminated.

Professor X. Stainier, the eminent Belgian authority on coal formation, has written an important memoir ("Matériaux pour l'étude de la formation des gisements houillers," *Bull. Soc. Belge de Géol.*, XLIV, fasc. 2, 1934, 51–274) in which he maintains the theory of allochthonous origin for coal. It is hoped to deal with this work in more detail later.

Dr. Marie C. Stopes's systematisation of nomenclature and classification of coal (*Fuel in Science and Practice*, Jan. 1935, **14**, 4–13) is discussed more fully under the heading of "Notes," p. 331).

The geology and sedimentary petrology of the Hangman Grits of the Quantock Hills are dealt with by A. D. Hallam (*Geol. Mag.*, LXXI, 1934, 433–46). They consist of fine-grained quartzitic sandstones and green slates; red, purple and mottled sandstones with red and green shales and marls; and finally a series of sandstones passing laterally into shales. The material constituting these sedi-

ments is shown to have been derived from a mass of acid igneous or highly metamorphic rocks.

The sandstones and shales of the Culm Series of north-west Devon have been investigated by B. Simpson and A. Stuart (*Geol. Mag.*, LXXI, 1934, 446–58). The restricted composition of the detrital mineral suite, together with the abundance of very small and well-worn grains of zircon, rutile and tourmaline, indicate that the source of the material lay chiefly in an older sedimentary series. The dune-sands of Westward Ho were also studied with a view to determining to what extent the Culm sandstones had contributed to their mineral composition.

The "Petrography of the Blea Wyke Series" of the Yorkshire Coast is described by R. H. Rastall and J. E. Hemingway (*Geol. Mag.*, LXXII, 1935, 125–40), and its stratigraphical implications are elucidated. The heavy mineral assemblage is extremely limited, and exhibits exactly the same type as prevails throughout many hundreds of feet of the succeeding series.

METAMORPHISM AND METAMORPHIC ROCKS.—In his paper on "Die chemische Klassifikation der metamorphen Gesteine" P. Niggli (*Schweiz. Min. Petr. Mitt.*, XIV, 1934, 464–72) more exactly defines and rearranges the twelve chemical groups of metamorphic rocks which were established by Grubenmann, in the light of investigation of the thousands of new analyses now available.

In a valuable paper, "Contribution to the Interpretation of Mineral Facies in Metamorphic Rocks," F. J. Turner (*Amer. Journ. Sci.*, XXIX, 1935, 409–21) draws attention to the desirability of taking into account other factors besides temperature, stress and pressure in defining equilibrium mineral assemblages and metamorphic facies. Certain volatiles such as water and carbon dioxide play an essential part in metamorphic changes; and their function depends to some extent on whether chemical readjustments take place in response to rising or falling temperature which, in turn, is determined by equilibrium conditions in the initial rock prior to metamorphism.

F. J. Turner describes a series of schists with the constant mineral assemblage albite-quartz-epidote-chlorite-sericite-sphene from western Otago, New Zealand, which are believed to have been derived from a greywacke series (*Trans. Roy. Soc. N. Z.*, **64**, 1934, 161–74). They belong to the chlorite zone and are due to metamorphism of the dynamothermal regional type.

In his paper, "Über tektonische Aufschmelzungengesteine und ihre Bedeutung," A. Wurm (*Z. f. Vulk*, XVI, 1935, 98–119) provides a good review of ultramylonites and pseudotachylytes. He deals

with their chemistry, association with tectonic structures, origin, and reviews their occurrences in the known fields. He holds that the temperature necessary for re-melting rocks by frictional heat is from 1000° to 1200° C. In conclusion the significance of melt-mylonites for magma formation and for the mechanism of movement in the earth's crust, are discussed.

A. C. Waters and C. D. Campbell record the presence and discuss the significance of mylonites and ultramylonites associated with the famous San Andreas fault-zone of California (*Amer. Journ. Sci.*, XXIX, 1935, 473–503). An excellent discussion of the nomenclature of cataclastic rocks is given, which includes the opinion that Lapworth's original definition of *mylonite* should be strictly adhered to. The San Andreas fault-zone forms a belt of at least half a mile wide which consists of various types of cataclastic rocks.

In E. Wenk's comprehensive memoir, "Beiträge zur Petrographie und Geologie des Silvrettakristallins" (*Schweiz. Min. Petr. Mitt.*, XIV, 1934, 196–278), the mylonites of the "Alpine-tectonic" facies of the Silvretta Gneiss receive detailed attention. Dr. Wenk includes a full discussion of mylonisation processes.

N. H. Kolderup describes the genesis of West Norwegian occurrences of slates and flagstones (*Bergens Mus. Aarb., Natur. Rekke*, No. 1, 1933, 17 pp.). The "slates" consist of quartzite or mica-quartzite, the "flagstones" of granitic gneiss. They occur at the junctions of overthrust masses of quartzite and gneiss with underlying Cambro-Ordovician mica-schists, the slaty cleavage being attributed to the thrust-movements.

The memoir by Professor H. H. Read on "The Metamorphic Geology of Unst in the Shetland Islands" (*Quart. Journ. Geol. Soc.*, XC, 1934, 637–88) is full of detail which will be of the greatest interest to all students of metamorphic petrology. In Professor Read's words: "Unst consists entirely of rocks that have undergone one or more metamorphisms under markedly different physical controls. The original rocks consisted of acid, intermediate, basic and ultrabasic igneous rocks and arenaceous, argillaceous, calcareous and mixed sediments. The island is broken up into blocks by dislocations of various types. Each block has its own metamorphic history, and in this history the dislocations play their part." Adjacent to the dislocations the rocks have been subjected to epi-metamorphism presumably under low temperature and powerful stress conditions. The phenomena of retrogressive metamorphism are displayed very clearly.

In continuation of his studies of Shetland metamorphic geology Professor H. H. Read writes "On Zoned Associations of Antigorite,

Talc, Actinolite, Chlorite and Biotite in Unst, Shetland Islands" (*Min. Mag.*, XXIII, 1934, 519–40). The original rocks whence these zonal bodies were derived were probably small ultrabasic intrusions. The formation of the zones is ascribed to reaction between the ultrabasic igneous rocks and fluids from acid injections which permeated the country rocks.

A further paper by Professor Read, "On the Segregation of Quartz-Chlorite-Pyrite Masses in Shetland Igneous Rocks during Dislocation-Metamorphism, with a Note on the Occurrence of Boudinage Structure" (*Proc. Liverpool Geol. Soc.*, XVI, 1934, 128–38), concerns a case of metamorphic differentiation in a complex consisting originally of spessartite intrusions into chloritoid-kyanite-schists. Thrusting movements induced retrogressive metamorphism, during which solutions were generated from which chlorite, quartz, pyrite and traces of copper minerals were deposited in fractures.

In an important paper entitled "The Central and South-west Highland Epidiorites: A Study in Progressive Metamorphism," J. D. H. Wiseman (*Quart. Journ. Geol. Soc.*, XC, 1934, 354–417) traces the progressive metamorphism of the epidiorite sills as they are found in regions of low-grade metamorphism, and in the garnet, kyanite and sillimanite zones respectively. The intricate chemical and mineral changes that occur with increasing metamorphism have been studied under the microscope and with the aid of numerous new chemical analyses. A study has also been made of retrogressive changes in the epidiorites as a result of differential movements. This work has made it possible to use the epidiorites, at least tentatively, in the delimitation of metamorphic zones.

H. H. Hess reviews "The Problem of Serpentinization and the Origin of Certain Chrysotile, Asbestos, Talc and Soapstone Deposits" (*Econ. Geol.*, XXVIII, 1933, 634–57), basing his work on the study of numerous Appalachian occurrences. Two distinct and unrelated types of alteration are recognised, firstly serpentinization, and secondly the talc-soapstone type. Serpentinization is found to be an autometamorphic alteration occurring in the final stages of the igneous cycle during which the ultrabasic parent-rocks were intruded; while steatitisation is a later process due to the action of hot dilute aqueous solutions derived in many cases from acid magmas.

The "Peridotites, Serpentines and Soapstones of Northern Sweden" are described by T. du Rietz (*Geol. För. Förh. Stockholm*, **57**, 1935, 133–260). The primary rock-types are dunites with transitions to saxonite. Alteration to soapstone occurs in the more highly metamorphic regions, especially where the peridotites come

into contact with amphibolites. On the other hand serpentinization of the ultrabasic rocks generally increases as less metamorphic areas are approached. The peridotites, etc., which occur as numerous small lens-shaped bodies intercalated conformably with the folding of the enclosing rocks, are of Lower Ordovician age at least.

H. Bader's memoir, "Beiträge zur Kenntnis der Gesteine und Minerallagerstätten des Binnentals" (*Schweiz. Min. Petr. Mitt.*, XIV, 1934, 319–441), deals with many kinds of ortho- and paragneisses, with valuable sections on the Mesozoic ophiolites and the Bündnerschiefer. More than half of the paper is concerned with ore-deposits.

H. Wiesenader shows that East Alpine eclogites are not entirely due to deep regional subsidence, but owe their origin in many cases to purely local conditions of metamorphism (*Min. u. Petr. Mitt.*, **46**, 1934, 174–210).

A. Schüller's memoir on "Prävariskische Glieder der sächsisch-fichtelgebirgischen kristallinen Schiefer, III. Über epizonal verformte Magmatite des westlichen Fichtelgebirges, ihre genetische Ableitung und ihre Weiterbildung in Hornfelsfacies" (*Abh. Math.-Phys. Kl. Sächs. Akad. Wiss.*, XLII, No. III, 1934, pp. 1–60), is prefaced with an essay by Professor K. H. Scheumann on "Zum Vergleich des fichtelgebirgischen und erzgebirgischen Anschnitts" (*ibid.*, pp. i–xii). The memoir is too detailed for satisfactory summary, especially as the author has neglected this very necessary task. Orthogneisses of various types are described, as well as mylonite-gneisses developed in the hornfels-facies. The two final sections are devoted to "Mechanische Verformung und Tektonik" and "Über den Chemismus der Magmatite und die Metamorphe Diffusion."

The course of metamorphism in the Saxon Granulitgebirge, according to H. Seng (*Min. u. Petr. Mitt.*, **45**, 1934, 373–423), may be divided into two phases, a granulitic and a granitic. The former, as shown by crystallisation, magmatic penetration and tectonic processes, may be regarded as gneissification. The granitic phase itself divides into two tectonic acts, both of which are closely associated with reconstruction processes.

Calcareous concretions in the hornfelsed clays (cordierite hornfels) of Tokati, Japan, are described by J. Suzuki (*Journ. Fac. Sci. Hokkaido Imper. Univ.*, Ser. IV, II, No. 4, 1934, 323–38). The concretions are composed of quartz, plagioclase, clinozoisite and diopside, and therefore represent a good example of selective metamorphism. The concretions and the adjacent argillaceous hornfels were recrystallised under isophysical metamorphic conditions, and

consequently their difference in metamorphic grade must depend on the primary chemical differences between the two original rock-types.

Glaucophane, riebeckite, ægirine-augite and albite occur as essential constituents in certain contact-metamorphic schists developed between ultrabasic and basic igneous intrusions and the adjacent crystalline schists of Hokkaido, Japan. These rocks are described by J. Suzuki (*Proc. Imper. Acad. Japan*, IX, 1933, 617–20 ; *Journ. Fac. Sci. Hokkaido Imper. Univ.*, Ser. IV, II, No. 4, 1934, 339–53) who ascribes their origin to soda-rich hydrothermal solutions derived from the igneous masses.

K. Schoklitsch has investigated accidental inclusions in the andesitic basalts and tuffs of the eastern borders of the Alps in Austria ("Pyrometamorphose an Einschlüssen in Eruptiven am Alpen-Ostrand," *Min. u. Petr. Mitt.*, **46**, 1934, 127–52). They consist of various plutonic igneous rocks, schists and sediments which have been converted into hornfels and into rocks of the "sanidinite-facies."

G. Kalb's paper, "Beiträge zur Kenntnis der Auswürflinge im besonderen der Sanidinite des Laacher See-gebietes" (*Min. u. Petr. Mitt.*, **46**, 1934, 20–55), concern other and more familiar examples of pyrometamorphism.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

Ecological.—An interesting series of experiments are reported by B. E. Gilbert and F. R. Pember in which various weed species were grown in culture solutions containing varying amounts of alumina. *Cerastium vulgatum*, *Taraxacum officinale*, *Stellaria media*, *Prunella vulgaris*, and *Poa pratensis* were all found to be very sensitive, the depression in growth being much greater than in culture solutions of equivalent acidity. Eight parts per million reduced the growth of *Prunella* to half and 24 p.p.m. reduced it to under a quarter. All these sensitive species showed a depression of over 20 per cent. with 8 p.p.m. of alumina. A similar depression in the case of *Leontodon autumnale* and *Digitaria humifusa* required 16 to 32 p.p.m., whilst the least sensitive of the species tested, *viz. Agrostis tenuis*, *Agrostis alba* and *D. sanguinalis*, required from 32 to 80 p.p.m. to effect more than 20 per cent. reduction of growth (*Soil Science*, **39**, 425, 1935).

T. M. Sperry reports the results of investigations on the root systems of Prairie plants in Illinois. The root systems of twenty-eight species are described, of which nine have previously been

studied from the Prairies of the west. With the one exception of *Stipa spartea* the root systems were deeper on the western prairies than on those of the east. The maximum depth of penetration of *Andropogon furcatus*, for example, which in Nebraska attains to from 174 cm. to 274 cm., in Illinois reaches only to from 108 to 118 cm. The deepest root-penetration was that of *Silphium laciniatum* which attained to 178 cm., whereas in Nebraska the same species sends its roots to 274–417 cm.

The rainfall is from 30 to 40 inches, of which 60 per cent. falls during the growing season as compared with 28 inches per annum in Nebraska. It would appear that the more abundant soil moisture is the main factor with which the less extensive root growth in Illinois is to be correlated. Visual estimates indicate that the shoot-root ratio is higher in the east than in the west (*Ecology*, XVI, 178, 1935).

To *The Naturalist*, No. 941, Dr. A. Malins Smith contributes data as to the age and rate of growth of Junipers. The average rate of growth is apparently low in bushes on limestone, and in general tends to increase with age, which is attributed to the browsing of animals in the earlier years of life. The average rates observed ranged from ·26 to 4·04 mm. per year. Estimates indicated that the oldest bushes were probably more than 170 years old and less than 250 years. Despite the greater growth rate on drift soils the bush is rare on these and so too are seedlings. A competition factor would seem indicated.

T. Lippmaa, in an account of the forests of the island of Abro (*Acta Instituti et horti. bot. Tartuensis*, IV, F. 1–2, 1935), analyses the vegetation on the basis of partial habitat (associations unistrates). Associations of *Alnus glutinosa* occur where the water table is highest and of *Ulmus montona—Acer platanoides* and *Tilia cordata* where the water table is lowest. In the former where the humus is acid (*p*H., 4–5) *Picea encelsa* is present or may even be dominant over a sandy soil with higher acidity. Humus of low acidity (*p*H., 6–4) is associated with the mixed elm and the alder woods. In the more acid conditions, *Asperula odorata* replaces *Aegopodium podagraria* and in the most acid *Maianthemum bifolium* is a feature of the herbaceous layer. The absence of certain species characteristic of corresponding communities on the Esthonian mainland is regarded as accidental.

An interesting monograph on *Arenaria humifusa* Wahlenberg, a segregate of *A. ciliata*, has just been published in *Bergens Museums Arbok*, No. 1, 1935, by R. Nordhagen. The species is an arctic one of America which also occurs in western Norway and Sweden. It

is regarded as one of the oldest elements in the Scandinavian flora. The author furnishes a map of the distribution of the two sub-species, *norvegica* and *pseudofrigida*, of *Arenaria ciliata*. These border the north Atlantic with *A. norvegica* from Iceland, Scotland and the south of Sweden to Finland and *A. pseudofrigida* from about 65° N. to nearly 80° N.

A recent addition to the British Flora is reported by G. M. Ash and N. Y. Sandwith (*Jour. Bot.*, **73**, 177, 1935). This is the North American *Epilobium adenocaulon* (Hausskn.), which quite probably was introduced into southern England during the Great War. It is now known from damp woods and stream sides in Surrey, west Kent, north Hants and west Sussex, together with putative hybrids with four other species.

Cytology Genetics, etc.—A. Gustafsson discusses the phenomenon of parthenogenesis in the light of work on *Taraxacum*, *Archieracium*, *Erigeron* and *Marsilia* (*Hereditus*, XXI, 1–112, 1935). The parthenogenesis is regarded as consisting of three processes; a decrease or omission of primary pairing; origin of diploid chromosome numbers through restitution nuclei; development of the egg-cell without fusion. The precosity theory is on the evidence available held to be inadmissible. Data respecting the frequency of abortive achenes and percentage germinations are furnished. In apomictic Taraxacum species the percentage of undeveloped achenes was found to range from 0·6 per cent. to 12·4 per cent.; the average for ten "species" being 5·4 per cent. The germinations obtained ranged from 63 per cent. to 97 per cent., with an average of 86. Sexual biotypes of Hieracium also show high germination values whereas the apomictic types yielded low germinations of from 6 to 65 per cent. with an average of about 39 per cent. A table of chromosome numbers in apomictic genera shows clearly that these tend in general to be higher in the apomictic than in the sexually functional types, but both in *Erigeron* and *Rubus* the reverse is true, so that the author concludes that apomixis is caused by several genes, perhaps by purely recessive factors.

The occurrence of diploid gametophytes in *Sphærocarpus Donnellii* is described by C. E. Allen (*Amer. Jour. Bot.*, **22**, 664, 1935). The plants are normally female in appearance and can function as such. They most frequently arise from dyad spores but may result from partial abortion. Whilst functionally female, evidence of the intersexual character of the diploid gametophytes was furnished.

The occurrence of gynodiœcious flowers in *Ranunculus acris* is recorded by E. Soderberg (*Svensk. Bot. Tid.*, **28**, 469, 1935).

The chromosome structure during meiosis of *Trillium erectum* is described by Huskins and Smith (*Ann. Bot.*, XLIX, 119–50, 1935). In this species there are five pairs of very large distinctive chromosomes and it has been possible to estimate the total number of chromomeres as between 900 and 1,000. No splitting of the chromomeres is to be observed prior to pairing. Spiralling of the chromatids begins at diakinesis and paired chromosomes commonly coil in opposite directions, changes in direction are usually associated with chiasmata. The evidence of Trillium indicates that crossing-over occurs at the time of visible splitting of the chromosomes.

ANATOMY.—The occurrence of an anomalous type of secondary thickening in the diarch root of *Spergalaria rubra* is recorded by P. C. Joshi (*Proc. Indian Acad. Sci.*, B. I, 729, 1935). The growth of the stem-cambium is normal and so is that of the first-formed cambium in the root, but later the pericyclic tissue and adjacent phloem-parenchyma become meristematic and give rise to arcs of cambium forming from two to three, more or less discontinuous and concentric rings. The secondary cambia function in the normal manner. It is suggested that the root has retained a character that was originally found in both root and stem.

The nature of the endodermis in a number of phanerogamic stems has been studied by S. Rojkowski (*Acta. Soc. Bot. Poloniæ*, XI, 19–50, 1935). An endodermis in which the Casparian strip is abnormally wide is recorded in the stems of *Comarum palustre*, *Calluna vulgaris*, *Mentha piperita*, *Rubia tinctorium*, *Galium boreale*, *G. mollugo*, *G. sylvaticum*, *Knautia arvensis* and *Campanula trachelium*.

PLANT PHYSIOLOGY.

By PROFESSOR WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

GROWTH HORMONES.—As is now well known, about two years ago Kögl and his co-workers succeeded in isolating, purifying and determining the chemical composition of the so-called growth substances, growth regulators or growth hormones, which play a decisive part in the stretching phase of growth of plant cells. They identified three such substances, *a*-auxin with a formula $C_{18}H_{32}O_5$, *b*-auxin $C_{18}H_{30}O_4$ and hetero-auxin $C_{10}H_9O_2N$, that is, β-indolyl-acetic acid. While all these substances behave similarly in their influence on growth, *a*-auxin appears to be the substance active in maize coleoptiles, while hetero-auxin is the substance constituting the growth hormone in yeast and some other Fungi.

Since the publication of Kögl's work, further advances have been made in our knowledge of the action of growth substances.

In a paper by K. V. Thimann and F. Skoog (" On the inhibition of bud development and other functions of growth substance in *Vicia faba*," *Proc. Roy. Soc.*, B, **114**, 317–39, 1934) the question of the relationship of growth substance to the development of lateral buds is considered. By decapitating terminal buds of *Vicia faba* plants and placing the buds on agar blocks, it was shown that growth substance diffuses out of the buds into the block and the amount of substance diffusing out was approximately determined. Similarly, it was found that lateral buds produce no growth substance until active growth is proceeding, when they produce it in considerable amount ; at the same time the growth of an adjacent bud is greatly retarded. After the unfolding of the bud the leaves continue to produce growth substance, the amount decreasing as the leaves grow older. If growth substance in an agar block is applied to the apex of decapitated plants the development of lateral buds is retarded, the degree of inhibition depending on the concentration of the growth substance in the agar block. It was also established that growth substance brings about elongation of the stem of plants of *Vicia faba*. Less of the substance is required for stem elongation than for inhibition of lateral buds. While it was found that the production of growth substance takes place only in light, the actual effect of the substance on stem elongation was greater in the dark than in the light. Thimann and Skoog put forward the theory that the growth substance not only promotes cell elongation, but inhibits the development of buds, the inhibition of lateral bud development being due to the diffusion of growth substance from the terminal bud.

K. V. Thimann has also devised a method for extracting the growth substance from plant tissues, and, using the method, has examined the distribution of the growth substance in the coleoptile and radicle of *Avena* seedlings (" Studies on the Growth Hormone of Plants. VI. The Distribution of the Growth Substance in Plant Tissues," *Journ. Gen. Physiol.*, **18**, 23–34, 1934). As regards the method of extraction, it was found that when plant tissues are ground up in water the growth substance is inactivated owing to the action of oxidising enzymes. Therefore to extract the growth substance Thimann first killed the tissue by immersing it in chloroform, and then added about one-fifth of its volume of normal hydrochloric acid. The tissue was then thoroughly ground in this mixture, the chloroform separated off and the acidified tissue ground twice more with chloroform. This procedure has the effect of inhibiting the oxidase action but does not destroy the growth substance. After evaporation of the chloroform the lipoidal material

containing the growth substance is taken up in water. The yield of growth substance by this chloroform extraction method was, according to the material, 1·2 to 25 times that obtained by simple extraction in water.

Applying this method of extraction to the oat seedling Thimann found that although the growth substance does not diffuse out from the lower part of the coleoptile it is present in this region in quite appreciable quantity. Nevertheless, the concentration does decrease regularly from the tip to the base, the concentration being 0·69 units at the apex and 0·19 at the base. The same is the case with the root, the concentration falling from 0·43 at the root apex to 0·26 in the physically uppermost region. It is important to note that the amount of growth substance which diffuses out of root tips into agar is not greater than that obtained by extraction. Hence it would seem that formation of growth substance does not take place in the root tip, or if it does, the conditions for its formation must be quite different from those for its production in the coleoptile apex.

The usual method of determining auxin quantitatively consists in measuring the curvature produced by a decapitated oat coleoptile when the auxin is applied excentrically to the cut surface. F. W. Went, who was responsible for the method, has now described a simpler test which is "semi-quantitative," and which he calls the "pea test method" ("On the Pea Test Method for Auxin, the Plant Growth Hormone," *Kon. Akad. van Wetensch. Amsterdam, Proceedings*, **37**, 547–55, 1934). For this test pieces of stem of etiolated seedlings of *Pisum sativum* are used. The uppermost 5 mm. of stem is removed and pieces 2 to 20 cm. long are employed either immediately after cutting or from 4 to 8 hours later. The top of each piece of stem is split longitudinally for 1 to 3 cm. If such a piece of stem is then placed in an aqueous solution the two halves bend outwards. If auxin is present, after about an hour at 25° C. the free ends of the two halves begin to curve inwards, curving further the more concentrated the auxin, the final state being reached in about 6 hours. By using a range of concentrations of the solution to be tested and finding the lowest concentration in which a recognisable reaction just occurs, the test can be made quantitative; the limiting concentration corresponds with 6 units (Dolk and Thimann) of growth substance per c.c., or 480 Avena units per c.c., or $1{\cdot}4 \times 10^{-5}$ mg. *a*-auxin per c.c.

The effect of ethylene on the production of growth substance in the oat and broad bean has formed the subject of an investigation by P. A. Van der Laan ("Der Einfluss von Aethylen auf

die Wuchsstoffbildung bei Avena und Vicia," *Rec. Trav. bot. néerlandais*, **31**, 691–742, 1934). It was shown that ethylene affects the growth substance in both these plants. In *Avena* exposure to this gas lessens the stretching phase of growth, an effect which is traced to an inhibiting effect on the production of growth substance. On the other hand, neither the transport of the growth substance, nor the utilisation of the latter, are affected by ethylene. In *Vicia faba* also the growth of all zones of the seedling is strongly depressed by ethylene, an effect again traceable to inhibition of growth substance production. But ethylene has no effect on a solution of auxin exposed directly to it and Van der Laan considers that his results can be explained by supposing that the production of growth substance in the cell depends on an enzyme action which is inhibited by a small quantity of ethylene.

Some experiments on the transport of the growth substance through the *Avena* coleoptile have been recorded by H. G. Van der Weij (" Der Mechanismus des Wuchsstofftransportes II," *Rec. Trav. bot. néerlandais*, **31**, 810–57, 1934). Agar plates containing known quantities of auxin were placed in contact with the upper and lower surfaces of small cylinders of the coleoptile 1 mm. long and the quantity of auxin in the plates determined after the lapse of some time. In all cases the auxin travelled downwards, and this was so even when the initial concentration of auxin in the lower plate was three times that in the upper. The transport of the growth substance is thus completely independent of any concentration gradient, but exhibits polarity inasmuch as the transport is always in the downward direction.

To determine how far this polarity is bound up with the life of the cell, Van der Weij examined the effect of ether on the transport of the growth substance. He found that the transport when the coleoptiles had been subjected to treatment with ether could take place in either direction, and appeared to resemble a pure diffusion phenomenon. He concluded, however, that transport of the substance under complete " ether narcosis " took place in water present *outside* the coleoptile cylinders, which before use, had been kept on damp filter paper.

Two papers dealing with the root-forming hormone may be mentioned. In the first of these F. W. Went describes a method for determining this substance, rhizocaline, quantitatively (" A Test Method for rhizocaline, the Root-forming Substance," *Kon. Akad. Wetensch. Amsterdam, Proceedings*, **37**, 445–55, 1934). Seedlings of *Pisum sativum* are grown under controlled conditions and a week after sowing, when the plants have reached a height of about 10 to 15

cm. above the ground, the shoots are cut off at about 1 to 2 mm. above the first node. At the third node the first normal leaf appears and the tip is decapitated just under this third node. A length of stem is thus obtained comprising most of the second and third internodes. The base of this piece of stem is kept in water for 4 hours and then in a 0·05 per cent. solution of potassium permanganate for another 4 hours for the purpose of sterilisation. After rinsing the basal end free of potassium permanganate, the apical end is slit longitudinally for about 1 cm. and this slit end then placed for about 12 to 15 hours under standard conditions in the solution containing rhizocaline. The tip is then rinsed in water and the base of the piece of stem placed in 2 per cent. sucrose solution for 6 days and then in tap water under standard conditions. The number of roots produced at the base of the stem are counted 14 days after treatment with the rhizocaline solution. The rhizocaline unit (RU) is the quantity of the substance which produces one root (in excess of those produced in an untreated control) when tested in the manner described. Within limits the number of roots formed is proportional to the concentration of the solution.

Further information regarding the nature of the root-forming hormone is given by K. V. Thimann and F. W. Went (" On the Chemical Nature of the Root-forming Hormone," *Kon. Akad. Wetensch. Amsterdam, Proceedings*, **37**, 456–9, 1934). They find this substance is fairly widely distributed in nature, having obtained it from rice polishings, urine, wheat embryos, the pollen of a number of species, leaves of *Helianthus annuus*, *Prunus laurocerasus* and *Malva sp.*, and etiolated buds and shoots of *Pisum sativum*. From various chemical tests it is concluded that rhizocaline is an unsaturated organic acid of about the same acid strength and solubility as α-auxin and hetero-auxin, and they conclude that the two hormones closely resemble one another, but that they are not necessarily identical, especially as the ratio of rhizocaline to growth substance as determined by the methods devised by F. W. Went varies greatly for different plant materials.

ZOOLOGY.

By G. R. de Beer, M.A., D.Sc.; E. B. Ford, M.A., B.Sc.; J. A. Moy-Thomas, B.A.; and J. Z. Young, M.A.; The University, Oxford.

General and Experimental.—Zehnder (*Acta Zool.*, **15**, 1934, 261–408, 25 plates) has given an exhaustive account of the processes of egg-laying, copulation, cleavage, and development of the external features of *Astacus fluviatilis* (Rond L.) and *A. torrentium* (Schrank).

The beautiful photographs with which this work is illustrated will serve as standard Normentafel of great value.

In a series of papers on the light-sensitivity of the lamprey, Young (*J. Exp. Biol.*, **12**, 1935, 229–38; 254–70) has demonstrated the existence of a region of marked photo-sensitivity in the tail. The impulses initiated by the light stimulus are conveyed not by the spinal nerves and the cord, but by the lateral line nerve. At the same time, the brain and spinal cord are sensitive to direct stimulation by light, when the pigment surrounding them is removed.

The biological significance of the light-sensitive area of the tail is to initiate swimming movements (regardless of the direction of incidence of the light), and thus to prevent the animal from remaining in an illuminated area. The burrowing movement of the larva is simply modified swimming. The normal diurnal rhythm of colour change is under the control of the posterior pituitary and pineal, impulses set up in the latter by light-stimulation controlling the secretion from the former.

Damas (*Arch. de Biol.*, **46**, 1935, 171–227, 2 plates) has studied the structural changes in the head of the lamprey accompanying metamorphosis. As regards the skeleton, he confirms the important findings of Tretjakoff to the effect that the mucocartilaginous elements of the larva do *not* become converted into the definitive cartilage of the adult, which is formed *de novo*.

Results of remarkable interest have been obtained by Greenwood and Blythe (*Proc. Roy. Soc.*, B., **118**, 1935, 122–32) from experiments on the response of the plumage of brown leghorn Capons to localised small injections of œstrone. Three rows of feathers on the breast were plucked, and a small dose of œstrone injected intradermally into the line of the middle row. The characteristic effect (the substitution of a salmon-coloured lipochrome for the melanin in that portion of the regenerating feather which is growing during the time that the injected substance is active) is obtained symmetrically in the regenerated feathers of the middle plucked row, the intensity decreasing with increasing distance from the site of injection. But in the neighbouring rows, the effect in the majority of cases is obtained with greater intensity on that side of the feather which is nearest to the middle row.

The production of such asymmetric feathers is in itself a matter of great interest. Particularly is it difficult to understand how a local diffusion-effect can result from an intradermal injection, and why the injected substance is not generally distributed through the body-fluids. It is further to be noticed that these asymmetrically coloured feathers are straight, which constitutes an objection to

Lillie and Juhn's contention that susceptibility to hormone is correlated with growth-intensity: an objection first pointed out by Espinasse (*Nature*, **133**, 1934, 330).

Fell and Landauer (*Proc. Roy. Soc.*, B, **118**, 1935, 133–54, 2 plates) have applied tissue-culture methods to the study of the problem of the deficient ossification of the long bones in the creeper fowl. It is found that culture of limb-buds in media containing growth-restricting substances leads to retardation of cartilage differentiation, practical absence of cartilage hypertrophy, and absence of perichondral ossification. Ossification of cartilage-bones does not occur in the absence of chondroblastic hypertrophy: on the other hand, membrane-bone ossification is not affected by the conditions of the experiments. It follows therefore that osteoblastic activity *in cartilage* is in some way dependent on the presence of hypertrophic cartilage-cells, and the abnormalities of the creeper fowl can be explained as a result of a non-specific growth-retarding effect at a particular critical period during the development of the limb-buds. This is all the more interesting since it is known that the creeper condition is under the control of a dominant Mendelian gene.

Hörstadius (*Pubb. Staz. Zool.*, Napoli., **14**, 1935, 251–429) has given a detailed account of his experiments on sea-urchin larvæ. By means of the special technique which he has perfected, enabling him to isolate certain specified groups of blastomeres, and to transplant micromeres into any desired position, he has been able to demonstrate the existence of a gradient of potencies distributed along the egg-axis of the sea-urchin egg. The material of the vegetative pole (normally represented by the micromeres) possesses the power of an organiser, and is capable of inducing the formation of an enteron and a larval skeleton.

Some very interesting results have been obtained by Holtfreter (*Sitzber. Ges. Morph. u. Physiol.*, München, **44**, 1935), who grafted pieces of presumptive epidermis from the Anuran *Limnodynastes* into the corresponding region of embryo of the Urodele *Triton*. The grafts, easily recognisable by their histological Anuran characteristics, differentiated in harmony with their surroundings, forming part of the brain, eye, nose, etc. But although the Anuran tissue follows the qualitative determination imposed on it by the Urodele host, it preserves certain Anuran characteristics, in respect of precocious formation of neural crest, etc., and also gives rise to such typical Anuran structures as a ventral sucker and horny teeth, normally not possessed by Urodeles at all.

From the morphological point of view, a result of extreme importance is the fact that the cartilage of the pterygoquadrate

and of the trabecula is formed of cells derived from the Anuran graft, which itself was presumptive epidermis. This goes far towards conclusive proof of the derivation of the visceral cartilages from the neural crest.

Pasteels (*Bull. Sci. Acad. Roy. Belg.*, **21**, 1935, 88–103) has applied the technique of intra vitam staining to the problem of gastrulation in Chelonia. The endoderm is formed by precocious proliferation from the primitive plate, and the process of gastrulation and formation of the open blastopore results in the invagination of chorda-mesoderm, accompanied by movements on the surface of the blastoderm analogous to those found in Amphibia. But the "archenteron" which opens at the blastopore is at first lined entirely by mesoderm, except for the mid-dorsal chorda, and it is only as a result of a longitudinal split in its floor, and migration forwards and laterally of this ventral mesoderm, that the endoderm forms part of the archenteric wall. Just as in Amphibia, the primary gut-roof of chorda-mesoderm is then excluded from participation in roofing the archenteron by the upgrowth and fusion of the free edges of the endoderm, to form the definitive gut-roof.

The process of gastrulation and the demarkation of the various presumptive organ-regions by means of intra vitam stains has been studied by Weissenberg (*Anat. Anz.*, **79**, 1934, 177–199) in the lamprey with highly interesting results.

Neurology.—During the past year there has been further progress in our knowledge of the physical and chemical structure of nerves. The old concept of a nerve fibre as a fixed structure containing rigid neurofibrils is being abandoned and the axon is beginning to be considered as consisting of a more or less fluid substance capable of considerable movement. Thus Speidel (*J. Comp. Neur.*, **61**, 1934, 1, etc.) has observed flowing movements within living axons, and Parker and Paine (*Am. J. Anat.*, **54**, 1934, 1) in the course of a study of degeneration of the lateral line nerve of catfish stress "the capacity of the axis cylinder, and especially its wall, to move its contents by a kind of peristalsis." Admittedly our knowledge of the transport of substances in an axon is fragmentary, but Parker believes that there may be a flow of substance emanating from the nucleated part of the neuron and passing out through its processes. Wallerian degeneration of the severed part of an axon would therefore be the result of interruption of this flow. This concept is speculative, but it is perhaps supported by the observation that the substance of the axons of the giant nerve fibres of Cephalopods is semi-fluid, so that it pours out from the cut end of a nerve (Young, *J. Physiol.*, **83**, 1934, 27P).

Further evidence that nerve cells may undergo rapid internal movements may be found in the observations of Ingersoll (*J. Comp. Neur.*, **59**, 1934, 267) that the size of the nucleus and cytoplasm of the cells of the cœliac plexus decreases after stimulation. Péterfi and Williams (*Arch. f. exp. Zellforsch.*, **16**, 1934, 241) also observed that when the cells in explants of nervous tissue were stimulated by means of micro-electrodes the cytoplasm became more granular and the axon and end-bulb increased in size.

Speidel (see above) has very greatly added to our knowledge of the growth of nerves by his studies of living neurons in the tails of tadpoles during development and regeneration. He has watched growth cones advancing by a slow, irregular flowing motion, spinning the nerve fibres behind them. Schwann cells have been seen migrating out from the spinal cord and proliferating by mitosis as they go. The observation that leucocytes may move along between the neurilemma and the myelin, but not between the latter and the axon, seems to show that the fatty layer belongs to the axis cylinder rather than to the sheath cell, though the presence of the latter is essential for its development. Anastomoses, apparently involving complete fusion of separate axons, may occur where two fibres cross, or between the adjacent fibres of a recently sectioned stump, but growth cones do not anastomose. After section of unmyelinated fibres the two stumps may heal by direct re-union (" first intention ") provided that the fibres of the peripheral stump are still in connection with cell bodies by more distal anastomoses.

As regards the directive influences controlling the course of outgrowth, Speidel finds that axons very often grow along the processes of fibroblasts, and he lays emphasis on orientation by mechanical and structural factors, while not denying the possibility of chemical, electrical or radiative influences. These are however excluded by the observations of Weiss (*J. Exp. Zool.*, **68**, 1934, 393) who investigated the factors controlling the direction of growth of axons in tissue cultures. In no case did chemical factors, such as tissue extracts, produce any orientation of the growing fibres, nor could he confirm the much-quoted experiments of Ingvar on the directive effects of an electric current or the electro-magnetic field around a conductor. However a very powerful orientating influence was found to be the stretching or stroking of the culture, which produces an " ultrastructural pattern " in the ground-substance. The nerve fibres grow along any such lines of stress in the medium, and various features of normal nerve-development can be paralleled artificially by the use of this knowledge. For instance, if two spinal ganglia are cultured together the fibres grow out in a line connecting the

two, this being due to the stresses set up by the abstraction of water from the medium around each mass of tissue. Karssen and Sager (*Arch. f. exp. Zellforsch.*, **16**, 1934, 255) were also unable to confirm the observations of Ingvar on the growth of neurons in electric fields.

The studies of Coghill on the parallel development of neural and behaviour patterns have been extended by the work of Orr and Windle (*J. Comp. Neur.*, **60**, 1934, 271) on birds. The first movements of a chick embryo are unilateral trunk flexions, occurring on the fifth day, at a time when the motor units are still quite independent of each other. The appearance of bilateral movements follows when the ventral longitudinal pathway makes connection with the motor-neurons by means of collaterals. The first reflex responses coincide with the growth of fibres from the dorsal funiculi (sensory pathway) to connect with the motor cells. The development of the behaviour pattern of birds has therefore marked similarities with that of Amphibia. On the other hand in cats Windle (*J. Comp. Neur.*, **59**, 1934, 487) finds that the first behaviour patterns consist of local movements of the fore-limbs, and there is no evidence of a gradually expanding total pattern of behaviour, completely integrated from the beginning.

A paper by Wilkinson (*J. Comp. Neur.*, **59**, 1934, 221) deals with the debated question of the innervation of striated muscles. He was unable to find the periterminal network which is alleged by Boeke to connect the neurofibrils with the substance of the muscle fibre. Moreover, after section of the somatic nerves to a muscle, leaving the sympathetic supply intact, he failed to find any sympathetic fibres running directly to the striped muscle fibres ; the existence of an autonomic innervation of striped muscle therefore remains doubtful.

There is doubt also about another long-standing neurological controversy, namely the existence of efferent fibres in dorsal roots. Hinsey (*J. Comp. Neur.*, **59**, 1934, 117) sectioned the lumbo-sacral dorsal roots of cats and failed to find any evidence of the degeneration or regeneration of efferent fibres. Barron and Matthews (*J. of Physiol.*, **83**, 1934, 5P) provide a possible explanation for many of the conflicting views on this question. They claim that the dorsal root fibres entering the cord send off collaterals which run longitudinally for some distance, and then leave the cord again by a rootlet different from that by which they entered, sometimes by a rootlet belonging even to a different segmental root. These re-emergent fibres do not, of course, degenerate in the central stump after section between a ganglion and the cord. They are held to constitute

30–50 per cent. of all the fibres in each root, and there may be, in addition, a small number of true efferent fibres with cell bodies lying within the cord. This interesting suggestion requires confirmation ; one difficulty is evidence such as that of Davenport and Bothe (*J. Comp. Neur.*, **59**, 1934, 167) that the number of fibres in the dorsal root is equal to the number of nerve cells in the ganglion.

Much interest has centred recently around the question of possible humoral mechanisms for transmission across synapses, or between nerves and their effector organs. Feldberg and Gaddum (*J. Physiol.*, **81**, 1934, 305) showed that if the cervical sympathetic trunk be stimulated during perfusion of the superior cervical sympathetic ganglion, then acetyl choline appears in the efflux, and after careful analysis of this phenomenon they come to the conclusion that the acetyl choline works as a synaptic transmitter. Eccles (*J. Physiol.*, **81**, 1934, 8P, etc.) however considers that it is not yet sufficiently demonstrated that the acetyl choline actually mediates the stimulation of the next cell in the chain and we are clearly still some way from a full understanding of the mechanism of the synapse, which is probably somewhat complex.

It has also been shown that acetyl choline is liberated during the stimulation of the nerves to the sweat glands, stomach, perhaps salivary glands and, most remarkable of all, to the adrenals. In this case Feldberg, Ming and Tsudzimura (*J. Physiol.*, **81**, 1934, 286) showed that eserine (which prevents the destruction of acetyl choline) increases the adrenaline output in response to stimulation of the nerves to the adrenal. Dale (*J. Physiol.*, **80**, 1934, 10P) points out that we now need names for the two kinds of chemical transmission due to substances like adrenaline and acetyl choline ; he suggests adrenergic and cholinergic. Thus post-ganglionic parasympathetic fibres are cholinergic, post-ganglionic sympathetic fibres adrenergic and all pre-ganglionics perhaps cholinergic.

Much further work has been done on the physiology of the nerve impulse, and the general conclusion that the impulses in a fibre can vary only in frequency still holds. However, there are still various theories which involve the passage of different types of impulse over the fibre, such as the resonance theory of Weiss, who has recently (*J. Comp. Neur.*, **61**, 1935, 135) performed the ingenious experiment of disconnecting the central end of a toad's dorsal root and planting it into a denervated muscle, thus establishing a mononeural connection between receptor and effector organ. The nerve fibres formed a successful junction, in the sense that the electrical stimulation of the sensory nerve was followed by contraction of the muscle ; however normal mechanical stimulation of the skin

failed to produce any such result, and this failure he interprets as supporting his resonance theory of the specific nervous impulses necessary to produce contraction of a muscle.

The investigation of the electrical changes accompanying the activity of the central nervous system may provide very important developments in the future, and Adrian and Matthews (*J. Physiol.*, **81**, 1934, 440) have begun a careful analysis of the potential changes led off by electrodes placed on the cerebral cortex of anesthetised rabbits. They find that the neurons pulsate in small groups over areas 3–4 mm. in diameter, and that slower waves of pulsation may pass over the whole cortex.

Lashley (*J. Comp. Neur.*, **59**, 341 and **60**, 57, 1934) has continued his most illuminating study of cortical function in the rat; showing by means of degeneration experiments that there is a point to point representation of the retina both in the thalamus and also in the occipital cortex. In discussing the significance of this "cortical retina" he shows that it cannot be a mere embryological accident since the orderly arrangement is wholly lost in the optic nerve, and the fibres are then re-sorted in the thalamus. Therefore "the spatial reproduction of the sensory surface upon the cortex becomes intelligible only upon the assumption that it forms the basis for some 'field organisation' in which the pattern of excitation in definite spatial relations is the determining factor in the arousal of the final motor response."

A similar standpoint has been adopted by Swann (*J. Comp. Neur.*, **59**, 1934, 175) during a study of the effects of partial destruction of the forebrain of rats on the retention of olfactory habits. He found that up to 70 per cent. of the whole cortex can be removed without loss of a habit. Particularly surprising was the fact that the whole hippocampal cortex, usually held to be mainly concerned with olfactory functions, can be removed without interference with the performance of a habit based on smell.

GENETICS.—N. P. Dubinin and M. A. Heptner (*J. Gen.*, **30**, 1935, 423–46) have demonstrated a new phenotypic effect of the Y-chromosome in *Drosophila melanogaster*. A dominant gene in the second chromosome produces uniform dark brownish eyes, and a so-called "factor," *My*, in the Y-chromosome interacts specifically with it to give a mottled effect. The action of Y is here very similar to that caused by genic material in the other chromosomes, since it controls the formation and distribution of pigment. Unlike "bobbed," however, *My* has no allelomorph in the X-chromosome. In this respect it presents a type of genetic action which is so far unique. It is, in reality, uncertain if the effect is due to a single

gene, and the position of the region concerned is in doubt, but there is some indication that it is situated in the middle of Y.

R. A. Brink and D. C. Cooper (*Genetics*, **20**, 1935, 22–35) have obtained a further instance in which it has been possible to show that genetic crossing-over involves an exchange of segments between homologous chromosomes. In studying a strain of *Zea mays* with two reciprocal translocations, they obtained 46 plants which combined the necessary crossing-over and cytological configuration to provide a test of chromatin interchange. These cytological tests of genetic crossing-over are very important. Previously, they have only been demonstrated three times: by Stern (1931), to whose work the present authors refer, by Darlington (1930) and by Mather (1933), both of whom they omit.

H. H. Plough and P. T. Ives (*Genetics*, **20**, 1935, 42–69) have confirmed in more detail the induction of mutation by high temperatures, according to the Goldschmidt-Jollos method. This involves exposing the larvæ to a temperature of 36·5° C. for 24 hours. Otherwise the stock (and controls) are kept at 24° C. They found that this treatment increased the mutation rate about sixfold.

J. B. S. Haldane (*Nature*, **135**, 1935, 907–8), studying the incidence of epiloia, an autosomal dominant, and hæmophilia, a sex-linked recessive, has obtained the first evidence bearing on the mutation rate in man (or, indeed, in any vertebrate). The former gives a value of about 1 in 120,000 per generation, and the latter of 1 in 50,000 to 100,000 per X-chromosome per generation. The author points out that this seems to indicate a somewhat higher mutation rate than that of *Drosophila*.

Palæontology.—Smith Woodward has recently (*Ann. Mag. Nat. Hist.*, **15**, 1935, 392–95) revised his views on the affinities of the Acanthodian and Arthrodiran fishes. He now considers that although they have the ordinary jaws of true fishes, their paired fins have evolved in the same ways as the Ostracoderms. The author bases this belief on the assumption that the original fin skeleton is the dermal skeletal covering as is the case in the Ostracoderms, and in the Arthrodires and Acanthodians the endo-skeletal supports are not developed except in the very late forms such as *Acanthodes bronni*.

Stensiö (*Med. om Grønland*, **97**, 1934, 1–58, 25 plates) has shown beyond doubt that *Phyllolepis* is a specialised relative of the Arthrodires and not related to *Drepanaspis* as has generally been accepted.

Piveteau (*Annal. de Paleont.*, **23**, 1934, 83–180, 10 plates) has described at great length in a beautifully illustrated memoir the Actinopterygian fishes of the Trias of Madagascar.

Stensiö (*Paleontol. Sinica*, C, **3,** 1935, 1–48, 17 plates) has described a new genus of Amioid, *Sinamia* from the Cretaceous of China. It is of particular interest in that it is the first Amioid to be described with rhombic ganoid scales. *Sinamia* is more primitive than *Amia* in having a supraorbital series of bones and six extrascapulars ; at the same time its parietals are fused into a single median bone.

Säve-Söderbergh (*Medd. om Grønland*, **98,** 1935, 1–211, 15 plates) has published the evidence for dividing the Embolomerous Amphibians into two superfamilies, the Loxommoideæ and the Anthracosauroideæ ; the former being related to the remainder of the Amphibia and the latter giving rise to the primitive Reptilia. The author has adopted the system of nomenclature for dermal bones, in which it is assumed that fusion of bones has occurred rather than the disappearance of a bone. Consequently such terms as "naso-rostro-premaxillary" are used in place of the current term "premaxillary." This system will not be acceptable to many workers.

PHYSICAL ANTHROPOLOGY. By L. H. Dudley Buxton, M.A., D.Sc., Exeter College, Oxford.

Before reviewing in detail some of the most important papers which have appeared recently, it may be convenient to state shortly the general lines which this science has followed during the last half year. To a certain extent the trend followed corresponds to national divisions, but this is not entirely the case. The Anthropological Society of Paris continues the anatomical tradition and is doing most valuable work in the accumulation of comparative data on the soft parts. Czekanowski and his school are developing in Poland, Germany, and elsewhere their method of analysis of actual populations into what they consider to be the relative proportions of the basal stocks of Europe. In this country for the most part recent work has tended in the opposite direction (as noticed previously in these notes), and workers have preferred to take populations as a whole and to compare them with another rather than to attempt to analyse the percentage proportions of say Nordic, Alpine and Mediterranean. Interest is still focussed in most countries on the analysis of blood groups, and a great deal of comparative material is being collected from all over the world. Considerable attention appears to be attaching itself to methodology, necessarily somewhat tentative and not to be summarised in a few words, but special mention may be made of two papers in the *Zeitschrift für Rassenkunde*, Vol. II, by Professor von Eickstedt on

local studies and by I. Schwidetzky on Von Eickstedt's formula for racial analysis.

Of recent papers the most outstanding is that by Drs. Karl Pearson and G. M. Morant (*Biometrika*, Vol. XXVI, Dec. 1934) on "The Wilkinson Head of Oliver Cromwell and its relationship to Busts, Masks and Painted Portraits." This very complete study, which is elaborately and carefully illustrated and documented, gives in detail all the evidence known on the physical form of the Lord Protector. Anthropological measurements are given of the Wilkinson head and of all known busts, masks and portraits, and the authors come to the conclusion that the evidence points to the Wilkinson head being actually that of Oliver Cromwell himself. The study is both of great historical interest and of importance as a model for the anthropological investigation of individual characteristics. The next volume of *Biometrika* (Vol. XXVII, 1935) contains an interesting account by K. Wagner on methods of measuring the internal diameters of the skull. The author concludes that the difference in general shape of the brain between anthropoids and prehistoric and recent races is not nearly so great as craniological investigations have led us to suppose, but he admits that it is possible that future investigation will lead us to attach great importance to such as exist. He also discusses the differences between the size of the two cerebral hemispheres. This paper has proved the occasion of a most valuable note by Dr. Pearson, who points out the difficulties raised by the last part of Dr. Wagner's paper and gives us in some detail an account of his methods of calculating cranial capacity from skull measurements. He once more explains the differences between *interracial* and *intraracial* constants. The two words are unfortunately very similar in sound and it is perhaps to be regretted that besides clearing up the confusion which obviously exists in certain quarters between them (they are respectively constants based on racial means and on individual measurements) Dr. Pearson cannot see his way to substituting two words which are less likely to be confused because of their similar sound.

The racial history of north-western Europe is a matter of interest to all of us and is represented in several recent papers. Dr. von Bonin in two papers (*Illinois Medical and Dental Monographs*, Vol. I, No. 1 (a newcomer in anthropological literature) and *Human Biology*, Vol. VII, No. 2) discusses the Cap-Blanc skeleton which is of Magdalenian date, that is the very end of the Upper Palæolithic period. He supports Dr. Morant's theory that the Upper Palæolithic people belonged to a single race. He concludes that the

type survived in Neolithic times and that there is at least one series which resembles the Upper Palæolithic on one hand and resembles on the other some primitive Nordics very closely in the size and shape of their skulls. Progress must obviously be based on variations of opinion. It would appear that Dr. Zejmo-Zejmis writing in *Anthropologie*, Tome 45, 1935, takes a very different point of view. His paper is on the "racial structure" of Scandinavia. He bases his conclusions on a study of the living population of that region and after an analysis by provinces based on the proportions of the Nordic, Mediterranean and "Lapponoid" races which he believes to occur in the various regions, he suggests the following racial history of the peninsula. First there was a primitive basis of the Mediterranean race, probably to be identified with the Cromagnon type of Upper Palæolithic times, a type which Von Bonin considers to be, if not Nordic, at least showing Nordic affinities. The second immigrants associated with the kitchen-midden culture are brachycephalic and the author would apparently connect them with the well-known round-headed people from Ofnet. Finally he suggests that the third invasion was that of Nordics who were the first political organisers, the Nordic centre of dispersion in Norway being of secondary origin. The point raised in these two papers is one of considerable interest to all students of the early history of man in north-western Europe, and the crux is whether the Nordic race are really the true indigines or whether the Neolithic people were of Mediterranean origin while the Nordics were later invaders. A not less interesting question about the population of England is raised by Dr. Morant in a brochure (*The History of the Human skeletons preserved in the ossuary of the Church of St. Leonard, Hythe*, by G. M. Morant, with a foreword by the Rev. Edwin W. Smith. F. J. Parsons, Ltd., London, Hastings, and Folkestone). Dr. Morant had previously discussed the bones at Hythe in a paper in *Biometrika*. This pamphlet is less technical. He holds that the Hythe bones are probably those of descendants of original Roman settlers in this country, with probably other admixture. He compares them with a long series of bones excavated at Spitalfields of uncertain archæological date. Both series show undoubted continental and non-British affinities. Morant finds it hard to believe that they represent immigrants in mediæval times and prefers to solve the dilemma by suggesting that the Spitalfields bones are those of actual Roman settlers. The matter is a very difficult one and of great interest in understanding the historical development of the English people. Apart from the Bronze Age people and one other series from Dunstable, also suspect archæo-

logically, all the very numerous finds of English bones are mesocephalic, the Hythe–Spitalfields people are definitely roundheaded. The present writer who has had the opportunity of examining two other mediæval series, as yet unpublished, which are of the same type, while fully admitting the continental affinities of all these bones prefers to see in them a mediæval rather than a Romano–British invasion, but until we have further evidence the matter must remain uncertain. A further study of Roman colonists in a very different part of the world is provided by Dr. Victor Lebzelter (*Anthropologie*, Tome 45, 1935, p. 65 *ff*). Dr. Lebzelter examined 4000 soldiers in Roumania. His conclusions are that the Roumanians of the Danube plain, of Moldavia, and of the central and southern part of the Carpathians, are the descendents of Roman colonists, both physically and linguistically. There was a pacific substitution of Slavs for these Romans. Subsequently these mixed peoples were rolled back into the mountains by the conquering German and Turanian peoples. The author concludes that an analysis of the Roumanian peoples shows a correlation between history, language and race.

A good deal further from home, Dr. Rivet in the *Anais da Faculdade de Ciencias do Porto*, Tomo XVIII, discusses the evolution and distribution of the " Oceanians." He suggests that at an extremely remote period a series of migrations originated from South-East Asia and the Malay Archipelago. These migrations extended fanwise and after peopling the islands of the two Oceans reached the New World to the East, Japan to the North, and Europe and Africa to the West. He finds no difficulty in suggesting that a maritime expansion took place at a very remote date. He concludes that the geographical environment in Malaysia, which witnessed such remarkable developments of the Primates in the Upper Miocene and Lower Pliocene, may have been such as to produce a condition of unstable equilibrium in the human species. This lack of organic equilibrium may have been such as to produce a series of mutations resulting in the successive appearance of Pithecanthropus (the Java Ape-man), Sinanthropus (Peking man), the Australian type, the Melanesian type, the Indonesian type and the Polynesian type. Dr. Rivet considers the differences between these not greater than mutants of *Drosophila*. This hypothesis, he concludes, would explain how it is that the Oceanic peoples, in spite of their physical and cultural differences, form a linguistic unity. We have here a very far-reaching hypothesis which is not likely to be accepted in all quarters, but a reasoned reply does not at present appear to have been published.

A very different type of paper is that by Professor D. G. Rokhline (*Anthropologie*, Tome 45, p. 33). The title of his paper is *La Radio-Anthropologie*, and he discusses very precisely the use of X-rays in Anthropology. He points out that this method enables the observer to study normal and pathological conditions and critical periods in general. The development of the individual in its various phases from infancy to old age can be exactly examined. Finally he emphasises the importance of radiology in the study of normal and pathological fossil and subfossil man. In an interesting aside he states that the occurrence of syphilis in Europe is long pre-Columbian. He quotes examples from Lake Ladoga (eleventh to twelfth centuries) and Trans-Baikalia in the Bronze Age in second millennium B.C. Finally he emphasises the point that, though much anthropological work which uses this method has been published, it has not for the most part been done by specialists in the technique.

NOTES

The Petrology of Banded Bituminous Coal (G. W. T.)

In 1919 Dr. Marie C. Stopes published a classification of coal based on macroscopic features, *i.e.* characters determinable in hand specimens. She coined the terms vitrain, clarain, durain and fusain for easily-determined varieties of bituminous coal. During the last fifteen years these terms have been widely adopted and have been made the basis of a great deal of fruitful research. Unfortunately, however, some investigators have caused confusion by applying these terms to characters observable in thin sections under the microscope. Dr. Stopes has now clarified the position by constructing a detailed and elastic schedule of classification in which not only are the macroscopic terms amplified, but new names are proposed for characteristic units identifiable under the microscope (" On the Petrology of Banded Bituminous Coal," *Fuel in Science and Practice*, Jan. 1935, **14**, 4–13). These units are called *macerals* on the analogy of minerals, and are given names with the distinctive termination—*inite*. Their nature is sufficiently indicated by such terms as *vitrinite*, *fusinite*, *xylinite*, *exinite*, *cutinite*, etc. ; while the rock-types which are composed of accumulations of one or more of these macerals retain the termination—*ain*.

The skeletal outline of Dr. Stopes's new classification may now be given as follows :

	Rock-types.	Macerals.
Simple	Vitrain Fusain	Vitrinite Fusinite
Complex	Clarain Durain	Vitrinite, xylinite, exinite, etc. Micronite (or " Residuum "), exinite, cutinite, etc.

This scheme is elastic in that it is possible to admit new terms without disturbing its structure. Dr. Stopes herself, by adopting the results of certain Continental workers, divides vitrain into *eu-vitrain* and *pro-vitrain*, eu-vitrain again being sub-divided into

ulmain and *collain*, pro-vitrain into *periblain*, *suberain* and *xylain*. Such terms as *cutain*, *algain*, etc., may in time find a place in the classification, with corresponding maceral names, when the scheme is extended to cover coals other than the Palæozoic banded bituminous varieties. We think that Dr. Stopes's new classification, or rather extension of the old classification, represents a considerable advance in the petrological study of coal, and incorporates a great many recent observations. A valuable bibliography of coal petrology is appended to the paper.

"Zeitschrift für Rassenkunde" (L. H. D. B.)

A new anthropological journal is an event which calls for special attention amongst all those who are at all interested in this branch of learning, and Dr. von Eickstedt is to be congratulated on his energy and enthusiasm—not to say daring—in this venture. The journal is of demi-quarto size and printed on paper of a quality which admits the use of half-tones in the text, and those of us who know the editor's own skill with the camera will realise the merits of this arrangement. It is a great advantage to have the illustrations in their proper place and not to have to search for plates at the end. In the first number there is an article which should be of considerable interest to English readers. After the International Ethnological Conference last summer, to which attention has already been drawn in these columns, Dr. von Eickstedt went on a short anthropological tour in Montgomeryshire. He measured about sixty men and a few women. The results of his work appear in this article. The author gives a brief summary of the anthropological history of Wales in particular and England in general leading up to the modern people whom he measured. It is an admirable popular summary of the subject. In some matters perhaps one might quarrel with the author, for instance he describes the Long Barrow skulls as being small and gracile. One can hardly think that he has ever looked at any large collection of these skulls; he could not have failed to notice that they are, taken as a whole, the longest of British skulls of any period, nor should we call them exactly "gracile". He believes in common with many anthropologists that they belong to the Mediterranean Race and concludes that the double peaked curve in his modern figure is due to the admixture of Nordic and Mediterranean. His figures are too few for any conclusions; it is extraordinary what peaks you may get in a curve based on a few measurements. But he gives us credit for being mainly Nordic. In spite, however, of what are admittedly controversial subjects this particular article is a delightful popular

contribution to the subject. A very different type of paper is that in the second part by Dr. Schwidetzky, who is himself a member of Professor von Eickstedt's school in Breslau. This paper is a long and detailed account of the anthropology of modern Poland, illustrated by a series of extremely valuable maps. This paper is not easy reading, it is most learned, careful and thorough, though it lacks the practised skill and easy style which makes the previous paper we have noticed so pleasant to read. The author gives first a critical discussion of the methods of the Lemberg school, followed by an analysis of the Czechanowski type system, a method which by mathematical analysis endeavours to divide up any group of people into its constituent main elements and hybrid types. An analysis follows of the various parts of Poland and the racial types actually found there. There can be little doubt that this paper will remain for some time the standard on the ethnology of Poland, both for its subject matter and for the maps and photographs which illustrate it.

These two papers have been selected as the editor seems to consider them as the two most important contributions, but other varied articles of different lengths appear, an analysis of literature and an account of appointments to anthropological posts.

The journal is published by Ferdinand Enke, Stuttgart, and costs RM.22—per volume of three parts.

Publications of the South African Institute for Medical Research (P. J.)

REPORT FOR THE YEAR ENDED DEC. 31, 1933

This report deals with research and routine investigations. In the routine division which represents work done by the departments of bacteriology, vaccine, pathology, biochemistry, parasitology and legal medicine, and which includes the clinical pathology of the Johannesburg General Hospital, the total number of investigations made was 97,690, the specimens being derived from Europeans, Eurafricans and natives. Short analyses of this work are given in tables and annotations. From these may be noted the great increase in the number of cases submitted for Wassermann test, a total of 23,795; also the total number 15,828 of tuberculosis specimens mostly sputa, about 19 per cent. of which were positive. There was a considerable increase in the incidence of typhus fever; 174 sera gave high titre of agglutination compared with 59 in the previous year. Specimens of a typhus-like disease known as "tick-bite fever" were tested against the requisite strains of *B. Proteus*. Much confusion might be avoided if this disease were known as

"tick-typhus" as invariably the name "tick-bite fever" is confused with "tick fever" or "relapsing fever" which is caused by a spirochæte; whereas "tick-bite fever," as it occurs locally, has been proved by Pijper to be caused by rickettsia bodies. Under general bacteriology there were 10,265 various specimens, an increase of 1,003 over the previous year. These figures reflect the progressive activity of this important Institute.

In the research division is reported an investigation into the bacteriology of pneumonia among non-mining and mining natives of the Witwatersrand district. Some of the mining natives were prophylactically inoculated with a vaccine containing seven different strains of pneumococcus. The absence of these strains in cases of pneumonia occurring in these subjects indicated immunologically the marked antigenic value of the vaccine. In the non-inoculated mining natives 64 per cent. showed pneumococcus of group B (American group II) confirming a previous finding that this strain is normally prevalent in native mine labourers; by, inoculation there was an almost entire elimination of this strain. The organism of the unclassified group S (American group IV), generally not regarded as especially virulent, is most commonly recovered from the respiratory tract of healthy persons, but in the Witwatersrand the pneumococcus of this group is undoubtedly responsible for acute pneumonia among natives.

The nature and injurious effects of air-borne dust of the mines is a continuous subject of enquiry. It has been unexpectedly found that the size frequency distribution of the particles of all air-borne dust is the same; is independent of different procedures of rock breaking and handling; is not influenced by the quantity of the dust, and moreover unfiltered air-borne dust presents similar size frequency distribution to filtered dust. Details of the method of estimating the size of the dust particles are not given, but these extraordinary results seem to need further study. It is remarkable that a series of ore bins dealing with 25,000 to 35,000 tons of ore per week and swept by 20,000 cubic feet of air will deposit more than 80 lb. of air-borne dust upon the filter in a week. Investigation was made of the quartz/alumina ratios of the rock which gives rise to the air-borne dust, of air-borne dust, and of air-borne dust recovered from the lungs of silicotics. It is known that there is less SiO_2 in air-borne dust than in the rocks that give rise to it. The "banket" is composed of hard rock quartz pebbles set in a "mortar" of rotten rock of mostly micaceous matter rich in sericite. The rock analyses give 70–75 per cent. SiO_2; the dust caught on the filter gave only 52–55 per cent., the proportion of alumina

correspondingly rising. Analyses of dust specimens (only 5) recovered from lungs of miners gave a close agreement with that of the air-borne dust. That micaceous particles are silicosis producing remains yet to be proved, but it is clear that in the presence of SiO_2 the fibrous particles are retained in the lungs in large numbers and set up tissue changes indistinguishable from the changes set up by particles of SiO_2.

In the biochemical department a dietetic and nutritional survey has been started; attention being directed to the betterment and increase of food resources, comprising soil survey, improvement of pastures, cattle and milk production; the use of fish, soya meal, etc.; prevention of waste in cooking; the preserving of fruit and vegetables, and the development of food transport facilities. A study of native foods and customs about which much is still quite unknown would be desirable but difficult to carry out. It seems advisable to reconsider some of the standards for diet adequacy; it would be especially useful if the degree of nutrition actually obtained were more susceptible of measurement. At present there is a good deal of uncertainty as to the optimum growth curve for groups of children.

A research extending over several years was undertaken to find out whether prolonged cultivation of strains of streptococcus and of *Salmonella gallinarum* on liquid media composed of extracts of Rous sarcoma and mouse carcinoma would acquire a specific affinity for tumour tissue from which the extract was derived. In a previous report it was stated that there is a slightly larger affinity for tumour tissue when the tumour in the living fowl is treated with injections of filtered cultures of micro-organisms grown on specific tumour extract media as compared with filtered cultures of micro-organisms grown on extract of normal or non-cancerous tissues. These results are not confirmed by subsequent trials. It was found that in the majority of fowls and mice which were either carriers of tumours or received tumour inoculation simultaneously with the filtered culture media, no difference could be found between fowls and mice treated with filtered culture of micro-organisms grown on tumour extracts and fowls or mice treated with filtered cultures of micro-organisms grown on extracts of normal tissue.

It is considered that this negative result is of value "because it definitely ranges with the negative results of all biological methods which aim at destroying cancer cells at a distance, *i.e.* via the circulation, whilst leaving undamaged the normal tissue of the body."

The non-specificity of Bendien's sero-diagnosis test for early cases of malignant disease were demonstrated on 44 neoplastic and

24 non-neoplastic cases. It is remarked that we have no indication of the presence in cancer cells of an extrinsic virus which would cause alteration in the serum similar to that found in disease due to bacteria or viruses.

An account of a paper by Dr. Ligneris dealing with tissue culture work is given below.

The above report is a valuable contribution to medical science.

Studies in Cell Growth (Part 2)

Dr. M. J. A. des Ligneris describes his experiments on the growth in vitro of normal mouse cells and mouse cancer cells (carcinoma and sarcoma) in neutral and immune media (serum and plasma). This paper is a contribution to the Lumsdon–Ludford controversy. In the author's opinion the question at issue and around which a serum treatment of cancer is centred, is that of the specificity or otherwise of anti-malignant—cell bodies in animals treated by injection of cancerous tissues ; and the basis for the possibility of the serum treatment of cancer is to be sought in the demonstration, by laboratory methods, of anti-malignant—cell bodies lethal to cancer cells but harmless to normal tissue cells, in the serum of animals treated by injections of cancerous tissue. After describing the technique employed, and detailing a number of experiments which are illustrated by some 109 microphotographs showing the characters of the in vitro growths he obtained, the author shows clearly that in the serum of sheep treated with repeated injections of mouse carcinomatous tissue, the developing anti-bodies have none other than antispecies characters ; it was not possible to detect in these sheep sera any kind of anti-body capable of showing any specifically " anti-malignant" characteristics.

The same effect was regularly obtained with mouse carcinoma or with mouse sarcoma cells in vitro, whether the serum or plasma used originated from a sheep injected with normal mouse organs or with mouse carcinoma. The effect on normal mouse cells (liver lung kidney spleen) growing in vitro was the same whether the one or the other of the two kinds (malignant or normal) of anti-mouse—sheep serum was used.

These results accord with those previously obtained in immunity experiments with Rous sarcoma in fowls ; the anti-bodies forming in the blood of tumour-bearing birds depend on individual and on racial particularities ; there are no signs of a specific " anti-malignant " anti-body, such as would be expected if an extrinsic factor (micro-organism, virus) were present. In the case of mouse-carcinoma injections into sheep, the anti-bodies forming in the

sheep's blood act equally well against normal mouse cells and against mouse sarcoma as they do against mouse carcinoma cells.

Institute for Medical Research, Kuala Lumpur, Annual Report for 1933 (P. J.)

Besides the routine examination of specimens submitted by medical and health officers and other departments, particular attention is invited to Dr. R. Green's report (Appendix IV) on malaria, and the work of Drs. Lewthwaite and Savoor on tropical typhus and Japanese river fever (Appendix II).

The malarial research has been concerned with the substitution of atebrin for quinine in the treatment of malaria as it occurs on rubber estates, that is, among ambulatory patients who do not need in-patient hospital treatment. Among labourers in Malaya the majority of malarial infection is subtertian.

A series of experiments were undertaken to compare the efficiency, cost and convenience of a mass treatment of all persons living in the estate lines with atebrin, followed after an interval of six to eight weeks by selective treatment of individuals who are found by thick film blood examinations to be infected with malaria, with the ordinary treatment as frequently given in such lines. The treatment-technique could be undertaken only on an estate provided with a microscope and a dresser proficient in examination of blood films. This series terminated at the end of six months. Analysis of the results shows that the whole atebrin-treated group only required about a fourth of the number of " day treatments " required by the quinine-treated control group; each individual treated with atebrin was under treatment for little over a fourth of the number of days of each individual treated with quinine. The costs in the two instances happened to coincide.

In a second series, intended to simulate conditions on an estate which was unprovided with a microscope, all individuals, including newcomers, were given a preliminary five-day course of atebrin; and *all* cases subsequently showing *any* kind of fever were re-treated with atebrin. During the whole of the period the working efficiency of the labourers maintained a high level, despite a 50 per cent. incidence of malarial fever during the seven months of the experiment.

At the worst malarial period only 2 per cent. of available working days were lost owing to fever; the labourers returned to work usually after the third day of treatment. The cost of atebrin per unit of the population was about \$1·60 during the seven months, or 75 per cent. greater than the corresponding cost in the previous experiment.

Atebrin has the advantage that the daily curative dose can be given at one muster. Workers preferred atebrin to quinine from the fear of cinchonism, so they reported earlier for treatment, which may account for the absence of serious cases.

A third series of experiments were concerned with the comparative efficacy and cost of giving a seven-day course of quinine in curative doses as against a six-day course of atebrin. The results are not yet complete, but there is evidence to show the superiority of short "curative" courses of quinine over prolonged courses averaging about X gr. daily.

No prophylactic effect was ascribable to atebrin.

The development of crescents and their persistence in the blood seems to be quite unaffected by quinine, atebrin or tebetrin; nor did these drugs prevent mosquitoes becoming infected from crescent carriers at any time during the treatment. Relapse rates, or the reappearance of trophozoites in the blood, were markedly less and after longer intervals with atebrin than with quinine treatment.

Of the typhus investigation Drs. Lewthwaite and Savoor report the laboratory maintenance of the three strains, the origins and characteristics of which were summarised in the 1932 annual report, *viz.* the B "Seerangayee" strain representative of rural, scrub or "K" type of tropical typhus, now in its 58th generation in the guinea pig; the "Manickam" strain representative of the urban or "W" type of tropical typhus, which is now in its 46th generation in the guinea pig; and the "Wellington" strain (isolated from a patient of that name) representative of Japanese river fever, which is now in its 54th generation in the rabbit, being maintained therein by the intra-ocular inoculation method. The "K" and "W" strains have been shown to be immunologically distinct in guinea pigs, and this distinction has now been corroborated by parallel experiments on rabbit strains.

With the view to investigate the relation of the two types of tropical typhus that occur in Malaya with Rocky mountain spotted fever collaboration of this institute was invited by Dr. Parker of the Rocky mountain spotted fever laboratory (Hamilton, Montana, U.S.A.). A supply of uninfected nymphs and adults of the wood tick, *Dermacentor Andersoni,* were forwarded from Hamilton to Kuala Lumpur. These were fed on infected guinea pigs of the "Seerangayee" "K" and "Manickam" "W" strains of tropical typhus, then removed and returned to Dr. Parker for reactivation by feeding on guinea pigs in his laboratory. This institute received ten infected Rocky mountain fever adult ticks, *Dermacentor Andersoni,* that had been fed at Hamilton two to three days prior to

despatch, and were placed on an uninfected guinea pig. Six of the ticks attached and the guinea pig showed a febrile reaction on the fifth day. By passage of heart blood virus from this guinea pig the disease was reproduced in a second generation of guinea pigs and no difficulty has since been experienced in maintaining the strain by similar passage ; it is now in its ninth generation. Scrotal swelling occurs in 90 per cent. of the infected males ; the majority die.

Eight guinea pigs recovered from the "K" strain infection, and six of which proved immune were inoculated with spotted fever virus ; they all reacted as the two control guinea pigs, all six (males) developed scrotal swelling ; the infection proved fatal. Of thirteen guinea pigs recovered from "W" strain infection ten reacted typically to spotted fever virus, six out of seven males developing scrotal swelling.

Many batches of serum drawn at the end of the first week of convalescence from both guinea pig and man recovered from either urban "W" or rural "K" type of tropical typhus were sent to Dr. Parker who in a personal communication reports that no protection against spotted fever serum virus was afforded by either type of convalescent serum sent to him.

Miscellanea

The Honours List published on the occasion of the Jubilee and of the King's birthday included the names of the following workers in the domain of science : *O.M.*: Sir Frederick Gowland Hopkins, P.R.S. *K.C.B.*: Dr. G. C. Simpson, director of the Meteorological Office. *K.B.E.*: Prof. J. C. McLennan, emeritus professor of physics in the University of Toronto. *Knights*: Mr. N. Ashbridge, chief engineer of the B.B.C. ; Prof. J. Barcroft, professor of physiology, University of Cambridge ; Dr. L. L. Fermor, director of the Geological Survey of India ; Mr. P. P. Laidlaw, pathologist to the Medical Research Council ; Dr. S. L. Pearce, engineer-in-chief of the London Power Co., Ltd. ; Dr. C. L. Woolley, archæologist. *C.B.*: Dr. W. T. Calman, keeper of zoology, British Museum (Natural History) ; Dr. F. S. Sinnatt, director of fuel research, D.S.I.R. ; Mr. H. E. Wimperis, director of scientific research, Air Ministry. *C.M.G.*: Mr. E. Harrison, director of agriculture, Tanganyika Territory ; Mr. E. J. Wortley, director of agriculture, Trinidad. *C.I.E.*: Lt.-Col. R. Knowles, secretary of the Calcutta School of Tropical Medicine. *C.B.E.*: Dr. W. F. Bewley, director of the Experimental and Research Station of the Ministry of Agriculture and Fisheries, Cheshunt ; Dr. C. E. Cook, chief protector of aborigines, Northern Territory, Commonwealth of Australia ;

Dr. G. W. M. Findlay, of the Wellcome Research Institution, London; Prof. A. Fowler, emeritus professor of astrophysics in the University of London; Dr. W. L. Miller, president of the Royal Society of Canada; Mr. F. T. Shutt, lately Dominion Chemist, Dominion of Canada; Dr. N. V. Sidgwick, reader in chemistry in the University of Oxford. *I.S.O.:* Mr. W. Dallimore, keeper of museums, Royal Botanic Gardens, Kew. *O.B.E.:* Prof. G. B. Bryan, professor of physics, Royal Naval College, Greenwich; Dr. W. A. Richardson, principal of the Technical College, Derby; Mr. A. Walter, director, Meteorological Service, East Africa.

Mr. Walter Elliot, Minister for Agriculture and Fisheries, has been elected a Fellow of the Royal Society under the statute which permits the election of "persons who have rendered conspicuous service to the cause of science, or are such that their election would be of signal benefit to the Society."

Dr. Irving Langmuir, physical chemist, and Prof. Max Weber, ichthyologist and oceanographer, have been elected foreign members of the Royal Society.

The Albert medal of the Royal Society of Arts for 1935 has been awarded to Sir Robert Hadfield for his work in metallurgy and his services to the steel industry.

The medal of the Society of Chemical Industry has been awarded to Dr. E. F. Armstrong for his conspicuous services to chemistry.

The Franklin Institute has awarded Franklin medals to Prof. A. Einstein for his work on relativity and the photo electric effect and to Sir Ambrose Fleming for his work on the thermionic valve.

Dr. J. S. Haldane, director of the Mining Research Laboratory in the University of Birmingham, has been elected a foreign associate of the U.S. Academy of Sciences.

Dr. Edward Mellanby has been elected Fullerian professor of physiology in the Royal Institution in succession to Sir Grafton Elliot Smith.

The Actonian Prize of 100 guineas which is awarded septennially by the Managers of the Royal Institution for "the best essay illustrative of the wisdom and beneficence of the Almighty" has been awarded to Mr. W. T. Astbury for his papers entitled "X-ray Studies of the Structure of Hair, Wool and Related Fibres" published in the *Philosophical Transactions of the Royal Society*. A shortened account of this work appeared in SCIENCE PROGRESS, Vol. XXVIII, p. 210.

Dr. Bernard Smith has been appointed to succeed Sir John Flett as director of the Geological Survey of Great Britain.

We have noted with great regret the announcements, during the past quarter, of the death of the following workers well known in scientific circles :—Sir Robert Blair, lately Education Officer to the London County Council ; Dr. A. Bramley, head of the Department of Pure and Applied Science, Loughborough College ; Mr. W. R. Butterfield, curator and librarian at the Brassey Institute, Hastings ; Prof. J. B. Cohen, F.R.S., emeritus professor of organic chemistry in the University of Leeds ; Prof. M. Cremer, emeritus professor of physiology in the University of Berlin ; Mr. J. T. Cunningham, marine biologist, Queen Mary College ; Dr. D. N. Dunlop, director of the British Electrical and Allied Manufacturers' Association ; Mr. W. S. Franks of Brockhurst Observatory, East Grinstead ; Prof. Edwin B. Frost, emeritus director of Yerkes Observatory ; Prof. R. M. Holman of the University of California, botanist ; Mr. C. T. Kingzett, author of the Chemical Encyclopædia ; Prof. W. Kolle of Frankfort, bacteriologist ; Prof. H. M. Macdonald, professor of mathematics in the University of Aberdeen ; Miss Ida M. Roper of Bristol, botanist ; Dr. C. E. St. John of the Mount Wilson Observatory, solar spectroscopist ; Mr. C. E. Stromeyer of Manchester, engineer ; Dr. H. H. Thomas, F.R.S., petrographer to the Geological Survey ; Prof. Hugo de Vries, For.Mem.R.S., emeritus professor of botany in the University of Amsterdam ; Prof. F. Went, For.Mem.R.S., of Leyden, botanist.

At the meeting of the International Astronomical Union held in Paris during the period July 10–17 it was resolved that in future the term U.T.—Universal Time—should be used in place of G.C.T.—Greenwich Mean Time reckoned from midnight.

A. J. Dempster of the University of Chicago, using a mass-spectrograph of new design, has investigated the isotopic constitution of a number of elements hitherto unresolved (*Nature*, June 15, July 13, Aug. 3). He finds that platinum has five isotopes of atomic masses, 192, 194, 195, 196, 198, but no companion to the rhodium isotope 103 discovered by Dr. Aston could be observed. Palladium appears to consist of six isotopes, 102, 104, 105, 106, 108 and 110, but with gold, as with rhodium, only one component, mass 197, could be detected so, that if other isotopes exist they must be present only in very small quantities. The result is not in agreement with the value now accepted for the atomic weight of gold, namely 197·2, and it is probable that this value needs revision. Finally Dempster has found that uranium has a second isotope of mass 235 which is present in very small amounts—apparently less than 1 per

cent. of the whole. The chief isotope, 238, was discovered by Aston who showed that it formed at least 97 per cent. of the common element. The lighter isotope is probably the parent of the actinium series of radioactive elements.

In the July number of SCIENCE PROGRESS Dr. Dobson gave an account of the results of recent investigations of the upper atmosphere pointing out, in particular, that at a height of 35 km. the temperature of the air begins to rise at the rate of about 6° C. per kilometre so that at 60 or 70 km. it has reached 100° C. Further evidence on this matter is provided by the seasonal change in the maximum ionisation in the F-layer. Assuming that the variation of the density of the atmosphere with height remains the same at all times of the year the ionisation in the F-layer at noon in summer should be nearly twice that at noon in winter, while in fact it is less. Appleton (*Nature*, July 13) points out that the discrepancy may be explained by supposing that the molecular temperature is from three to nine times as great in summer as in winter (and the air density correspondingly less). If this is so the temperature of the air at a height of 300 km. must be about 1,000° C. on a summer day. The rate of variation of the ionisation at sunset in winter supports his conclusion and it is possible that the sub-division of the F-layer into two parts during the summer period may be due to large changes of temperature at high altitudes.

The *Journal of Research* of the National Bureau of Standards for April 1935 contains an important paper by Vinal and Craig dealing with the chemical reactions which occur in the normal working of an ordinary lead accumulator. The double-sulphate theory, given in the elementary text books, was proposed by Gladstone and Tribe in 1882. It is expressed by the well-known equation

$$PbO_2 + 2H_2SO_4 + Pb \rightleftarrows 2PbSO_4 + 2H_2O$$

which implies that "the passage of one faraday of electricity (96,500 coulombs) in the direction of discharge should result in the consumption of one equivalent each of PbO_2 and Pb and two equivalents of sulphuric acid, while two equivalents each of lead sulphate and water are formed." Many investigators have endeavoured to test the truth of this statement, and while some of them have found that two equivalents of sulphuric acid are consumed per faraday others have concluded that it is materially less. In 1916 Féry put forward a theory which supposed that a higher oxide of lead is formed on the positive plate (at charge) and a

subsulphate at the negative, the reaction requiring the consumption of only one equivalent of acid per faraday. Others have supposed that a basic sulphate is formed on discharge so that less than two equivalents of acid are used up.

Measurement of the total amount of acid present in the cell at any stage of the charge or discharge is complicated by the difficulty of determining the amount in the pores of the plates and in the separators. Vinal and Craig have overcome this difficulty by using the "method of mixture": the concentration of the solution in the cell is found; a known mass of electrolyte of known but different concentration is added; the concentration of the final solution is determined, and then an obvious calculation gives the mass of acid present originally. The mixture was stirred occasionally by means of an air jet and it was observed that two hours after the liquids had been mixed the e.m.f. of cell became constant, indicating that the electrolyte in it had become homogeneous. The cell used was built up of pure lead grids pasted with lead oxide mixed with dilute sulphuric acid and all necessary precautions to avoid errors were taken. As a result the mean value of the equivalents of acid per faraday was found to be $2{\cdot}02 \pm 0{\cdot}03$ in excellent accord with the double-sulphate theory. It is interesting to note that two sets of experiments were made, one in the spring and the other in the autumn of 1934. In the intervening six months the cell was left in a partially discharged condition without harmful sulphation taking place or visible liberation of gas from the negative plate—a result probably due to the use of pure materials in the cell and the careful insulation of the terminals.

The *Report of the National Physical Laboratory* for the year 1934 (H.M. Stationery Office, 13*s*. net) shows that there has been a marked increase in the industrial work carried out in the laboratory. For example, in 1933 forty-five ship designs were tested in the William Froude Laboratory, while in 1934 the number was sixty, a new record which involved an increase of staff and overtime work. The Sound Division of the Physics Department also was exceptionally busy owing to the increased attention now being devoted to noise problems, in particular to the silencing of aircraft, the soundproofing of walls and floors in flats and the limitation of the noise produced by mechanically propelled vehicles. The number of high-precision thermometer tests made during the year was 30 per cent. greater than in 1933, while the volumetric glassware tests showed a 40 per cent. increase; on the other hand fewer clinical thermometers were tested—a mere 410,000!

The Heat Division investigated materials suitable for the heat insulation of containers for solid carbon dioxide and found that, for this purpose, expanded rubber (density 5·4 lb. $ft.^{-3}$) has the lowest thermal conductivity, namely 0·000067 cal. $cm.^{-1}$ $sec.^{-1}$ deg. $C.^{-1}$ at $-27°$ C. The substances tested included balsa wood and cork of various densities, and it was found that, for such materials, the thermal conductivity at a fixed temperature is approximately a linear function of the cube root of the density, the conductivity of air lying on the extrapolated line.

The Metrology Department reverified the various Laboratory standards of mass and reported that the reference standard kilogrammes and pounds have remained constant to within 1 part in 10^7 during the last five years. A discrepancy of 5 parts in 10^7 was found in the values obtained for the wave-length of the cadmium red radiation at the Laboratory and at the Reichsanstalt and efforts to trace the cause of the difference were still being made when the Report was written. An investigation on pivots and jewels for electric meters showed that the durability of the jewel varies very markedly with the direction, relative to the crystal axes, in which it is cut. The best watch submitted for test was made by the Omega Watch Co. of Bienne. It obtained 97 marks as against the record of 97·4 awarded to a watch submitted by the same company in 1933.

The Engineering Department completed a series of measurements of the mean specific heats of a number of gases over the range 100° C. to 2800° C. It continued the work on cylinders for gas traction purposes reported last year, and its other multifarious activities include measurements of the movement of parts of the Tower of London, of the vibrations of the buildings of New Scotland Yard and tests of various types of firemens' helmets.

The Aerodynamics Department has tested a new type of biplane designed to permit slow flight under full control. The upper wings are much tapered and the lower wings slope considerably so that their tips come close behind the narrow tips of the upper wings. The William Froude Laboratory has obtained data to enable the shipbuilder to design a ship giving minimum pitching in rough weather, and has shown how the shape of the hull and the design of the propeller of coastal vessels may be modified to improve their performance by a figure approaching 20 per cent.

ESSAY REVIEW

OUR BLIND NEGLECT OF KNOWLEDGE. By PROFESSOR H. E. ARMSTRONG, Ph.D., LL.D., D.Sc., F.R.S. Being a Review of **The Frustration of Science,** by SIR DANIEL HALL, F.R.S., J. G. CROWTHER, J. D. BERNAL, PROFESSOR V. H. MOTTRAM, DR. ENID CHARLES, DR. P. A. GORER and PROFESSOR P. M. S. BLACKETT, F.R.S. With a Foreword by FREDERICK SODDY, M.A., LL.D., F.R.S. [Pp. 144.] (London: George Allen & Unwin, Ltd., 1935. 3*s*. 6*d*. net.)

A STRANGE medley, little bread to much sack; yet a book worth reviewing, so many questions are raised. In his brief *Foreword*, Professor Soddy, who stands as a forest giant above the low arboreal growth of Oxford, a man who, daring to look abroad, has views of his own and expresses them, forcibly urges upon scientific workers the need in which they stand of developing a sense of social responsibility. Certainly, at present, as a body, we are behaving as hopeless idiots—allowing the public to believe that "science" means fine writing about the stars and the smashing of atoms, to little useful end, whatever the ultimate value may be—no one of us making clear how our work has to do with bread and butter. A strong feeling of public dissatisfaction is growing up, even among ourselves, that we have in no way made our knowledge felt. It is for the public to insist, says Professor Soddy, that it be ruled by those who have made knowledge:

> It should require that its universities and learned societies should no longer evade their responsibilities and hide under the guise of false humility as the hired servants of the world their work has made possible but do that for which they are supported in cultured release from routine occupations and speak the truth though the heavens fall.

They will have far more to do than speak the truth: they will need first to learn what is the truth, often even to deny their faiths. The indictment is certainly true of our Royal Society, which seems to have lost all sense of social responsibility, having allowed an organised bureaucracy, the Department of Industrial and Scientific Research, to take its place, so much so that the two bodies are

officered by the same officials: the sacrifice of independence would seem to be complete, defeat can no further go. Not only has our so-called "science" no collective voice—it has not a single voice in the councils of the nation.

To apply Professor Soddy's solution will be difficult. How an ignorant public is to galvanise into social workers a privileged class of armchair specialists, reclining in the languorous groves of abstract experimental inquiry, is not clear. Perhaps a good beginning could be made, following the lines of a recent Spanish novel, *The Seven Pillars*, by calling in His Satanic Majesty to rid the world of sin in the form of Oxford. Over a long period, Oxford has been a complete bar to all scientific advance in education, being in the hands of a class whose scientific mentality is such that it cannot understand the nature of modern progress. As the schools are controlled either by Oxford or by men (and women) of the Oxford type from Cambridge, our educational system remains so prehistoric that it is without relation to present-day needs and knowledge; therefore, no foundations are laid.

The essays are mainly written from a point of view scarcely congruent with Professor Soddy's. Their authors display no great anxiety to make themselves useful; most take up the "What a good boy am I" attitude and seem to feel hurt that their wonderful superiority is not more recognised and further subsidised. In reality, scientific workers have to establish their worth; are the majority worth their salt—is the work they are doing likely to be of any account?

My old friend Sir Daniel Hall—I was able fifty years ago to help him to obtain his degree at Oxford—still has the elegant Oxford manner and too much of its timidity. His essay upon *Science and Agriculture* is necessarily interesting, discursive though it be, he has so wide and cultured an outlook. Agriculture, however, now needs roaring bulls. Taking it together with his graceful Rede Lecture at Cambridge,[1] we have a fair picture of the Agricultural situation, above all of its overwhelming complexity; yet he gives no clear lead. The field of Agriculture must be the great world-field of battle in the future—a battle in which the nations will all be forced more and more to engage. Hitherto, we have only toyed with its problems, though at the moment we are engaged in an already serious skirmish with our Dominions over beef, mutton, bread and milk: poor Cinderella may remain at home on

[1] *The Pace of Progress. The Rede Lecture, 1936.* By Sir Daniel Hall. Cambridge University Press.

the farm in rags, for all our cousins care: they would feed us, letting us only work with and be machines; as the mother country, our place should be under the factory roof, not the free light of the heavens. Growing crops is a minor consideration. How to distribute and dispose of crops economically, when grown, is our most serious problem. We are so commercialised, so capitalistic, also now so mechanised, so obsessed with the idea of always buying in the apparently cheapest market, so utterly selfish, by nature, in individual outlook—because we have never yet thought of seeking to help one another—that at present it is impossible to see the wood for the trees. We need a Lawrence to study this new war problem, in its economic entirety, to devise tactics which will make it possible to meet the numberless irregular attacks to which our agriculture is now subjected. To market milk is not enough. Sir Daniel Hall sees that ultimately some measure of State organisation must be introduced but that to do this, without destroying the individuality of the farming community, will be a difficult task. He fears that control will inevitably slow down progress and the rate of change. The question is—what is Progress? Surely, what we need is to make systematic use of our knowledge—this would indeed be progress and should involve no loss of interest nor diminution of the sense of responsibility.

Education alone will solve the problem—yet we are not making the slightest attempt to provide this. Apart from our worthless schools, it is doubtful if we have a single effective Agricultural College; most so-called Agricultural Research is of too academic a character to be of any prospective, practical value. The Ministries of Agriculture and Health are giving no scientific thought to the problems; they are not organised to this end. The situation is simply farcical.

Passing to the second essay, that on *Aviation*—J. G. Crowther talks little short of nonsense in this. Flying is probably the greatest curse that "science" has brought upon mankind, as we shall learn if ever we go to war again. To quote his conclusion:

> Long-distance aviation has been severely frustrated by the nationalistic refusal of governments to allow air-routes to be operated in the most economical and effective manner over their territories. Aviation has been frustrated by its submission to military needs, by the possessors of real estate, by nationalistic governments; and its influence on human culture has been frustrated because it has been developed for the use of soldiers and sportsmen and not for the service of the working civilian, whose intellectual and physical labour creates the most durable human values. Aviation will remain frustrated as long as it is not conducted primarily to serve the creative classes.

Surely this is just—bunkum! As we can't get culture into ourselves, let alone our next-door neighbours, flying to our cousins in Australia won't help us. The essay is a good illustration of the unbalanced thought and talk in specialist circles to-day.

J. D. Bernal "high-fallutes" on *Science and Industry*, at greater length than any of the other essayists. The wonderful things that "science" might do, if only it were allowed liberty of action, is dwelt upon *ad nauseam*; everything, in turn, might be improved away by the advances, if only free play were allowed to the worker; everything might be manufactured.

According to whether agriculture was still practised or had been superseded, the country itself could be all wild or partly wild but the city could come under one roof, a roof which could be transparent glass without any visible support. Inside, the weather need not be left to the chances of nature. It could be provided with all its varied characters of wind and rain and sunshine, according to what people liked. As people would certainly like quite different weathers, from tropical to arctic, each section of the town could have its own weather. Climatically the town would be isolated and all parts of the world would become equally habitable. The beginning of such air conditioning has already appeared in the homes and resorts of wealthy Americans.

Those who know what wealthy Americans look like at forty and how they enjoy life won't want their city roofed over and conditioned. The real frustration of science the writer has at the back of his over-cultured head is the inadequate endowment of his class.

Surely we might shut down the whole of scientific Research by his class, with the greatest advantage, for a time—if only we could devote ourselves to finding real means of so improving education as to raise the general level of intelligence—including the professorial—to the point of enabling people generally to live up to our present state of knowledge and to become better fitted to live together, with some sense of security. Personally, I am prepared to see no more crystals measured, no more crystals interpreted by X-rays, until this be done. We are spending not a penny *effectively* upon education. This way frustration lies—in no other. That the Professor can so talk is proof enough.

The Invention of Sterility, by Enid Charles, isn't worth reading about and should not have been included. The subject is one to be discussed seriously and at length, if at all. In any case, there is no frustration of knowledge about it, at present. Every chit who writes a novel has something to say on matters sexual.

Bacterial Warfare, by P. A. Gorer, is a similarly inapt subject for open discussion. The public needs useful information to-day, not to consider doubtful "ifs and whens." Should we ever get to the stage of attempting such means of killing, the only course will be for the world to rise up in its wrath and exterminate the offending nation. We have to remember, however, that in old days scurvy was the most effective agent of warfare, if not directly recognised as such. The invader cheerfully sat down before a fortress and starved it out.

The final essay by Professor Blackett, under the title, *The Frustration of Science*, at least serves as an explanation of the title attached to the volume by the publishers. Professor Blackett tells us what all intelligent people know, that a few of us have been too clever for the rest—in fact, our "damned science" has upset the world. His cure would seem to be—Socialism! He does not tell us how the blind are to lead the halt.

The one essay in the book, to which considered attention should be given, is by Professor Mottram, on *Medicine*. This is both practical and without any frills of academic rhetoric. In courageous but sober terms, he pictures the disabilities affecting the practice of Medicine, owing to our neglect to use our knowledge. He fully recognises the great service rendered by the profession but is equally alive to the faulty character of the provision now made not only for the initial education of the student but for subsequent extension of his knowledge by the practitioner:

> The whole organisation or lack of it of the recruitment of the medical profession and the training of ordinary practitioners leaves them scientifically but half-baked. It would be tolerated in no other profession.

He deplores the fads and fashions which beset medicine; also the exploitation of the field by commerce—especially the issue of misleading, often scandalous, advertisements. "While the State allows the commercial exploitation, in the form of patent foods and medicines, of medical research, that research will be frustrated." We have no social conscience in these matters—shopkeepers ever, as Napoleon said. Professor Mottram fully recognises that, in future, medicine must be mainly preventive rather than curative and that success will depend upon the proper use of food. The most glaring illustration of our failure to use knowledge is that of diet:

> Medical research has shown that the addition of a pint of milk per day to the diet of a growing boy improves the growth rate, the physique, the power of resistance to disease and the mentality, out of all proportion.

The production of milk in Great Britain, one of the best fitted in the world for dairy produce, is so badly organised that there is not one pint per head of the population even if we could get it to them. And even if we could get it to them, the majority could not afford to pay for it.

He might well have further called attention to the way in which the bread supply of the masses is now controlled by commercial combines and is little short of worthless as a nutritive food. This is a real frustration of knowledge.

One asks oneself—To what end does the Ministry of Health exist? Why carry on Medical Research, if so little use be made of the knowledge gained. In feeding ourselves to-day, we are more and more neglecting to use our knowledge—mainly because of the crass inability of the medical profession to understand the problems. I would give my entire support to Professor Mottram. I can speak with some degree of certainty, having begun my career (in 1870) in a Medical School. More than once, in this Journal, in days gone by I have deplored the inadequacy of medical training—improved though it be since I began. It will never be satisfactory, until proper foundations of scientific method have been laid in the schools and until professional training be divorced from Mumbo jumbo and scientifically associated with clinical practice from the beginning. To say, as Professor Mottram does, "that the advance of medical research is frustrated in Great Britain to-day by the parsimony of the Treasury" is, I believe, incorrect. Advance is retarded by the lack of competent leaders, of competent workers, especially by the ultra-academic character of most of the work.

On the whole, an instructive but disappointing book, owing to the narrowness of outlook of the writers. "Science" is not frustrated. Simply, we have not yet learned to use knowledge, on our own behalf. The progress we have made, apart from that which we owe mainly to one man, the great Pasteur, is all but entirely the outcome of mechanical invention. Agriculture is where it was—"science" has but explained its ancient methods and proved their efficiency. Schoolmaster-intelligence has been lowered rather than improved. Machinery alone has been developed and is more and more displacing man. The Press does not help. *Nature*, for example, notwithstanding its motto,[1] tends more and more to advocate the mechanical rather than the spiritual. Commerce has seized upon machinery, this being within the orbit of its special and very limited intelligence. We have taken no thoughtful in-

[1] 'To the solid ground of nature trusts the Mind that builds for aye'; this it has abandoned in the current volume.

terest in ourselves. Man is the most difficult of all machines to understand: being never a fixed quantity. Still, it were time that we began to study him and tried to work him effectively. For the time being, I would put all academic research aside—excepting that directed, firstly to the discovery of rational methods of education; secondly, to an understanding of food and its production; thirdly, to the proper utilisation of our power supplies, the Sun in particular in agriculture.

To avoid misunderstanding, let it be said: there is no desire on the part of sensible people to see the scientific worker in the place of the politician; their wish is merely that government be conducted with knowledge and understanding—scientifically.

At the moment, as this is written, everything is being done to foment the thought of war; not a word is being said as to our use of peace, so-called—that we are at commercial war in every direction. Germany, behind the scenes, is quietly but steadily organising her forces to social ends: her steel output went up by nearly 60 per cent. last year; she is drilling her youth into obedience. Japan is methodically arming herself industrially for a great Western attack. We remain the gullible nation that we ever were—although the most level-headed and unemotional on the face of the earth. We will not use our intelligence on our own behalf. We are allowing our youth of both sexes to grow up with the cigarette as an appendix; content to spend its leisure in the picture palace and in gladiatorial contests upon the roads: even a Lawrence, a highly vaunted leader, is overcome by the mania of speed and is killed; the art of education is nowhere considered. Is this "Science Progress"? What does it all mean? While head-hunting is going out in the Solomon Isles, Mr. Eden & Co. would seem to be contemplating its return to Europe. Are no triumphs to be won by improving the uses of peace? On Saturday, May 25, I was present, when my brother-in-law was buried from Hodeslea, Eastbourne, the house Huxley built for himself on his retirement, whence he issued his anti-clerical fulminations and other essays, over forty years ago —a great self-erected monument. I bethought me what his blazing wrath would be to-day at our ever-continued neglect to organise for social ends. Are men such as Carlyle, Ruskin, Charles Kingsley and he never again to arise among us? Is the scientific fraternity ever to remain without thought of self-improvement—merely content to provide means for man's self-destruction? Only Russia seems to have thought the "research" worth entering upon with forethought.

REVIEWS

MATHEMATICS

Solid Geometry. By L. Lines, M.A., B.Sc. [Pp. xx + 292, with 160 figures.] (London : Macmillan & Co., Ltd., 1935. 6*s*.)

The scope of this book is best indicated by the titles of the various chapters. The first seventy pages deal with the elementary geometry of parallels, perpendiculars and solid angles and are followed by chapters on surface areas and volumes, centroids, rabatments, polyhedra, semi-regular and star polyhedra, space-lattices, sphere-packs, patterns and crystals.

To judge by its contents, the book will be of most value to students of crystallography, for whom it should form a good introduction to the subject. The preface states that the book " is intended primarily for the use of pupils preparing for one of the Higher School Certificate examinations." It would have been better if the author had told us the main subject of examination. As far as mathematics, physics and chemistry go (the reviewer is not familiar with the requirements of mineralogy), not much beyond the first half of the book will be needed for the H.S.C.

So far as the book is addressed to mathematicians, some grounds for criticism are as follows. The author evidently sets himself a fair standard of rigour, as shown by his use of the limit concept. Some of the definitions might then have been framed with more care or else an explicit acceptance of intuition been made. For example, the definition of a surface as " a boundary separating one portion of space from a neighbouring portion " is unsatisfactory to anyone at all familiar with space-structure. That the truth of Desargues's perspective triangle theorem in three dimensions should be thought to infer " as a limiting case " the truth of the theorem in a plane will not do. The definition of magnitude of a figure (*e.g.* area, volume) relative to the concept of " element " needs to be made clearer so that the same magnitude will result from any mode of specifying the elements.

Much ground is covered although the book is not large, and this is achieved by conciseness of argument. There is an elaborate notation classifying the theorems, worked and ordinary examples. At the end there are a set of H.S.C. questions (apparently none on the later part of the book) and a section giving answers and hints to all the problems, which are numerous and not very difficult. The printing is well done and a special feature of the book is an abundance of clear and helpful diagrams.

J. W. A.

PHYSICS

An Introduction to Atomic Physics. By JOHN THOMSON, M.A., D.Sc. [Pp. ix + 228, with 4 plates and 36 figures.] (London: Methuen & Co., Ltd., 1935. 10*s*. 6*d*. net.)

"THE aim of this book," writes the author, "is to provide students with a concise logical account of the fundamental facts and theories of atomic physics." To the present reviewer it appears rather that the student has been furnished with a formal presentation of theories and an abstract discussion of the principles involved in a certain number of fundamental experiments, without even the illusion of examining for himself the facts—not infrequently difficult of mutual reconciliation—which these experiments provide. For there has been a rigid censorship of data: "the purpose of introducing each new fact . . . being to strengthen . . . the chain of reasoning"—so much is openly admitted. It is obvious that the chief concern has been for the logical neatness of the account.

Yet all this is not to criticise the theoretical portions of the book: these the student will find clearly and concisely developed, and for those whose knowledge of the experiments is fairly wide it will be convenient to have the relevant theory gathered together within a reasonable compass. But they must definitely turn elsewhere for further information concerning methods of experimenting and the data obtained.

Unfortunately for accuracy those data which are included are sometimes misleading, or wholly erroneous. Thus the discovery of He^5 is regarded as accomplished fact (p. 28), the disintegration of boron by α-particles is wrongly represented by equation 5, p. 39 (the disintegration of beryllium by protons, after equation 3, p. 39, has yet to be established), a completely impossible transformation scheme (p. 194) shows the derivation of protoactinium through uranium Y (emission of a β-particle increases the mass number by one unit); it is stated that ionium is the one radioelement for which the half value period is not known (p. 195) and the Geiger-Nuttall relation is incorrectly stated (equation 5, p. 200). A footnote (p. 39), "According to the latest theory, the neutron is the elementary massive particle, and the proton is complex," appears as an ill-advised amendment to the text, whilst the statement "the atomic numbers of the isotopes of copper are 63 and 65" (p. 33) is, to say the least, unfortunate.

Finally, be it recorded without criticism, certain variants of spelling and vocabulary occur which are not to be found in the *Oxford English Dictionary*—or, if found, then with usage indicated as "Scotland only."

N. F.

Magnetism and Matter. By E. C. STONER, Ph.D. [Pp. xvi + 575, with 87 figures.] (London: Methuen & Co., Ltd., 1934. 21*s*. net.)

SINCE the author wrote his first book entitled *Magnetism and Atomic Structure* some eight years ago, such revolutionary changes in our outlook on magnetic problems have taken place that *Magnetism and Matter* is almost an entirely new work. It is of signal importance both to teachers and students, as, firstly, it provides a valuable outline and guide to the literature of modern experimental methods for the determination of magnetic susceptibilities, and, while this outline may be considered not over-critical or complete, it serves its purpose. Secondly, the main purpose of the book lies in the description of modern ideas on the theory of magnetism in such a way that a non-specialist

reader can hope to gain a considerable knowledge of the subject. How often does one hear the phrase "Utterly unintelligible" applied to a learned mathematical-physical treatise by persons who are by no means without honour in the realm of physics? That phrase cannot be applied to this book. It is most definitely an intelligible and readable book, for Stoner takes the extraordinary welter of information which constitutes the basic facts of experimental magnetism, and he shows in what way they are explained by modern theory; his rôle is essentially that of an interpreter.

At the present time the worker in magnetism must be fully prepared to acquire the jargon of modern spectroscopy. To understand the modern treatment of diamagnetism he must know something about the evaluation of screening constants; to understand paramagnetism he must be fully conversant with the significance of wide and narrow multiplets. All these matters are discussed by Stoner in an illuminating manner, but in no branch of the subject is the author's power of exposition more valuable than in ferromagnetism. Here he is able to present a quantum theory discussion which is quite adequate for the large majority of readers. He also gives a sufficient description of the work in very strong magnetic fields, and concludes his book with a chapter on metals and alloys, which, useful as it is, one hopes he will enlarge in a future edition—or in a future new work, if the present rate of progress is maintained!

This is most definitely a book to be purchased and read. It is most heartily recommended.

L. F. B.

The Diffraction of X-Rays and Electrons by Amorphous Solids, Liquids, and Gases. By J. T. Randall, M.Sc. [Pp. xii + 290, with frontispiece and 197 figures, including 30 plates.] (London: Chapman & Hall, Ltd., 1934. 21*s*. net.)

It would not be correct to say that this book fills a long-felt want, because the need, although there, had neither been felt nor was it capable of being filled. But now the time has come, and the book is of very great value, because it shows what has been done in extending our knowledge of the structure of solids to that of liquids, by indicating the gaps yet to be filled and the methods by which they will be filled. After an outline of the theory of diffraction of X-rays by minute crystals and the elementary principles of X-ray crystallography, the diffraction of X-rays and electrons by gases and vapours follows. The next part deals with liquids, and later sections deal with gases and "amorphous" solids, organic fibres and surface structure. The last chapter discusses the transition solid-liquid with special reference to liquid crystals.

The book is an admirable survey of the field and is to be recommended to research workers particularly. It is excellently illustrated.

A. S. C. L.

Introduction to Atomic Spectra. By H. E. White, Ph.D. International Series in Physics. [Pp. xii + 457, with 261 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1934. 30*s*. net.)

Although there are several well-known text-books in English which devote a considerable part of their space to spectroscopy, there is no completely special-

ised treatise available, with the exception of Pauling and Goudsmit's "Structure of Line Spectra" and one or two lesser works. There was therefore an undoubted need for a really comprehensive and up-to-date account of the subject, and it can be said at once that that need is admirably met by the volume under review. Dr. White is well known as one of the leading research workers in the field of atomic spectra, so that one is entitled to expect from him an informing and authoritative treatment of his theme. Such expectations are amply fulfilled. In fact, the quality of his achievement is such as will arouse real enthusiasm in those who are in a position to appraise it.

Two outstanding virtues may be mentioned. First, the method whereby the wave-mechanics theory is approached and illuminated by means of the earlier quantum conceptions. It may be longer and less satisfactory logically than the direct approach, but most students will find it an easier road to travel. Secondly, the numerous reproductions of actual spectra which are included. After all, these do represent the foundations of the whole edifice of spectroscopic knowledge, yet how seldom does one get a glimpse of them in the average text-book, apart perhaps from the traditional print of the Balmer series. They must have cost the author a great deal of trouble, since most have been specially photographed, but the trouble was well worth taking. The "pictures" are undoubtedly one of the most valuable features of the book. These, together with a generous provision of diagrams and numerical data, securely anchor the ship of theory to the rock bottom of fact, and will render the book invaluable to theoretician and experimentalist alike.

It is unnecessary to detail the contents, since practically the whole field of pure atomic spectroscopy is covered, but one chapter, that on hyperfine structure, merits special mention as constituting the best account of the subject which is to be found anywhere.

The volume is admirably produced and misprints are extremely few. On p. 3 the H and K lines of Ca^+ are referred to as the H lines, and a pseudo-compensation is attempted by labelling them both K in the reproduction of Rowland's map immediately below. On p. 57 there is an obvious error in the value of the reduced mass of the electron, but no other blemishes of even this degree of triviality have been detected. It is safe to predict that "White" will take its place immediately as the standard text-book of line-spectroscopy, and it is a pleasant duty to offer thanks to the author and congratulations to the publishers on this addition to a notable series, since it cannot fail to enhance the already high reputation enjoyed by the latter.

W. E. C.

Applied Acoustics. By HARRY F. OLSON, E.E., Ph.D., and FRANK MASSA, B.S., M.Sc. [Pp. xiv + 430, with 228 figures.] (Philadelphia: P. Blakiston's Son & Co., Inc., 1934. $4.50.)

WHILE the work of Rayleigh is still the classical reference for almost all fundamental problems in acoustics yet the thermionic valve has opened up such entirely new practical possibilities that extensions of this classical theory have become inevitable.

Such extensions as have been made are for the most part to be found only in scattered journals. But improvement in laboratory equipment and technique has resulted in great strides in applied acoustics, and in order that intelligent and productive research may be stimulated it is essential

that the underlying theory be made available in book form. Such are the considerations that have prompted the writing of this book.

With such an end in view the authors have wisely refused to water down their introductory treatment in the vain attempt of making it capable of assimilation by those of poor physical equipment or of weak mathematical digestion. The presentation presupposes a familiarity with the principles of elementary physics and a knowledge of electric circuit theory. And a development of the fundamental three-dimensional wave equations of hydrodynamics precedes a consideration of their implications and limitations in simpler problems. A complete mastery of this fundamental mathematical theory is not, however, essential in order to derive benefit from succeeding material. The subject matter relating to applied acoustics is introduced by a consideration of the analogues between electrical, mechanical and acoustical systems, so that physical and mathematical treatment go hand in hand.

The range of the book is wide and the treatment thorough. Microphones, telephone receivers, loud speakers are dealt with seriatim and in considerable detail both as to their constructions and underlying principles and also from the point of view of calibration and testing. In the case of microphones some account is given of the authors' own work on the pressure gradient ribbon type of instrument.

There are useful sections on Architectural Acoustics, Noise Measurement, and Physiological Acoustics.

The Book is well printed and illustrated, and can be confidently recommended as accomplishing what it sets out to do, *viz.* to show the relation of modern developments and problems in applied acoustics to the fundamental theory of dynamical systems.

G. A. SUTHERLAND.

Lighting Calculations. By H. H. HIGBIE. [Pp. xii + 503, with 115 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 31*s*. net.)

THIS volume is a very welcome addition to the literature of illuminating engineering. Each of the ten chapters is devoted to one of the different branches of the subject, *e.g.* the calculation of direct illumination from point sources, the calculation of average illumination by means of utilisation coefficients, the economics of light production by various sources, etc., etc. In each chapter the author first sets out the general principles and shows how the relevant formulæ are derived; he then illustrates his treatment by means of a number of fully worked examples; finally he gives an extensive series of problems in the solution of which the student learns how to apply the knowledge he has acquired from reading the text and studying the examples. Many of the problems are based on statements made either in books or in published papers written by illuminating engineers. For instance, one of the problems given in Chapter IV (Brightness) is stated as follows: "Trotter, in his *Illumination*, 1911, p. 23, gives the brightness of the sun as 800,000 candles per square inch, and the moon as 2 in the same units. From data on sizes and distances that you can find in any encyclopædia or text-book on astronomy, calculate what must be the mean reflection factor of the moon's surface, neglecting corrections for light absorption in the earth's atmosphere." There is no doubt that the book will receive a warm welcome, not only from those responsible for courses of instruction in illuminating

engineering, but also from individual students. In future editions the space devoted to street lighting and similar calculations might well be extended, but apart from this the book is well balanced. The proof-reading has evidently been done with great care and there is a good index.

J. W. T. W.

Torsional Vibration. By W. A. Tuplin, M.Sc. [Pp. xviii + 320, with 89 figures.] (London: Chapman & Hall, Ltd., 1934. 21*s*. net.)

Although the internal-combustion engine is now generally accepted as a familiar characteristic of the times in which we live, the difficulties associated with its vibrations are less generally appreciated. Commonly enough, it is assumed that those difficulties have been "overcome" in the march of progress, and may be forgotten with "last year's model." In fact, however, the same difficulties remain as potent as ever, setting limits to progress in design and to safety in operation; and although they may be evaded with skill and experience, they can never be wholly eliminated.

Torsional vibration in particular remains a problem of special practical importance, being responsible for certain "critical" speeds at which engines run "roughly" or, more commonly, cannot be run without immediate danger; and it is remarkable also for another important characteristic—it lends itself to clear mathematical presentation. Not only on account of its intrinsic importance to mechanical engineers engaged in design, but also as a logical subject of interest to the greater number of engineers concerned with the running of engines of all kinds, it is well suited for inclusion in the courses of study of technical colleges.

Mr. Tuplin's volume can be recommended to all who propose to study torsional vibration in internal-combustion or other engines. The treatment is clear and readable, and is developed in a simple mathematical manner that is well within the scope of students likely to take up the subject. The opening chapter of thirty pages gives an excellent revision of natural and forced vibrations with and without damping in the simplest of cases; and later chapters are devoted to other cases more representative of practical examples. Chapter 9 on harmonic analysis leads to the study of forced vibrations in multi-crank engines—which is well developed with numerous examples to illustrate the relative importance of major and minor critical speeds. Chapter 13 deals with vibration dampers, and other chapters with special couplings and with the torsiograph. Throughout the work, the author relies on the principle of reducing complex systems to simpler combinations of flywheels with massless shafts, except in one chapter only where the mass is treated as distributed along a "heavy" shaft. It seems perhaps a little remarkable that the excellent alternative method in which the masses of cranks are regarded as distributed is not more generally employed; but it is believed that the author's treatment is probably more acceptable to a large number of readers.

The book is well suited to the needs of students who wish to make a careful logical study of a branch of engineering of great practical importance, and the numerical examples are presented in detail in a manner that will assist those engaged in practice.

B. P. H.

Radio round the World. By A. W. Haslett. [Pp. vii + 196, with 7 plates and 22 figures.] (Cambridge: at the University Press, 1934. 5s. net.)

During the past decade numerous books on radio have been published, but nearly all have dealt with the design, construction or operation of transmitting or receiving stations. These matters are not mentioned in the volume under review; instead the author gives a clear and concise account of other aspects of radio communication which are equally interesting.

The first half of the book is devoted to a general account of the properties of electromagnetic waves and the propagation of radio waves through space. The chapters dealing with the discovery and investigation of the Heaviside and Appleton Layers are particularly clear and up to date. A good account is also given of the correlation found to exist between sunspot-activity, magnetic storms and atmospherics.

The remainder of the book contains chapters devoted respectively to the properties and possibilities of ultra-short- and micro-waves, to television and to the uses of radio in medicine, war, weather forecasting and direction-finding. The treatment throughout is non-mathematical and the book can be strongly recommended to all who require an accurate account of these fascinating branches of the subject.

C. W. O.

Automatic Protection of A.C. Circuits. By G. W. Stubbings, B.Sc., F.Inst.P., A.M.I.E.E. [Pp. viii + 293, with 209 figures.] (London: Chapman & Hall, Ltd., 1934. 15s. net.)

The book may be regarded as dealing with three main divisions of the subject, namely, the principles underlying automatic protective apparatus, the application of these principles to practical protective systems, and the testing and maintenance of protective apparatus.

The first division gives an excellent account of the theory of protective apparatus and the treatment and diagrams should certainly enable the student of this subject to obtain a clear understanding of it. With reference to Fig. 53, a point that should be corrected in the next edition is that under the condition of a fault between two phases of the protected feeder the resultant secondary voltage is the sum of the two secondary voltages concerned, which are in phase opposition and are not displaced 120° as stated in the explanation of the figure.

The subject matter dealt with in what has been referred to as the second division of the book is naturally more controversial, because it includes points that depend upon the practical operation in service of various protective systems installed by different manufacturers. In some cases names of firms have been given when describing apparatus manufactured by them, whereas in other cases the names have been omitted; a more uniform treatment would have been preferable. Further, the omission of the names under which certain protective systems are universally known and the substitution of theoretical names makes it difficult for the practical engineer to recognise easily the systems with which he is familiar. For example, the protective systems illustrated in Figs. 164 and 167 are known as the "Merz-Beard" and "McColl-Bias" systems respectively. The book describes briefly and in an interesting manner many protective systems in everyday use; obviously many systems had to be excluded or dealt with peremptorily on account of

space limitation but, notwithstanding this, certain modern protective systems might with advantage have been included or treated more fully, and the "Interlock" and "Ratio-Balance" systems used on the British Grid come within this category. Further, the description of the "Split-Pilot" system shown in Fig. 169 omits any reference to its important feature of electrical tuning, and this is also omitted in the subject matter under the paragraph heading "Tuned Relays" on page 167. Incidentally, Fig. 117 illustrates what is known in America as a "Ratio-Differential" relay, and the reference in the title to "Ratio-Balance" is an error which may easily confuse the student who is not familiar with the actual "Ratio-Balance" protective system.

The subject matter in the third division is a useful compilation of methods for testing and maintaining protective apparatus.

The book presumably has been written for the student who wishes to make himself familiar with the theory of automatic protective apparatus rather than for the practical protective gear engineer who is more particularly interested in protective systems and their rating. The author has been successful in achieving the aforementioned object, whilst the practical engineer will also obtain useful information from the book, particularly from the chapter on Testing and Maintenance.

B. H. L.

CHEMISTRY

The Ideas of Physical Chemistry. By H. McKay, B.Sc., and H. A. C. McKay. [Pp. x + 301, with 8 plates and 87 figures.] (London: William Heinemann, Ltd., 1934. 7*s*. 6*d*. net.)

This book forms an excellent introduction to the recent developments in physical chemistry. Its aim is to present the modern theories of the subject in language that a layman can understand. This the authors make a valiant attempt to do, and they are to be congratulated upon the measure of success they have attained. The style is simple, technicalities are avoided, and the book is absorbingly interesting. It is doubtful, however, whether the aim can possibly be fulfilled in a book of three hundred pages. A certain amount of physical and chemical knowledge on the part of the reader must be assumed. The book will therefore find its greatest use amongst those who have lost touch with the development of physical chemistry, those students of chemistry who wish to prepare themselves for a more advanced study of the subject, and those students of other sciences who require a working knowledge of the basis of chemistry.

Among the topics dealt with are the structure of the atom, leading up to the various types of chemical linkage, and their explanation on the theory of wave mechanics; molecular structure, reaction velocity, catalysis, electrolytes, and colloids. There is a useful vocabulary of chemical terms. A brief account of the work of Svedberg on the molecular weights of proteins might have been given in the chapter on "Giant Molecules," or possibly when dealing with colloids.

The book must not be regarded as a text-book. If it is true to its aim it is bound to be somewhat too superficial for that; but to anyone who reads it, whether he be layman or student, it will most certainly prove a stimulant to further study.

A. J. M.

Colloidal Electrolytes : A general Discussion held by The Faraday Society, September, 1934. [Pp. iv + 421, with frontispiece and numerous figures.] (London : Gurney & Jackson for the Society, 1935. 18*s*. 6*d*. net.)

THIS volume, reprinted from *The Transactions of the Faraday Society*, comprises the papers and discussion of the Third Colloid meeting arranged by the Colloid Committee of the Faraday Society.

In the opening address, Freundlich points out that the term colloidal electrolyte is a general one and refers to colloidal systems which exhibit Tyndall scattering, dialysis, etc., but which also possess a well defined electrical conductivity. To the soaps—the classical example of colloidal electrolytes—may therefore be added long chain sulphonic acids and amino compounds, dyestuffs containing SO_3H and NH_2 groups, proteins, salvarsans, certain starch compounds and even silicates and silicic acid. Such a variety of substances implies a wide field, with the result that no less than thirty-six papers were contributed to the discussion.

The first part of the discussion is devoted to theory and here there are accounts of the application of the Debye-Huckel theory, the molar (micellar) mass, electrovalency of ions and osmotic pressure of colloidal electrolytes and the application of Gibbs's equations to protein systems. This is followed by a very short section on experimental technique.

In view of the great importance of colloidal electrolytes in technology, much of the discussion deals with soaps, dyes and proteins. It is, however, too detailed to mention more specifically in this review.

In so far as the general reader is concerned, there are no convenient summarising introductions to the separate parts, no doubt owing to the nature of the subject, but which are, none the less, a most valuable feature of many Faraday Society discussions. The book will therefore appeal more particularly to those directly interested in the field of colloidal electrolytes.

It would be an improvement if the volume were bound in somewhat stiffer covers.

H. W. MELVILLE.

Chemische Stofferklärung. By Dr. W. TOMBROCK. Translated from the Dutch into German by Dr. J. GROSSFELD. [Pp. 30, with 13 figures.] (Leipzig : Otto Hillmann, 1934. RM. 1.)

THE author of this essay regards the Bohr theory of the atom as illogical and irrational, and proposes to replace it by a theory in which the properties of the elementary particles are in no way different from those of matter in bulk. The elementary particles are protons, electrons, and ether atoms. The ether is regarded as a gas, made up of atoms, smaller than the electron, moving at great speed. Protons are ether cyclones. Using these assumptions, the author explains weight, which is due to ether pressure, the dual nature of light, the electric field, the periodic system, and the structure of the atom. The atom does not consist of a planetary system, but is steric.

To the reviewer, the assumptions of the Bohr theory are considerably more palatable than those of Dr. Tombrock. Although the author claims that it is logical to assume the existence of ether particles, one finds this more difficult to believe than the theory of stationary states. Moreover, judging only from a reading of this essay, the theory of Dr. Tombrock would be

incapable of giving the many quantitative results which have gone so far to impress scientists with Bohr's theory.

A. J. M.

Chemical Engineering Plant Design. By F. C. VILBRANDT, Ph.D. Chemical Engineering Series. [Pp. x + 341, with 53 illustrations, including diagrams and flow-sheets.] (London and New York: McGraw-Hill Publishing Co., Ltd., 1934. 24*s*. net.)

THIS book, which relates to plant design as distinguished from equipment design, should be particularly welcome to advanced students of chemical engineering, for whom it is primarily intended. It gives clearly and in considerable detail an outline of the basic principles which underlie and govern the development of a process from the laboratory stage, through the pilot plant stages, to the commercial unit, together with the economic considerations involved, due emphasis being laid upon costing as an all-important factor in plant design. The fact that the purely production side forms but a part of the organisation of a manufacturing concern is sometimes insufficiently realised by those immersed in laboratory and process details, with the result that the need for an intelligent appreciation of the many other factors involved is frequently overlooked. To such persons also the book should prove of great benefit, as well as to those occupying administrative and executive positions in the chemical industry who lack a chemical engineering education.

The author's main object is to indicate how the relevant data may be correlated to form a workable basis for the design of plant for the production of a chemical commodity in a commercially feasible manner. In addition to such technical matters as flow of material and energy, plant location, foundations, waste disposal, selection and specification of equipment, industrial buildings, and plant layout and assembly, considerable space is devoted to pre-construction cost accounting. Consideration is also given to such general economic factors as market survey, finance, etc., as well as to health, safety and legal factors. Sound advice is offered in connection with the preparation of working drawings which the novice, as well as those more experienced in design, would do well to remember. The appendices include some useful physical data and conversion tables, and references have been provided as a guide for collateral reading.

Despite the general treatment of the subject, the book covers a wide and useful field, and may be recommended as an excellent companion volume to those which deal more specifically with actual unit operations and processes employed in the chemical industry.

H. W. CREMER.

GEOLOGY

The Changing World of the Ice Age. By REGINALD ALDWORTH DALY. [Pp. xix + 271, with 149 figures, including 8 plates, and 29 tables.] (New Haven: Yale University Press; London: Humphrey Milford, 1934. 22*s*. 6*d*. net.)

IN this important addition to glacial literature that should be in the hands of all geologists and scientifically minded laymen Professor Daly presents his Yale University Silliman Memorial Lectures of 1934 in an expanded form.

The book is profusely illustrated, and with its valuable maps and tables usefully summarises much of our present knowledge of ice-caps past and present and of the late glacial events in North America and Europe, including the kaleidoscopic succession of lakes and seas which came into existence in these regions on the melting of the ice. The book deals chiefly with the effects of the Pleistocene glaciation and deglaciation upon sea-level all over the world and with the behaviour of the crust on the imposition and the dissolution of the ice. Professor Daly modifies and amplifies his earlier views in certain important respects. He replaces, for example, the " bulge or wave " movements due to viscous flow at shallow depth by a new " punching hypothesis," according to which the flow at a depth of *c.* 1200 kilometres was linked with shearing stresses in the crust and a punching of this down or up along a sheath of circumferential fractures coincident with the well-known " hinge-lines." He also seeks to explain the alleged world-wide sinking of the sea-level in recent times not by a growth of the polar ice-masses as previously assumed, but by a belated, plastic adjustment of the earth's body, caused by the weight of the water piled up on the suboceanic crust when the last ice melted. He restates his " glacial control theory " and corrects certain misrepresentations of it made by the late Professor W. M. Davis, but approaches the views of his critics by granting that the chilling of the tropical seas was too small to have had any effect on the coral growth. Professor Daly's more speculative views, based upon close inductive reasoning, are both stimulating and suggestive. They are, however, not always convincing, and, while likely to be provocative of much fruitful discussion, will probably have to be abandoned or considerably modified.

J. K. C.

Structural Geology. With Special Reference to Economic Deposits. By B. Stočes and C. H. White. [Pp. xv + 460, with frontispiece and 663 figures.] (London : Macmillan & Co., Ltd., 1935. 25*s.* net.)

This excellent work is an elaboration of a book by Dr. Stočes which has already been published in Czech and German. Dr. Stočes is Professor of Geology in the Czechoslovakian School of Mines and is an authority on ore-deposits and engineering geology. Dr. C. H. White, the eminent American mining geologist, has re-written Dr. Stočes's book, thus making it available for English-speaking geologists and in doing so has rearranged and greatly expanded its contents.

The main emphasis is naturally on geological structure with particular reference to the occurrence, arrangement and methods of exploitation of valuable mineral deposits. All aspects of economic geology, including not only metalliferous deposits but also coal, oil, water, and problems of mining, quarrying and tunnelling which are dependent for their solution on the study of rock-structures, have thus been illumined.

The text is divided into two parts dealing respectively with the primary structures of rocks (sedimentary and igneous rock structures), and induced or secondary structures due to orogenic movements of the earth's crust. Under the latter heading folding, faulting, joints and unconformities are dealt with. There are also good sections on surveying and mapping, on geophysical methods of prospecting for minerals and on the influence of structure in mining practice.

A notable feature of the book is the number, excellence and unhackneyed character of the illustrations. The great majority of the figures are drawn from Czechoslovakian sources although there are not a few of the beautiful Geological Survey photographs from Great Britain; and thus geological maps, sections and photographs of geological features from a country hitherto unfamiliar to British and American workers have been brought into general circulation, and have made available to teachers and students of economic geology some novel material.

A few misprints and mistakes have been noted. "Haug" is misspelt "Haugh" (p. xi); "Gleam Einich" (Fig. 101) should be "Gleann Einich"; in Fig. 45 the conventional stratigraphical and lithological signs have been disregarded and conglomerate is represented by the ornament commonly used for limestone; and in Fig. 125 the "series of basalt flows," Dickson Bay, Spitsbergen, are almost certainly sediments.

This book may be warmly commended to both teachers and students of applied geology.

G. W. T.

BOTANY

An Index to the Genera and Species of the Diatomaceæ and their Synonyms. Parts XVI, XVII, XVIII, XIX, XX and XXI, 1934–5. Compiled by F. W. Mills, F.L.S., F.R.M.S. [Pp. 1162–1726.] (London: Wheldon & Wesley, Ltd., 1934–35. 10*s*. each.)

It is gratifying to find that the later parts of Mills' Index are practically free from the numerous typing errors that occurred in the first part issued. This decided improvement enables one to form a juster estimate of the enormous task that the author has undertaken and of the immense value of his compilation to all who are concerned with Diatoms. Mr. Mills has successfully accomplished a very difficult piece of work and one which will rank as a very definite contribution to the advancement of science. From this point of view one cannot but regret that the work is not printed, since the rather feeble reproduction of certain pages leaves one in doubt as to the degree of permanence of the copy.

The parts under review complete the genus *Navicula* and, apart from a number of smaller genera, include *Nitzschia*, *Pinnularia*, *Pleurosigma*, *Rhizosolenia*, *Stauroneis*, *Surirella*, *Synedra* and *Triceratium*. The final part contains some supplementary matter.

F. E. Fritsch.

Everyday Botany. By L. J. F. Brimble, B.Sc. (London and Reading). [Pp. viii + 589, with 340 figures.] (London: Macmillan & Co., Ltd., 1934. 7*s*. 6*d*.)

Applied Science has its origin in great part in pure science and owes much to it, but pure science also stands to gain from applied science. As Mr. Brimble very truly remarks the applications of Botany, although very important, meet with scant treatment in most elementary text-books of the subject, being at the most concerned with a few horticultural processes; while, especially of recent years, text-books on elementary chemistry and physics give a considerable amount of space to the applications of these sciences.

Our author seeks to provide an elementary work which stresses the applied sides of the subject and also introduces a certain amount of human interest in another way by touching lightly on its historical aspects. The result is certain to prove of interest and especially to the type of student who is often repelled by a study of its purely academic aspects. The book seems to attempt a higher standard than the author claims for it. The illustrations are numerous, and with few exceptions suitable to the text. The meaning of Figure 79 on p. 96 is not clear and a few illustrations have a little too much detail and shading for the scale on which they are reproduced, *e.g.* the Primrose on p. 391. The marram leaf should close upwards not downwards as suggested on p. 243. The history of the subject could obviously not have been treated in detail, and this leads at times to a slightly wrong perspective. The appendix, consisting of a list of well-known scientists, scarcely seems a useful or desirable thing for students. Suggestions for practical and field work are appended to the chapters. The book is a very interesting one and can be strongly recommended.

E. M. C.

The Genetics of Garden Plants. By M. B. CRANE, Pomologist, and W. J. C. LAWRENCE, Curator, the John Innes Horticultural Institution, Merton. With a Foreword by Sir DANIEL HALL, K.C.B., LL.D., D.Sc., F.R.S. [Pp. xvi + 236, with 53 figures and 42 tables.] (London: Macmillan & Co., Ltd., 1934. 10*s.* 6*d.* net.)

As stated in the preface to this book the authors' aim is twofold—"to give an introduction to the essential principles of genetics and cytology" and "to give an account of recent results in relation to horticulture." The first project, in any case not an easy one, is here made difficult by the condensation necessitated by the space at their disposal and also by the wide circle of readers aimed at. The latter difficulty especially seems to have been a cause of embarrassment. In trying to simplify they at times do not give quite the correct idea. For example, on p. 2 we are told that the higher plant arises from the union of two germ-cells—the pollen and ovules (the annotation here *re* ovules only causes more confusion in the mind of the reader. On p. 19 two illustrations are given and the legend states that there is a chiasma in each figure, but this would seem to be true of the second only. Viviparous is defined as producing young in a living state, and the definition of diploid parthenogenesis on p. 38 is not clear and seems incorrect. Apart from these and similar instances of vagueness the account is a good introduction to genetics and cytogenetics.

The special portion of the book, however, is quite a different matter. Here the authors have given us a careful, authoritative and critical account of the genetics of horticultural plants. Stress is laid on the protective influence of cultivation, which it is claimed preserves good varieties rather than improves bad or indifferent ones. It is also pointed out that in horticultural work sterility may not be the drawback it at first sight would seem. The practical applications of results as well as their theoretical implications are dwelt upon and the last few chapters on incompatibility, sterility and the origin of new forms will be found of special interest. The book is well produced and illustrated and the price is a very reasonable one.

E. M. C.

Gardening in East Africa. By Members of the Kenya Horticultural Society and of the Kenya and Uganda Civil Services. Edited by A. J. Jex-Blake, M.D., F.R.C.P. [Pp. xvi + 330, with 6 coloured plates and 6 figures.] (London, New York, Toronto: Longmans Green & Co., 1934. 12*s.* 6*d.* net.)

This manifestation of the enterprise and enthusiasm of the gardening devotees of Kenya is, as Sir Arthur Hill points out in a foreword, "a testimony to their unbounded faith, constant hope and deep love for their plants."

Mr. Walter contributes an account of the climatic conditions, which is illustrated by twenty-one useful graphical presentations of the seasonal distribution of rainfall in various parts of Kenya, from the coast to the Great Lake, and for five stations in Tanganyika, three in Northern Rhodesia and three in Uganda. The cultivator has to contend with the two seasons of drought on the one hand and the periodic torrential rains on the other.

The nature of the soils and their cultivation is the subject of a chapter by Mr. Beckley and other chapters treat of the various classes of horticultural plants, annuals, perennials, roses, flowering trees and shrubs, climbing and bulbous plants. Others with lawns, vegetables, fruits, pests and diseases.

Some idea of the difficulties encountered in garden culture may be obtained from the fact that such plants as Sweet Peas, Lobelia, Mimulus and Nemesia can only be grown at altitudes above 6,000 ft., Lupins above 7,000 ft. and Sweet Williams above 8,000 ft. Wallflowers, although they will grow at the lower altitudes, will only flower at the higher.

The whole forms a work which should prove very useful to gardeners in these areas, and there is also much of general biological interest.

E. J. S.

The Diseases and Curing of Cacao. By H. R. Briton-Jones, D.Sc., Ph.D., D.I.C., A.R.C.S. [Pp. x + 161, with 37 figures.] (London: Macmillan & Co., Ltd., 1934. 10*s.* net.)

This book is the outcome of a suggestion made at the Imperial Mycological Conference in 1929 that a series of handbooks should be prepared on the diseases of the major tropical crops. The author has had much experience of cacao cultivation in the West Indies, and the book contains a full account of the chief diseases of the crop there, in South America and in West Africa. The book, which is well illustrated, is primarily written for Agricultural Officers and Planters, but some parts of it will be of great interest to plant pathologists in general, notably the author's views on the incidence and control of root diseases. Professor Briton-Jones advocates the growing of cacao under the most favourable agricultural conditions as being the best means of avoiding serious disease, maintaining that if optimum conditions are provided for the crop, disease will not generally assume large proportions. Such an ecological standpoint will commend itself to plant pathologists, although it must be remembered that certain epidemic diseases of more valuable crops can only be controlled by specific treatment. Cacao is mostly cultivated by small proprietors and it is impossible economically to spend much on disease-control.

A valuable chapter at the end of the book describes "The Preparation or Curing of Cacao." This is an account of the various methods in use for the preparation of the cacao beans of commerce. The biochemistry of these

processes is discussed at some length and the author indicates where further knowledge is needed.

The book contains a bibliography on cacao fermentation and a general bibliography of 192 titles, but as the references are not numbered in the text one does not know sometimes which of a number of papers by an author is under consideration. Several papers included in the bibliography are not mentioned in the text.

There are some misprints: *e.g.* on p. 52 "Went" should be "Rant," and the genus *Trachysphæria* should be *Trachysphæra*.

F. T. B.

The Science of Rubber (Handbuch der Kautschukwissenschaft). Edited by PROF. DIPL.-ING. K. MEMMLER, Director of the Staatliche Materialprüfungsamt at Berlin-Dahlem. Authorised English Translation edited by R. F. DUNBROOK, Ph.D., and V. N. MORRIS, Ph.D., of the Research Staff of the Firestone Tire and Rubber Co. [Pp. xvi + 770, with 7 plates and 213 figures.] (New York: Reinhold Publishing Corporation, 1934. $15.00.)

THIS work is a composite production which has arisen out of a book entitled *Der Kautschuk und seine Prüfung*, published by the Editor jointly with the late Prof. Dr. W. Hinrichsen in 1910. When the publishers approached him regarding a new edition the idea occurred to him to extend the scope of the book and to produce a comprehensive review of the entire field of rubber science; he therefore called in the assistance of a number of collaborators, each of them specialists in some branch of the subject, and as a result produced the present volume, not as a second edition but as the first edition of a new publication. The translators' preface opens with the expression of opinion that although good books have been written in English on Plantation Practice and on the Technology of Rubber no book in English covers the subject of rubber so fully from the scientific point of view; it was thought that scientists and technologists would welcome a really comprehensive work in English covering not only latex, crude rubber, the chemistry of rubber, vulcanisation and the analysis of rubber, but also microscopy and pigment dispersion, the physics of rubber and the recently developed equipment for physical testing of rubber. The editors have been assisted in their translation by ten members of the Firestone Research Laboratories and others outside, and have endeavoured to supplement the original work with translators' notes covering newer work since the German text was written, on such subjects as synthetic rubber, direct utilisation of latex, X-ray examination, American physical testing apparatus and recent theories of vulcanisation. The value of the book has furthermore been greatly enhanced by the inclusion of a very comprehensive bibliography. From what has been said it will be seen that the book goes far towards meriting the title of an encyclopædia of rubber. The Editor in his preface to the original German edition states that the technology of the preparation of rubber goods and factory practice was omitted on the ground that this did not fit into a book devoted essentially to the scientific aspects of rubber and that books dealing with this subject adequately existed already.

To give an adequate account of the contents of this very valuable book would require considerably more space than is here available; suffice it to say that all tastes have been catered for, both specialist and other. Some

idea of the ground covered can be obtained from a recital of the main headings. Thus following an Introduction comes a section on "Botany, Cultivation, Collection and Preparation of Rubber." This contains a complete list of commercially exploited rubber-producing plants with detailed description of their chief characteristics and figures illustrating their morphology; included among these plants is the latest recruit in the form of the species of Scorzonera, which is being cultivated in Soviet Russia. This section also contains an account of the anatomy of laticiferous tissues leading on to the physical characteristics of latex as well as to its physiology; then follow methods of tapping, application, coagulation and conversion into crude rubber. The next big section is entitled the Chemistry of Rubber, including the molecular size and constitution of rubber as well as the synthesis of artificial rubber. The Vulcanisation of Rubber deals with the methods and theory of vulcanisation and with the reclaiming of vulcanised rubber. The remaining sections are entitled Chemical-analytical Testing Methods, Physics of Rubber, Physical Testing Methods and Microscopy of Technical Vulcanizates. Each section being written and translated by experts is written in an authoritative and exhaustive manner, and the result is a most valuable and reliable up-to-date treatise. The bibliography covers some 60 odd pages and there is a very complete author and subject index which adds greatly to the utility of the book, which may be confidently recommended to all who are interested in the subject of rubber in its manifold aspects.

P. H.

ZOOLOGY

A History of Embryology. By Joseph Needham, Sc.D. [Pp. xviii + 274, with frontispiece and 40 figures, including 15 plates.] (Cambridge: at the University Press, 1934. 15*s*. net.)

It is difficult in a short notice to do justice to a work such as this, which embodies research covering a very wide field and making formidable demands on the learning and experience of the author. The first two chapters deal with Embryology in Antiquity and from Galen to the Renaissance, and the last two trace the history of Vertebrate Embryology in the seventeenth and eighteenth centuries. The work concludes with a section of a general and philosophical nature. It is evident that much thought and labour are brought to a focus in this treatise, and many of the conclusions reached are not only important but new. We propose, however, to take the merits of the work for granted—they are sufficiently obvious and considerable—and to comment only on that aspect of it which admits of improvement. We do this in the hope that the next edition may be made even more valuable than is the present one.

Experience extending over many years teaches us that one thing essential in the technical equipment of the historian of Science is a detailed knowledge of the history of the *literature* as distinct from the history of the *phenomena* of Science. Ignorance or weakness in matters bibliographical recoils heavily on the head of the offender, and he finds himself guilty of errors which are not pleasant to remember. It is true that these slips are found in the work of the most careful scholars, who, however, may be trusted to avoid the more obvious pitfalls. Mistakes unhappily seem to be inevitable. The simple rule that the *first* relevant edition is the one which should be most carefully studied, and later editions examined *only* for new matter, is fre-

quently ignored, but rarely with impunity. In the work under review many corrections are necessary under this head alone. Bibliographical research is laborious but not difficult or even boring. The investigation of a problem is accompanied by all the attractions of the chase, which make the solution of these puzzles a fascinating as well as a profitable pursuit.

In reading Dr. Needham's book it became clear that the text should be critically worked over from the bibliographical point of view before the next edition is printed. Admitted that the bibliography covers some fifty pages of print, and that there is abundant evidence that these books have been personally examined, the fact remains that the handling and integration of this vast body of literature have not been completely mastered. One date in the text, another in the bibliography, both being wrong, simply should not be. There is only space to quote a few representative examples. The statement on p. 107 that "it is one of the puzzles of this period why Harvey made no mention of it [Highmore's *Treatise on Generation*] in his work," becomes less of a puzzle if Highmore's work was published *after* Harvey's, as it was. On p. 112, after pointing out that Sir Thomas Browne refers to Harvey's work on Generation in the *Pseudodoxia*, although the latter was published in 1646 and Harvey's work in 1651, Dr. Needham asks: "Did Browne see manuscript or proof sheets? Does any other evidence exist that Browne was in close touch with Harvey?" Again, the mystery vanishes when it is discovered that the reference to Harvey occurs for the first time in the *third* edition of the *Pseudodoxia* published in 1658 [also in the *Hydriotaphia* of the same date]. On p. 215 it is stated that the automatic microtome was invented by Threlfall and others "about 1860." Threlfall was born in 1861, nor is the date of the invention in any doubt. The automatic microtome was first suggested by Threlfall in 1882, the first instrument was made in 1883, and the Cambridge "Rocker" dates from 1885. On p. 159 the question of the demonstration of the independence of maternal and fœtal circulations is thus decided: "F. Hoffmann, 1681. (He proved the point by injection long before Hunter, who is stated by Cole to have been the first to demonstrate this.)" Here Dr. Needham is confusing two persons of the same name. He quotes the father instead of the son, the work selected has no embryological interest and is moreover of the wrong edition, and the statement that the discovery in question was attributed to Hunter by Cole is also incorrect. We owe the first complete demonstration of the vascular arrangements in the placenta to Monro primus (1734), but he was partially anticipated by F. Hoffmann the younger in 1718. In Dr. Needham's bibliography the date of Redi's work on the Generation of Insects is given as 1688 instead of 1668, but even this slip does not explain the statement on p. 129 that "Harvey's scepticism about spontaneous generation antedated by nearly a century the experiments of Redi." Only seventeen years actually separate the publication of these two works. A lapse of another and less excusable type is the reference to Leeuwenhoek on p. xvii, where the following sentence occurs: "The time-killing dilettante, almost philatelic, quality of Leeuwenhoek's investigations is, as Becking says, too obvious to be overlooked." No one who was really familiar with Leeuwenhoek's researches could possibly have described them in such terms, nor is Becking an authority to be quoted in this connection. The statement by Dr. Needham on p. 190 that Bonnet (1779) discovered parthenogenesis in Aphids illustrates this very point. Bonnet's work was first printed in 1745—long after Leeuwenhoek had published a circumstantial and remark-

able demonstration of parthenogenesis in Aphididæ in a series of letters between 1695 and 1702.

A critical examination of the list of "Books not consulted" (p. 223) which "have proved quite unobtainable in any library, and for the most part are not mentioned in any of the principal Bibliographies," and concerning which Dr. Needham would be "extremely grateful for any information," is only another illustration of the justice of the criticism which has here been stated. A student working far from a great University Library would have no difficulty in solving all the problems in this list, nor would he even be tested by them. Authors' names misprinted, wrong attributions, wrong dates, and failure to consult classic sources of information readily account for every item. Most of these works are, in fact, in the British Museum, and some are in the Bodleian, Edinburgh University and doubtless other libraries. The number of printed scientific works which cannot be seen or traced is so small that it is wise to hesitate before ignorance is admitted.

F. J. C.

Sea Trout of Lewis and Harris. By G. HERBERT NALL, M.A., F.R.M.S. [Pp. 71, with map and 6 plates.] (Edinburgh: H.M. Stationery Office, 1934. 4*s*. net.)

THIS is the fourth of the Salmon Fishery papers of 1934 published by the Fishery Board for Scotland, and in it Mr. Nall continues his long series of scale studies of Scottish sea trout. He has produced already two papers on the sea trout of South Uist (Howmore and Kindonan) and a short paper which may be regarded as preliminary to this. He is able, therefore, to make useful comparisons.

The details of the many small river systems found in the Long Island are not to be understood easily unless through personal experience or a careful study of the map, but many of the streams and lochs have been famous for their sea trout for a long period. It is found that the fish here make a good start in life and at the time of migration as smolts are quite up to the mainland standard, but that in their sea life they do not grow quite so fast as do either the quick growers of the mainland, or the fish of South Uist nor are they quite equal to North Uist fish at Loch Maddy or to certain of the Orkney sea trout. It is noted, however, that as many of the fish were of the poor run of 1933, it is possible that full justice may not be apparent in the rather restricted number of samples (1813 in all, spread over three years). At the same time there is a difference noticeable in the age at which most of the sea trout smolts migrate, the proportion of three-year-olds being less than elsewhere.

In the rivers of the mainland, a decline in the numbers of sea trout has been remarked for some years. In the streams under review the catch is by rod alone, so that the possibility of over-netting does not arise. An interesting record of totals covering a period of sixty-one years from North Harris, shows an increase of salmon (most of which are grilse) and no very evident sign of a scarcity of sea trout. Many have stated that it exists but Mr. Nall suggests that a rhythm or periodic rise and fall is possibly the explanation. A most careful paper, and a full statement of detail as is always the case with the author. A short paper by Mr. MacFarlane, the Assistant Inspector of Salmon Fisheries, on some salmon scales is included.

W. L. C.

Proceedings of the Third International Locust Conference, London, September 18, 1934. [Pp. 179, with 2 maps.] (London: H.M. Stationery Office, 1934. 3*s*. 6*d*. net.)

The *Proceedings of the Third International Locust Conference* consist of an account of the meeting of the delegates in London, of the resolutions which were passed, and twenty-two appendices which consist partly of short papers dealing with particular aspects of the locust-problem, partly of summaries of existing research-programmes.

Now that the international nature of the locust-problem is clearly realised and research in different countries is being co-ordinated, it is becoming increasingly necessary for the various research-workers to have certain basic assumptions in common and to employ a common terminology. The main purpose of the various resolutions which were passed by the conference was to provide such a background to all locust-research, at least in Africa and Western Asia. In some respects the advantages of conformity are obvious, as when a set of symbols is put forward for recording locust-migrations. But some doubt may be felt as to the wisdom, at this stage, of fixing the lines of all research on locusts. Actually the resolutions are mostly so broadly drawn that almost any sort of research might be included under them, but, as a matter of principle, the question is worth some consideration. The danger of "legislating" for the future in this way appears particularly in relation to Resolution 3 (*e*) which deals with the standardisation of biometric methods. The ratios of certain linear measurements of the locusts are important characters of the phases. It is laid down that in future all such ratios should be calculated as the ratio of the means of the two sets of linear measurements, rather than as the mean of the separate ratios. While the former method is economical of arithmetical, it is very doubtful if ratios obtained in that way have any statistical validity, except with very uniform stock and with very large samples.

However, the real purpose of the resolutions is probably more to set a minimum standard for research than to insist on a sterile uniformity.

Several of the appendices contain interesting new material. Appendix 7 consists of a valuable summary and analysis of the basic problems (*i.e.* the problems presented by the swarming habit) by Mr. B. P. Uvarov. Appendix 8 by Mr. H. H. King deals with the recent application of aircraft delivering sodium arsenite dust to the destruction of flying swarms. A full account of the recent experiments is given but it is too early to come to any conclusion as to their value. It has at least been proved that locusts falling to the ground during the experiment contained appreciable quantities of the arsenite. In Appendix 9, Dr. T. J. Naudé suggests a modification of the experiments, *viz.* that swarms should be dusted in the early morning, when they are stationary. This would eliminate some of the difficulties raised by scattering poisoned locusts all over the countryside.

O. W. Richards.

MEDICINE

The Biochemistry of the Eye. By A. C. Krause, M.A., Ph.D., M.D., F.A.I.C. [Pp. xvi + 264, with 17 figures.] (Baltimore: The Johns Hopkins Press; London: Humphrey Milford, 1934. 15*s*. net.)

The second of a series of monographs on ophthalmology—a volume dealing with the Biochemistry of the Eye—published by the Wilmer Ophthalmological

Institute of the Johns Hopkins University, keeps up the high standard set by the first which dealt with immunology as applied to diseases of the eye. The present volume is written by Arlington C. Krause, whose intensive researches at this Institute in this particular branch of the subject have put him in a position of no inconsiderable authority to write such a book. For over a century sporadic studies have been carried out on the chemistry of the tissues of the eye, but until recently the results had risen little beyond the level of isolated and unrelated observations which, for lack of coherence, carried with them little value and less meaning. During the last twenty years, however, the position has greatly changed, and a new era of intensive and systematic investigation has introduced a voluminous literature in which, unfortunately, chemical facts have been too liberally clothed in theories and speculative hypotheses. To a large extent this may be due to the fact that much of the experimental work has been done by workers whose training has been clinical rather than chemical, and who have allowed enthusiasm to outstep the limits usually set by long training in the colder and more logical atmosphere of the laboratory. A review of the literature at the present time is therefore more than usually useful, and, although perhaps sometimes complete success may not have been attained in winnowing the wheat from the chaff, the present volume is on the whole so good that minor criticism seems out of place.

Most of the book is, of course, a collection of somewhat unrelated facts: and it is astonishing, viewing these facts presented together, how little one knows of their ultimate collective significance. The eye is an organ designed for a very specialised function, and its tissues present physico-chemical properties differing widely from other organs of the body. Moreover, many of these tissue-constituents are very complex, and the problems in thermodynamical and colloidal chemistry which they raise are unfortunately imperfectly understood in the region of pure chemistry, a fact which makes their elucidation in a complex biological system very difficult indeed. But when it is remembered that all the major problems of ocular pathology are essentially physico-chemical—such as cataract, glaucoma, the etiological factors of detachment of the retina, corneal opacities, and so on—it is apparent how important and immediately practical is this young and relatively unknown branch of science. On it will depend most of the revolutionary therapeutic advances of the future, and a book such as the present fulfils a very important function both as an incentive to further work, in so far as it demonstrates our essential ignorance, and as a ready reference to, and a review of the work which has been already done.

S. D.-E.

Psychology and Health. By H. Banister, M.Sc., Ph.D. [Pp. viii + 256.] (Cambridge: at the University Press, 1935. 7*s*. 6*d*. net.)

Dr. Banister's book is intended to provide medical students and practitioners with sufficient knowledge about the workings of the human mind to enable them to exercise a favourable influence upon the psychological processes of patients. The author, who is Lecturer in Experimental Psychology at Cambridge University, has not allowed the preoccupations of the laboratory to prevent his acquiring considerable experience in clinical psychology. This experience, combined with wide reading in psychopathology, has enabled him to produce a well-written volume, which should prove useful to such

general practitioners as are prepared to accept the authority of a lay psychologist. The subjects dealt with include the psychological problems of the healthy and the psychological phenomena complicating physical disease as well as the frank psychoneuroses (the psychoses are wisely omitted from discussion). The psychotherapeutic techniques of suggestion, hypnosis and "analysis" are also described. As regards analytical therapy, the rival theories of Freud, Jung and Adler each receive a chapter; and these three chapters possess the rare merit of providing concise and accurate accounts of the rival theories without critical comments. The theories are thus allowed to speak for themselves. Unfortunately, however, the author's description of the psycho-analytical technique is not only misleading but inaccurate. It is also regrettable that the author should employ the term "neurasthenia" as a synonym for "anxiety state," since the two terms have been commonly used to denote distinguishable conditions. Dr. Banister's own theory of the psychoneuroses is that they are due to the influence of childish and ill-adapted sentiments. The psychotherapeutic method he favours is a modified analytical technique directed towards the dissolution of ill-adapted sentiments and the creation of more satisfactory ones. Of its kind the book is good, but it is a question whether there are not already too many of the kind.

W. R. D. F.

Your Meals and Your Money. By Gove Hambidge. [Pp. xvi + 190, with many charts and illustrations.] (New York and London: Whittlesey House, McGraw-Hill Book Co., Inc., 1934. 6s.)

It is a pity that the Ministries of Health and Agriculture and Fisheries are at opposite ends of Whitehall. Could we get them physically and spiritually together and thrust this book into their hands we might stimulate them to a planned agriculture leading to a planned national nutrition.

The dietician says that healthy diet must be based on dairy foods, market garden produce and (fat) fish. The agriculturists (Sir Daniel Hall and A. G. Street, for example) say that our agriculture should be based on dairy and market-garden produce. For heaven's sake, why not do it? We know what is happening to the herring trade. This book is a welcome aid to those who believe in planning for the health of the nation and should be read by every sociologist, M.O.H., dietician, welfare worker, domestic science teacher, etc. It is especially recommended to politicians who can read American and think.

V. H. M.

HISTORY OF SCIENCE

The Doctor in History. By Howard W. Haggard, Associate Professor of Applied Physiology in Yale University. [Pp. xiii + 408, illustrated.] (New Haven: Yale University Press; London: Humphrey Milford, Oxford University Press, 1934. 17s. net.)

This is essentially a history of medicine or, as the author prefers to call it, "a history of health," although most, if not all, of the book is taken up with an account of man's struggle against disease. Indeed, it is really "a history of ill-health," beginning not with man's ailments, but with the diseases of the dinosaurs and sabre-toothed tigers. Written for the author's children

and obviously intended for the general reader, it traces the work of the physician and his ally, the surgeon, from the time of the primitive medicine-man of pre-history right down to the present day. It includes very interesting chapters on Imhotep, Æsculapius and Hippocrates, on medicine under the Egyptians, Greeks and Arabs and during the Crusades, on the Black Death and the Dancing Mania, on Paracelsus, Vesalius and Paré, on the rise of modern medicine under the influence of scientific discovery, on the introduction of European medicine into America, on medicine in prisons, barracks and war, and on the laboratory fight against disease.

The author has produced a very well-written book. It differs considerably from other histories of medicine in that it does limit itself to medicine without dragging in the discoveries of half a dozen other sciences in order to make a history. Everywhere full account is taken of the conditions of life in the various periods studied, which makes it an extraordinarily readable book. It contains over a hundred illustrations and is packed with interest: we read, for example, that mediæval undergraduates groaned under the restrictions against bringing "bean shooters" into lecture-theatres and against "heckling the public hangman in the execution of his duty," and that the average length of life in those days was eight years, an obvious excuse for a little gaiety with "bean shooters" and the hangman. The still current mistake about John Mayow appears here once more; and the author apparently supposes that "oil of bricks" (the distillate from olive oil, into which red-hot fragments of brick had been cast) was "oil squeezed from bricks" (p. 268), an under-estimation of the intelligence of the alchemists.

D. McKie.

MISCELLANEOUS

Confessions of a Scientist. By Raymond L. Ditmars. [Pp. xiv + 241, with 23 plates.] (New York and London: Macmillan & Co., Ltd., 1934. 15*s.* net.)

Dr. Ditmars writes with an engaging simplicity about the everyday affairs of his life, so that as one reads one lives it with him. And what an amusing life it is! First he is off to Panama, to collect venomous snakes; then again for tarantulas; then he is bringing back to New York the only true vampire bat ever exhibited in a Zoo. Late at night, he sees his captive stalk silently, like a big, four-legged spider, across the floor of its cage to a dish of blood and lap it with the tongue—not suck it up as is generally supposed. He films these things and prints excellent pictures from the film in the book. Then he rushes off to the Berkshire Mountains to catch rattlesnakes, and returns to his desk in the New York Zoo, where we watch him interviewing reporters, trying not to accept people's pets for the collection, or called away to some cage or other to deal with a crisis. We stand beside him as he lectures, to a school perhaps or on a rolling ship, and shows living animals on the lecture bench which are always, apparently, on the verge of escaping into the audience. We go with him into his private cinema studio where he is making a collection of animal-pictures—here, among other adventures, a mamba gets loose among the overhead wires and pipes and is only recaptured with great difficulty. And so on, to the end of an unpretentious and very entertaining volume. Lovers of Frank Buckland and Charles Waterton will like Dr. Ditmars too.

G. P. Wells.

Race and Culture Contacts. Edited by E. B. Reuter. [Pp. viii + 253.] (London & New York: McGraw-Hill Publishing Co., Ltd., 1934. 18*s*. net.)

Professor Reuter has here selected from the communications presented to the 28th Annual Meeting of the American Sociological Society fourteen papers which are likely to appeal to a wider public than that to which they were addressed originally. The subject propounded as the focussing point of the meeting was racial and culture contact. In dealing with this topic the papers included in this volume fall into two groups. All, with one exception, to which must be added an introductory chapter by the editor, are descriptive and historical; but those which fall in the first group are general in scope, while those of the second are concerned with problems which specifically affect the United States. Thus in the first group Mr. R. O. McKenzie in his "International Expansion and the Interrelation of Peoples" deals with the beginnings of cultural and racial conflict and at the same time provides something of a historical background for the other papers composing the volume. Mr. Robert E. Parks rapidly surveys conditions in three continents where backward and advanced peoples meet, incidentally providing those who are interested in conditions in South Africa with food for thought. Mr. Max Hardman, in dealing with social adjustments of conflicting racial elements, gives an instructive analysis of conditions in Transylvania. English readers, however, may, perhaps, turn with greater interest to the second group in which the material analysed will be less familiar. Here attention may be called particularly to papers which deal with the family organisation and mental attitude of the present-day negro population in the southern States by Messrs. Franklin Frazier and C. Johnson respectively and those on the remarkable absence of race prejudice in Hawaii of which the characteristics are described and the origins traced by Messrs. Romanzo Adams and A. W. Lind.

E. N. F.

Creation and Evolution in Primitive Cosmogonies and other pieces. By Sir James Frazer, O.M., F.R.S., F.B.A. [Pp. ix + 151.] (London: Macmillan & Co., Ltd., 1935. 8*s*. 6*d*. net.)

It is a great pleasure to be able to read yet another book from the pen of Sir James Frazer, and one too of a more intimate character than some of his larger works. All except one of the essays have appeared before, but in a less accessible form. The eight fall into well-defined groups, the first from which the book takes its title shows Sir James as an anthropologist, two short chapters form his last tribute to two distinguished colleagues, Sir Baldwin Spencer and Canon Roscoe. But, as we have learnt long ago, Sir James covers in his scholarship a wider field than anthropology and both Gibbon and Condorcet are treated with an urbanity which many attempt but few succeed in achieving. A short notice on Mediæval Latin fabulists serves to remind us of the author's achievement in the field of Latin and Greek. Finally come two papers—the last hitherto unpublished, the first his speech on receiving the freedom of the City of Glasgow—which add that intimate character to the volume which has been mentioned above. In these papers Sir James tells us of his early life, his struggles and ambitions, and of the help which he received from his parents at the beginning of and later in his

career. His debt is our debt, for it is to Sir James that most of us who are anthropologists owe our original inspiration to undertake this study.

L. H. D. B.

Sa Torreta. By MARGARET A. MURRAY, D.Lit., F.S.A. (Scot.), F.R.A.I., Fellow of University College, London. With Chapters by T. BALAKRISHNAN NAYAR, M.A., and JOHN CAMERON, M.D. Cambridge Excavations in Minorca, Part II. [Pp. 86, with 50 plates and 4 figures.] (London: Bernard Quaritch, Ltd., 1934. 20*s.* net.)

THIS book, which forms Part II of the Excavations in Minorca, is a worthy successor to the first part, Trapucó, which appeared in 1932. The archæological work is described with Dr. Murray's characteristic care and thoroughness and throws further light on the curious rude stone structures of the Balearic Isles. Very special attention may be directed to the report on the human remains, and it is difficult to praise adequately the care lavished both by the excavator in rescuing the much-comminuted fragments and by Dr. Cameron in studying them. The report includes a detailed study of the skulls and of the other bones of the skeleton. The writer has not only described the racial characteristics of the people, but has also attempted to explain the causes of such phenomena as *platymeria* and *platycnemia* which have excited the attention of anthropologists ever since Manouvrier, following the work of Busk and Falconer, devoted considerable attention to the matter. There can be no doubt, as Sir Arthur Keith states in his introductory note, that the people buried in the naveta were of the same racial stock as the early Maltese. The method of burial also is similar. We have in both cases a confused medley of human bones, presumably the remains of many interments, probably a charnel house, not dissimilar to some which still are in use in certain parts of the Mediterranean. Both groups of people belonged to the same division of humanity. Your reviewer had the opportunity some years ago of studying the early Maltese remains in detail, and this identification seems certain. The difficulty comes in when one attempts exactly to define which group of humanity they do belong to. While it seems reasonable to suggest that they are a branch of the Mediterranean stock, they differ from most accepted examples of this stock, except such series as an early Predynastic group from Egypt examined by Fouquet, in the great length of the head, which comes well within the "Nordic" range, from which division they are however separated by the small cranial breadth. They differ from the Neolithic peoples of the British Isles, which Dr. Cameron himself has elsewhere stated that he believes to be of Mediterranean affinities, in their generally smaller size. This is not the place to discuss these interesting problems in detail, but they cannot be passed over, more especially since a careful monograph of this type is invaluable in providing material for the subsequent study over a wider area of the difficult questions of the racial affinities of the early peoples of the Mediterranean and their relationship both to their successors, such as the modern Maltese, and to such early peoples elsewhere, as the Long Barrow men of the British Isles. Such detailed anthropological studies are particularly valuable when they are so clearly documented archæologically as they are in this volume.

L. H. DUDLEY BUXTON.

An Introduction to Theory and Practice of Psychology. By Ll. Wynn Jones, M.A., Ph.D. [Pp. x + 308, with 35 figures.] (London: Macmillan & Co., Ltd., 1934. 12*s*. 6*d*. net.)

Psychology, says Dr. Wynn Jones, is now an independent science based on principles of its own; it has become a separate branch of knowledge, of the utmost importance to the doctor, the teacher, and the man of business. Accordingly, he has written a text-book from which physical, metaphysical and physiological questions are excluded, and in which psychological facts and theories are described in purely psychological terms. The whole work is based on lectures given to students of medicine, education, and industrial psychology; and is admirably adapted to the needs of the practical student.

The volume begins with an exposition of Professor Spearman's fundamental hypotheses; and ample space is devoted to an exposition of Professor Spearman's work. But the views of all the modern schools are touched upon—behaviourism, Gestalt psychology and (a little more briefly) psycho-analysis.

Throughout, the treatment is highly original, and unlike that of the ordinary manual of psychology. With his declaration that "experimental psychology without theory is futile, and that general psychology without experiment is sterile" every psychologist will agree. And, in accordance with this principle, he chooses as the starting-point for almost every chapter some concrete mental process taken from everyday life and reduced to experimental form. Thus each topic is at once made living, interesting, and real; and the student is led to deduce his own conclusions from objective facts studied at first hand. In addition to the usual themes, such problems as those of the psychology of wit, of music, and of art have chapters to themselves. Abstract and difficult matters like psycho-physical and statistical methods are relegated to the end. The book is at once scientific and versatile, and forms a most useful and readable introduction to the whole subject.

C. B.

Statistical Methods for Research Workers. By R. A. Fisher, Sc.D., F.R.S. Fifth edition. [Pp. xvi + 319, with 12 figures and numerous tables.] (Edinburgh and London: Oliver & Boyd, Ltd., 1934. 15*s*. net.)

Professor Fisher is to be congratulated. In 1925 he produced the first edition of the present work. As was to be expected, it was entirely novel, presenting as it did statistical methods by example and precept, rather than pages of mathematical formulæ. The book is now in its fifth edition, which may be taken by the editors of the series of Biological Monographs and Manuals, and by the author, as sufficient justification for their initiative in the first instance.

Professor Fisher still maintains, in the fifth edition, that he is right in refusing to bow to the critics who want mathematical proofs. Those who are desirous can consult the original papers, to which exhaustive references are given; the book is mainly intended for research workers in the biological and kindred sciences, who can apply themselves to an understanding of the principles underlying the methods described, and subsequently use the methods themselves on their own data. It is inevitable, of course, that workers in other fields, who do not understand the technical problems and

terms of the biologist and geneticist, find some difficulty in appreciating the application of the methods to some of the examples given in the book, and may, in consequence, introduce errors when they use the methods to aid them in the interpretation of their own results.

The author and "Student" may be considered to have opened up new avenues of research in statistics, and have had a great deal of influence on modern methods of handling statistics, especially those which emerge from laboratory experiments, where tests of significance are important. Since the first edition many of Professor Fisher's ideas, which were then considered unorthodox, are now accepted, and it is of very great importance that these should be available in the present form, not only to be appreciated and criticised by the professed statistician, but by the large number of other readers, who would perhaps lose themselves in a maze of streets of an old-fashioned mathematical town, but are able to use a by-pass consisting of clear statements of ideas.

The clear type and good paper are assets, which assist the reader in realising the "efficiency" (to use the author's word) of the book.

E. C. RHODES.

The Teaching of Chemistry. By N. F. NEWBURY, M.A., M.Sc. [Pp. xii + 247, with 23 figures.] (London: William Heinemann, Ltd., 1934. 6*s*. net.)

MR. NEWBURY has collected a great deal of valuable material not easily accessible elsewhere. He covers a wide range, his advice is always sound and practical, his comments are often illuminating and his criticisms balanced. Younger teachers will derive benefit by reading what he has to say on the organisation of the school laboratory and on the framing of syllabuses; while even the more experienced will read with interest many of the chapters, such as those on the influences of language and on the use of diagrams and models. The author has added still further to the utility of his book by paying attention to some of the objective researches which have been carried out on the teaching of Science and, also, by providing numerous references.

There is no doubt that the teaching of chemistry has suffered greatly from a method of treatment at once too formal and too abstract. The movement away from this attitude is illustrated by Mr. Newbury's insistence on the importance of basing the course on the environment and on using home-made apparatus: his concrete suggestions will be welcomed.

He has also included a chapter on the school application of small scale apparatus, which progressive teachers are beginning to use. The saving in cost together with the greater safety and neatness of these methods make them very attractive.

Mr. Newbury registers critical approval of a modified heuristic method of teaching. It is rather a pity, however, that he speaks so much of the "Heuristic Theory." It might have been preferable to insist more on the essential need for introducing into all Science teaching a definite element of enquiry, rather than warning people against following *blindly* the heuristic ideal. In any case, no one now does so, but all good teachers seem to have acquired a heuristic bias and their changed outlook may perhaps be counted as Prof. H. E. Armstrong's lasting contribution to science teaching.

The author states in his introduction that "The chief aim of this book is to give practical advice, and no attempt has been made to provide an exhaustive

study of the general philosophical, psychological and educational influences affecting chemistry teaching." Within these self-imposed limits he has produced an excellent book, which may be warmly recommended. It should be read by all who are in any way concerned with the teaching of chemistry. The publishers may well be congratulated on producing at so moderate a price so attractive a volume.

J. A. Lauwerys.

Thoughts of a Schoolmaster or "Common Sense in Education." By H. S. Shelton. [Pp. 256.] (London: Hutchinson & Co. (Publishers), Ltd, 1934. 6*s*. net.)

This book fully deserves its subtitle, and the author brings to his subject unusual breadth of experience: he was in four schools as a boy and as a master in twenty-five. Mr. Shelton is certainly not afraid of tackling controversial subjects, and he discusses at length—in a pleasant and lively style—the problems of the boarding school, co-education, subject-masters *v.* form-masters and so on. Nor does he leave us in doubt as to his own preferences—he tells us exactly why he prefers day schools to boarding schools, why schoolmasters are unpopular and why the power of headmasters should be curtailed.

Probably his most interesting theme is his considered plea for a more extensive school science; that is extensive both as regards scope and as regards time-table importance. He realises, however, that a considerable change must take place in our methods of dealing with the subject before this position can be generally accepted. Most schoolmasters will agree with Mr. Shelton that a reform of the time-table is overdue, and all of them will deplore with him the academic bias which is forced on our schools by the demands of university examinations. Again, nearly all science masters will approve Mr. Shelton's argument that it is difficult to do worth-while work in General Science unless the subject is allotted far more time. Whether classicists will agree to all this remains to be seen. The book contains, incidentally, most interesting comparisons of the position of the teacher thirty years ago and now, as well as very useful and practical advice on science teaching and on discipline. All this will be appreciated by those whose experience of teaching is not yet as extensive as Mr. Shelton's, while the others will find it interesting to compare notes with him.

The book deserves to be widely read. It is an eminently sane and reasonable contribution to the discussions which have now been taking place for some years.

J. A. Lauwerys.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

The Binary Stars. By Robert Grant Aitken, Director and Astronomer, Lick Observatory, University of California. Second edition. McGraw-Hill Astronomical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xii + 309, with 1 plate and 19 figures.) 21*s*. net.

The Emergence of Life. Being a Treatise on Mathematical Philosophy and Symbolic Logic by which a New Theory of Space and Time is Evolved. By John Butler Burke, M.A. (Dubl. & Cantab.), F.Inst.P., formerly Berkeley Fellow of Owens College, Manchester. Oxford: at the University Press; London: Humphrey Milford, 1931. (Pp. x + 396, with 4 figures.) 30*s*. net.

Intermediate Physics. By C. J. Smith, Ph.D., M.Sc., A.R.C.S., Lecturer in Physics, Royal Holloway College. Second edition. London: Edward Arnold & Co., 1935. (Pp. xii + 900, with 674 figures.) 16*s*. net. Also obtainable in parts: Properties of Matter, 3*s*. net; Optics, 4*s*. net; Heat, 4*s*. net; Acoustics, 2*s*. net; Magnetism and Electricity, 6*s*. net.

Physics for College Students. An Introduction to the Study of the Physical Sciences. By A. A. Knowlton, Ph.D., Professor of Physics, Reed College. Second edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xxii + 623, with 429 figures and 14 tables.) 21*s*. net.

An Introduction to Physical Science. By Carl W. Miller, Ph.D., Associate Professor of Physics in Brown University. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 409, with frontispiece and 194 figures.) 15*s*. net.

The Structure and Properties of Matter. By Herman T. Briscoe, Professor of Chemistry, Indiana University. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. x + 420, with 139 figures and 10 tables.) 21*s*. net.

A Textbook of Light. By L. R. Middleton, M.A., B.Sc., Physics Master, Latymer Upper School, Hammersmith. London: G. Bell & Sons, Ltd., 1935. (Pp. viii + 288, with 189 figures.) 6*s*.

Atomic Physics. By Max Born, M.A.(Cantab.), Dr.phil.(Göttingen), (Hon.) Sc.D.(Bristol), Stokes Lecturer in Mathematics, University of Cambridge. Authorised Translation from the German Edition by John Dougall, M.A., D.Sc., F.R.S.E. London and Glasgow: Blackie & Son, Ltd., 1935. (Pp. xii + 352, with 103 figures, including 8 plates, and 11 tables.) 17*s*. 6*d*. net.

The Diffraction of Light, X-Rays, and Material Particles. An Introductory Treatment. By Charles F. Meyer, Associate Professor of Physics, University of Michigan. U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1934. (Pp. xiv + 473, with 283 figures.) 22*s*. 6*d*. net.

X-Rays in Theory and Experiment. By Arthur H. Compton, Ph.D., Sc.D., LL.D., Charles H. Swift Distinguished Service Professor of Physics, and Samuel K. Allison, Ph.D., Associate Professor of Physics, University of Chicago. Second edition of "X-Rays and Electrons." London: Macmillan & Co., Ltd., 1935. (Pp. xiv + 828, with 279 figures and 120 tables.) 31*s*. 6*d*. net.

The Flow of Water through the Straits of Dover as gauged by continuous Current-Meter Observations at the Varne Lightvessel (50°56′ N.–1°17′E.). Part II: Second Report on Results Obtained. By J. N. Carruthers, D.Sc., Fisheries Laboratory, Lowestoft. Ministry of Agriculture and Fisheries, Fishery Investigations, Series II, Vol. XIV, No. 4. London: H.M. Stationery Office, 1935. (Pp. 67.) 3*s*. net.

Electrical Water Heating. With Special Reference to the Domestic Storage Heater. By D. J. Bolton, M.Sc., M.I.E.E., Philip C. Honey, and N. S. Richardson. London: Chapman & Hall, Ltd., 1935. (Pp. viii + 192, with 66 figures.) 7*s*. 6*d*. net.

The Fundamentals of Chemical Thermodynamics. Part I: Elementary Theory and Electro-chemistry. By J. A. V. Butler, D.Sc., Lecturer in Chemistry in the University of Edinburgh. Second edition. London: Macmillan & Co., Ltd., 1935. (Pp. xvi + 253, with 65 figures and 28 tables.) 7*s*. 6*d*.

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Problems in Soil Microbiology. By D. Ward Cutler and Lettice M. Crump, General Microbiology Department, Rothamsted Experimental Station, Harpenden. The Rothamsted Monographs on Agricultural Science. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. viii + 104, with 18 figures, 31 tables and a map.) 9*s.* net.

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Living Things. An Introduction to Biology. By Richard Palmer, Lecturer in Education in the University of Liverpool. London: George Allen & Unwin, Ltd., 1935. (Pp. 400, with 182 figures.) 7*s.* 6*d.*

Zoological Technique for Teachers and Senior Students. By T. L. Green, B.Sc., F.R.E.S., Lecturer in the Department of Education, University of Bristol. London: Allman & Son, Ltd., 1935. (Pp. 118, with 3 plates and 12 figures.) 5*s.* net.

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Report on the Fisheries of Palestine. By James Hornell, F.L.S., F.R.A.I., late Director of Fisheries with the Government of Madras. London: The Crown Agents for the Colonies on behalf of the Government of Palestine, 1935. (Pp. 106, with 14 figures.) 7*s.* 6*d.*

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The Arachnida. By Theodore H. Savory, M.A., formerly Exhibitioner of St. John's College, Cambridge. London: Edward Arnold & Co., 1935. (Pp. xii + 218, with 8 plates and 99 figures.) 25*s.* net.

Limnology. By Paul S. Welch, Ph.D., Professor of Zoology, University of Michigan. McGraw-Hill Publications in the Zoological Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xiv + 471, with 46 figures and 53 tables.) 30*s.* net.

Pacemakers in Relation to Aspects of Behavior. By Hudson Hoagland, Professor of General Physiology, Clark University. Experimental Biology Series. New York and London: Macmillan & Co., Ltd., 1935. (Pp. xii + 138, with 34 figures, including 9 plates.) 12*s.* 6*d.* net.

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A Source Book in Physics. By William Francis Magie, Professor of Physics, Emeritus, Princeton University. Source Books in the History of the Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xiv + 620, with 111 figures.) 30*s*. net.

The Discovery of Specific and Latent Heats. By Douglas McKie, Ph.D., B.Sc., Lecturer in the History and Methods of Science, University College, London, and Niels H. de V. Heathcote, B.Sc. With a Foreword by E. N. da C. Andrade, D.Sc., Ph.D., F.R.S. London: Edward Arnold & Co., 1935. (Pp. 155, with 6 plates and 2 figures.) 6*s*. net.

Ability and Knowledge. The Standpoint of the London School. By Frank C. Thomas, M.A., B.Sc. With a Foreword by C. Spearman, F.R.S., Ph.D., and an Appendix by Elsa Walters, B.A., Ph.D. London: Macmillan & Co., Ltd., 1935. (Pp. xx + 338, with 49 figures.) 15*s*. net.

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The Teaching of Biology. By Mary E. Phillips, B.Sc., formerly Biology Mistress, Godolphin and Latymer School, and Lucy E. Cox, B.Sc., F.L.S., formerly Lecturer in Biology, Graystoke Place Training College. With a Foreword by Sir Percy Nunn. London: University of London Press, Ltd., 1935. (Pp. viii + 155, with 4 plates and 4 figures.) 4*s*. 6*d*. net.

The Golden Science Series. Book II. By Elsie V. M. Knight, B.Sc., Head Teacher, The "Marlborough" L.C.C. Girls' School, Chelsea. London: University of London Press, Ltd., 1935. (Pp. 254, with 4 coloured plates and 193 figures.) Limp cloth, 2*s*. 6*d*.; cloth boards, 2*s*. 9*d*.

This English. By Sir Richard Paget, Bart. With a Preface by R. R. Marett, D.Sc., F.B.A. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1935. (Pp. xii + 118.) 4*s*. 6*d*. net.

SCIENCE PROGRESS

SOUND AND NOISE

By G. W. C. KAYE, O.B.E., M.A., D.Sc.

Superintendent, Physics Department, The National Physical Laboratory; Chairman of the Ministry of Transport Noise Committee

Not many years ago the physical study of sound received scant attention from the majority of physicists as something of no great practical significance. While it is true that the late Lord Rayleigh gave much attention to the subject, bringing to bear, as always, a profound analytical ability and a genius for simple and precise experimentation, the general outlook on sound did not materially change until recent times. Wireless, broadcasting, telephony, the gramophone, and architectural acoustics now provide outstanding illustrations of the degree of significance which the subject has since acquired to the civilised world. These developments are largely bound up with research developments made possible by the replacement of the sensitive flame and the Helmholtz resonator by the microphone, the thermionic valve, and the electrical resonating circuit. Sound has simultaneously expanded its terminology, a glossary of which has been recently completed by a Committee of the British Standards Institution.

Sounds and noises are of course made up of mixtures of pure notes of various intensities and frequencies. The quality of a sound is bound up with its composition, the low notes usually giving volume, while the high notes give character (just as they do in speech) and are the ones by which we identify sounds. The extreme frequency range of the ear is from say 20 to 20,000 cycles per second, the upper limit declining substantially with advancing years. Present-day technical acoustics confines itself, however, to a narrower range, from say 50 to 10,000. Sounds also possess physiological and psychological characteristics, pitch, timbre and loudness being essentially subjective features.

It may here be remarked that the composition of a sound when

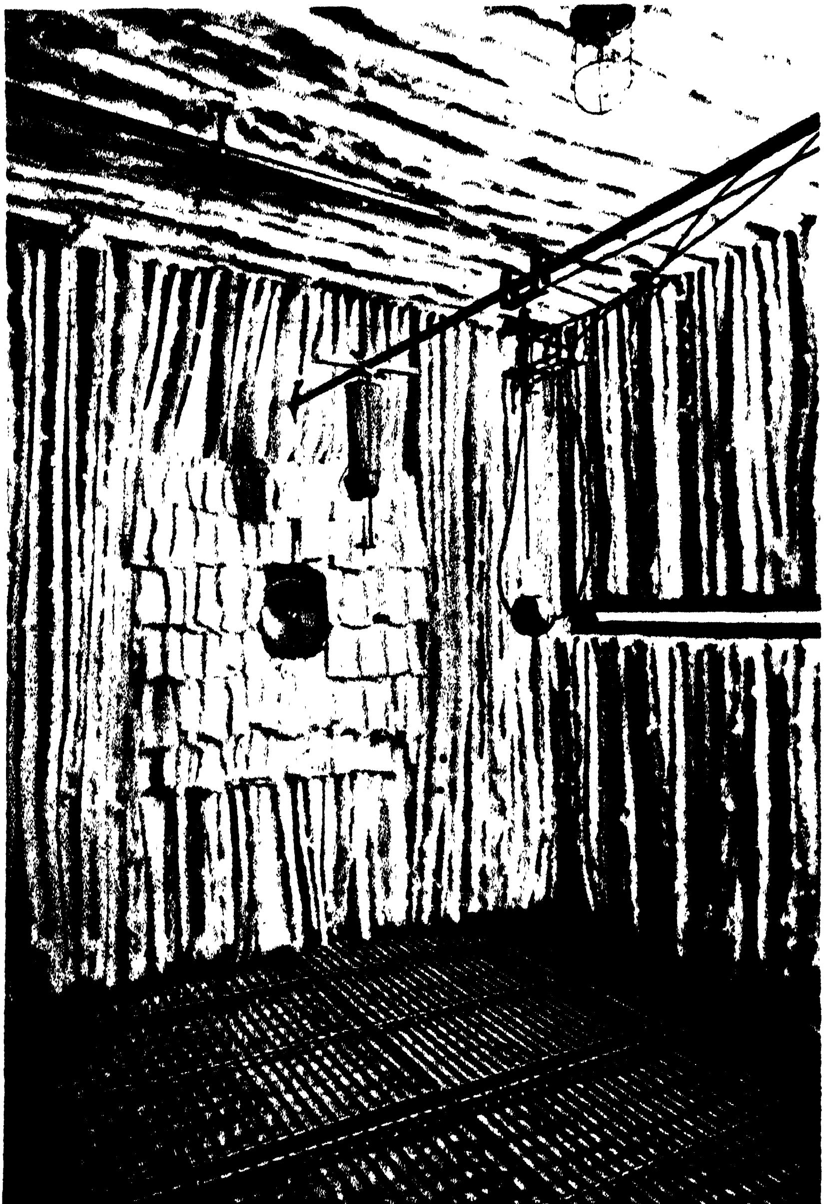
it reaches the hearer may differ appreciably from that which it had to begin with. For example, the high-pitched components of traffic noises may be reflected by hedge-rows or trees while lower notes pass through or round, so that, to a limited extent, trees may form a useful muffler of street noises. Furthermore the high-pitched components of a sound may be abnormally absorbed by passing over different types of ground, or by the air itself if it is humid. Knudsen has shown that the nitrogen in the air plays no part in this, but that the effect is due to interaction between the oxygen and water molecules. He estimates that if we lived in an atmosphere of oxygen with a humidity of about 20 per cent., the high notes of the violin and piccolo would be completely inaudible 50 yards away. Incidentally he obtained similar abnormal absorptions of sound when the water was replaced by alcohol, benzene, acetylene, hydrogen, or sulphuretted hydrogen.

Sound and Noise

Dictionary writers and others have long endeavoured to preserve a distinction between sound and noise, but mostly from a psychological point of view. Any sound made by somebody else is liable of course to be regarded as objectionable, but usually the listener is much influenced by background, environment, or force of association. Even weak sounds, if they are not familiar or not understood, may excite fear, especially if they are ambiguous in direction. Sudden sounds which are needlessly loud, excite annoyance at the unreasonableness and the unjustifiable intrusion. On occasion, sounds not in themselves particularly objectionable, may mask or obscure sounds which are important or are of interest and so engender a feeling of irritation or stultification. Again the sheer monotony of the sound of an electrically maintained tuning fork may become exasperating to a degree—be the note ever so pure.

It is perhaps not safe to generalise in matters of psychology, but in developing the notion that noise is sound which is unwelcome, we must take cognizance both of the hypersensitive to whom noise is an abounding source of irritation and of those at the other extreme who seem entirely immune to noise and cannot tolerate reasonable silence. It is plain, however, that the appraisement of sound as noise is a purely personal matter and that a collection of people are not likely to agree among themselves that a particular sound is a common nuisance. The British Standard definition of noise—"sound undesired by the recipient"—epitomises the matter; and a definition on these lines will probably be accepted internationally.

PLATE I



Heavily lagged Room at the National Physical Laboratory for fundamental standardisation of microphones. The "lagging" of the room consists of a foot or more of eel grass and cotton wadding. The object is to try to prevent reflexion of sound from the walls of the room, so that measurements may be made in a calculable sound field.

The Measurement of Sound and Noise

Man has developed very many and ingenious ways of making sounds and noises, for example in musical instruments. In many cases the amount of energy expended is only small and as the process is usually very inefficient, the noise being only a small by-product, the actual power of the noise itself may be only trifling. For example, if we let a steel ball drop on a table, or clap our hands, only about 1 part in 1000 of the energy dissipated goes into sound. We can do better than this, however, in the case of motor horns, where the efficiency is of the order of a few per cent., being much the same for bulb and electric horns. The coil-driven cone type of loud speaker used in domestic wireless sets has an efficiency of about 3 per cent., but this figure is raised to 30 per cent. or more with the high-efficiency loud speakers used for talking-pictures.

The actual powers of some familiar sounds are interesting. The acoustic output of a motor horn ranges between about $\frac{1}{10}$ watt or less for a bulb horn and about 1 watt for a powerful electric type. An orchestra of 75 players has a normal output of about $\frac{1}{2}$ watt (the bass drum claiming some 60 per cent.) which may rise to 50 watts in stirring passages. The loud speakers used for talking-pictures may operate at a few watts, while ship's sirens and fog sirens attain figures in the region of a horse power. By comparison the human voice is very weak, reaching in normal use only about 10 microwatts. This only increases 100 fold even when shouting.

The Measurement of Intensity

The measurement of the small energies commonly concerned in acoustics offers no great difficulty to the microphone and amplifier plus a suitable rectifier and microammeter. The choice of microphone depends on the circumstances. Moving-coil, ribbon, condenser and crystal microphones all have their uses. The sensitivity of piezo-electric microphones is low but the response curves are remarkably uniform, and it is pleasant to contemplate an effect which was known to Coulomb as long ago as 1780, finding still another sphere of application. As regards the absolute calibration of microphones, we rely largely on the Rayleigh disc, such work demanding a heavily lagged room and the resources of a standardising laboratory (Plate I).

The amplified microphonic current can be turned to account in divers ways—it will light a lamp or turn a motor like any other alternating current. It can act as a noise-alarm meter, or if joined to a cathode-ray oscillograph it will display the wave form of a

noise. When dealing with complex noises, however, the interpretation of such wave forms is usually not convenient in practice, and the composition of noises is normally revealed more readily by other means.

The Analysis of Sound and Noise

The analysis of a complex sound involves the determination of the frequencies and magnitudes of the various components which it contains—the splitting up of the sound into its spectrum. The methods which are used for this purpose in optics are not directly applicable to the case of sound owing to the enormous difference between the wave lengths in the two cases, and to the fact that, whereas the frequencies of the visible spectrum cover a range of about one octave only, the frequencies of sound to which the ear responds cover a range of some ten octaves.

For the analysis of sound, the earlier experimenters used acoustical resonators which could be tuned to respond to the different components present in the complex sound. With the development of the electrical technique of sound measurement, improved methods have been devised. In all these the oscillatory pressure variations which constitute the sound are first converted, by means of a microphone, into corresponding electrical oscillations; and the problem is reduced to the analysis of a complex electrical oscillation, a process which may be effected by tuning or filtering circuits or by the use of a search or heterodyne tone.

Steady complex sounds are readily analysable and yield line spectra, but the matter is not so simple with transients which are acoustical impulses starting suddenly and dying away rapidly. Examples of transients are speech (and in particular certain consonants like *p* and *b*), whip cracks, rifle shots, blows, foot-steps, hand clapping, percussion instruments, *e.g.* the piano, drum, and, in a lesser degree, most musical instruments. All such impulsive sounds are found to be largely of the band spectrum type with superposed line spectra and nearly all contain very high frequencies. Noise analysis may be of particular service in studying machine noises as a means of tracking the source of an offending component. Synthesis of sounds is also possible.

The Measurement of Loudness

We come now to the major question of noise, that is the measurement of loudness, and to what extent it is determined by energy. And as loudness is a subjective effect, in the appraisement of

which the ear is the sole arbiter, we shall do well to recall some of the peculiarities of the ear.

(*a*) Even for pure tones in free air it has been established by Kingsbury and others, that only when the intensity [1] is high, do loudness and intensity keep even approximately in step as the frequency changes. At moderate intensities the ear is more sensitive to medium frequencies than to equally intense high or low frequencies. At feeble intensities the selectivity of the ear is still more pronounced, the sensitivity being much greater for medium frequencies than for equally intense high frequencies, and very much greater than for equally intense low frequencies. Equal increments of energy do not in fact produce equal increments in loudness at different levels.

(*b*) The thresholds of hearing and the threshold of pain are very different for different frequencies. The range of intensity with which the ear can deal is relatively restricted for high and particularly for low notes, but for medium pitch notes the intensity range is enormous, amounting to 10^{13} fold.

(*c*) In the case of complex sounds the loudness depends upon their character; and, in general, we find we cannot estimate the loudness from the known energy—loudness relations of the pure-tone components.

It is clear that while loudness and intensity are related, we cannot evaluate the aural judgment of loudness by means of an energy meter. We meet the difficulty by setting up a reference standard of comparison in the shape of a pure tone of specified frequency operated under specified conditions by a specified amount of energy. The British Standards Institution specifies a pure 1000 cycle tone as standard, and in view of the enormous energy range of the ear, the intensity is expressed in decibels above an arbitrarily chosen "zero" of 0·2 millidyne per sq. cm., a value which is near the threshold of hearing. If the reference tone is operated at n *decibels* above this zero under free-air conditions, it is said to have a loudness of n *phons*.

The reader may be reminded that a "bel" represents a 10 fold increase in energy, while each "decibel" signifies roughly a $\frac{5}{4}$ increase (*i.e.* antilog. 0·1), two decibels a $(\frac{5}{4})^2$ increase, and so on. Various "zeros" have been employed in the past, *e.g.* 1 millidyne per sq. cm., which results in numerical values of loudness some 14 phons less than with the British Standard zero. In Germany the

[1] The "intensity" at a point is the normal sound energy flux per unit area in a progressive wave. The intensity is proportional to the square of the r.m.s. value of the sound pressure at the point.

zero used is 0·0003 dyne per sq. cm., but as there is a difference in the methods of listening, the British and German scales of loudness are probably undistinguishable. The scale used in America agrees with the British.

In assessing other sounds or noises, whether simple or complex, we speak of an "equivalent loudness," that being the value in phons of the reference tone when it appears to the ear to be equal in loudness to that of the sound to be measured.

The following table sets out the British Standard scale in simple form :—

British Standard Scale of "Equivalent Loudness"

Standard tone . . . 1000 cycles per sec.; plane wave in free air.
Zero . . . . 0·0002 dyne per sq. cm. (near threshold of audibility).
Listening . . . With both ears; observer facing source.

Intensity of Standard Tone		Loudness of Standard Tone in Phons.	Popular Description.
In Terms of the "Zero" Intensity.	In Decibels above Zero.		
1	0	0	Silence
10	10	10	
10^2	20	20	Quiet
10^3	30	30	
10^4	40	40	
10^5	50	50	Moderate
10^6	60	60	
10^7	70	70	
10^8	80	80	Loud
10^9	90	90	
10^{10}	100	100	
10^{11}	110	110	Very loud
10^{12}	120	120	
10^{13}	130	130	

Two points may be mentioned in regard to the phon scale. The first is that many workers in the past have been in the habit of using the decibel to express both intensity and loudness. This practice although often interpretable is slipshod and is to be deprecated in view of the foregoing. The B.S.I. recommends that the decibel should be restricted to energy measurements.

The second is that the phon scale is objected to in some quarters because it does not interpret numerically one's sensations of loudness, a phon at the top of the scale meaning much more than it does at the bottom. While this is not contested, does it really matter?

We do not expect a bowl of water at 90° F. to feel twice as hot as at 45° F., neither need we expect 90 phons to sound twice as loud as 45. We buy screws, nails, wire, rashers of bacon, shoes, buttons, by unfamiliar scales some of which arbitrarily ignore common relationships. It is all a question of experience, and while a scale of equivalent loudness in phons may not be subjectively ideal, it does rest on a sound physical foundation and on the admitted ability of the average individual to match equality of loudness.

Noise Meters

There are a number of commercially available noise meters on the market, some of them portable. They consist of two main types, each requiring to be calibrated against absolute standards.

(*a*) *Subjective Noise Meters* which depend on the equality matching of the loudness of the noise, as heard by the ear, with a reference tone (usually a pure tone of specified frequency and of an adjustable range of intensity) as heard in a telephone earpiece held tightly to one ear. A masking procedure can also be employed. The tone of the subjective meter may be produced by an electric buzzer, valve oscillator or other means, various frequencies being used in practice (Plate II, Fig. 2).

Subjective meters are useful for certain purposes, but the aural judging of equality of loudness of the standard tone and of a noise very different in character is not always easy. Individuals are found to differ widely in their judgments and the same individual is not always consistent. A team of experienced observers is usually employed when higher accuracy is desired. The difficulties are increased in the case of unexpected or short-lived impulsive noises.

(*b*) *Objective Noise Meters.*—These are microphone-amplifier instruments usually arranged to be direct-reading. Their use depends on the fidelity with which they can be constructed to simulate the selectiveness and response of the ear in different circumstances. This is done by interpolating in the circuit an electrical network, the constants of which are adjusted until the same meter reading is obtained from steady pure tones of different pitches which sound equally loud to the ear, whatever the pitch. The instrument may be designed to simulate the ear in other respects, for example in not giving a full reading until a steady sound has persisted for about $\frac{1}{5}$ second and also in dealing faithfully with intermittent sounds whether isolated impulses or rapidly recurrent. Not all commercial instruments have these refinements. Plate II, Fig. 3, shows one designed at the National Physical Laboratory.

As regards the general question of the adequacy of objective meters as loudness measurers, the experience of responsible authorities in Germany, the States and this country is that objective instruments of appropriate design and properly calibrated will, in suitable cases, give meter readings corresponding to average aural judgment. While universality of application cannot at present be claimed for such instruments, their practical convenience may often outweigh their theoretical imperfections. They do not require trained observers and will deal with noises of a type which do not give time for aural judgments of equality.

Noises in Everyday Life

There are many miscellaneous sounds in life which are a potential source of annoyance to someone, but it is possible to touch on only

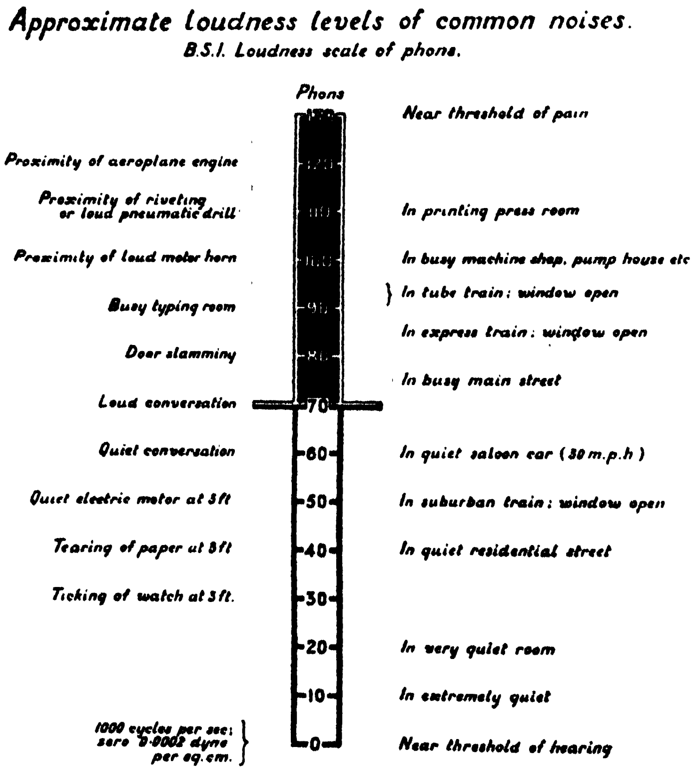


Fig. 1.

a few. During the last few years the loudness levels of many types of noise have been determined by the National Physical Laboratory.

Some of them are shown in the "noise thermometer," illustrated in Fig. 1.

ROAD-TRANSPORT NOISES

Road-transport noises are much in the public mind at the moment and are at present the subject of enquiry of a Ministry of Transport Committee. The major components of motor vehicle noises are those from the exhaust, engine and transmission, though from the point of view of the public it is the "over-all" noise which matters. For vehicles fitted with pneumatic tyres there does not seem to be much to choose, from a noise point of view, between present-day road surfaces of good quality. Tyre noises are not very conspicuous except at moderate speeds or on a wet day.

One's general impression is that the average motor vehicle is quieter than it was a few years ago. Few things have had more attention showered on them from the industry which as a class deserves great praise for its efforts to secure quietness. This applies particularly to private cars, a large percentage of which on the road to-day are unobjectionable from the point of view of noise when normally used. The flexible mounting of engines, the counter-balanced crankshaft, silent gearing, the straight-through type of exhaust silencer are all major contributions. In the matter of exhaust silencing certain motor-car constructors have achieved remarkable perfection.

The newer designs of buses are attractively quiet, and the replacement of the tramcar by the trolley bus has been an acoustic boon. Complaints are, however, still frequent of certain lorries, motor-cycles, and sports cars. It is admitted that the last two have hitherto had to cater largely for a section of the public which revels in noise, but the suppression of the flagrant offenders will come about if the recent recommendations of the Ministry of Transport Committee are given effect.

While the growing volume of road traffic is a potent factor in the noise problem, the ever-increasing speed of individual vehicles is no less important. There can be no doubt as to the beneficent value of the Minister of Transport's 30 m.p.h. speed restriction in built-up areas. Recent measurements conducted by the National Physical Laboratory for the Ministry show that the overall noise of vehicles is largely conditioned by their engine speeds, that is if their exhausts are adequately silenced. In most cases, vehicles which are exceptionally noisy at maximum road speed have unusually high maximum speeds.

The noise of a vehicle heard in the open depends of course

on its remoteness and, to a less degree, on its environment. For a vehicle in an open space free from buildings, etc., the rate of diminution of the average noise with distance appears to follow the inverse-square law fairly closely. In a tunnel or garage the law does not hold and a vehicle always sounds louder than in the open, the noise level being built up by multiple reflection. To a pedestrian, a car passing from an open square to a narrow street, sounds but little louder, but the driver of the car may experience an appreciable increase in the noise of his engine, exhaust or tyres by reflection from the walls of the street. Most drivers will have noticed how the high-pitched components of the noise from their cars are reflected to their ears as they pass quite minor way-side objects such as tree trunks, telegraph poles or lattice fencing.

Multiple reflections account of course for much of the noise discomfort experienced by the occupants of the upper stories in busy narrow streets : this would be largely mitigated if high buildings on opposite sides of a street were not built directly facing each other. The effect is well known in New York where wise visitors book rooms in certain hotels at as high a level as possible, or at any rate above a "set back" or a parapet in the building. Bedrooms and rooms where quiet is essential should of course be at the backs of houses. Perhaps we ought to build our houses face about.

To come now to the question of motor horns, a subject to which the Minister of Transport not long ago made a valuable practical contribution. His beneficent suppression of horn hooting at night has taught many people to drive more quietly in the daytime also. Many will agree that certain motor horns are acoustic atrocities and should not be permitted anywhere. Stridency of blast of a horn does not make for safety but rather leads to exasperation or downright fear. The National Physical Laboratory has conducted measurements on motor horns for the Ministry of Transport and found that stridency is mainly due to sheer loudness and the presence of high-pitched components particularly when they are unrelated. There seems no doubt that further legislation with regard to motor horns would be in the public interest.

Abatement of Noise

Complaints against noise, and regulations to abate it are as old as civilisation itself. Juvenal (A.D. 47) writes of sleep being impossible in Rome owing to the noise of herds of cattle and the rumbling of waggons in the narrow winding streets. The clatter of the medieval town provoked many restrictive regulations.

In the Middle Ages noisy occupations were zoned. It was ruled

in Halle that a goldsmith might not live next to a professor since he disturbed the professor by his hammering while the latter by his learning enlightened the world. We find embargoes on nocturnal horn blowing and wife beating in Elizabeth's reign. A well-known picture of Hogarth's illustrates the tumult of the streets in 1741. Complaints of the noise of stage coaches and sweep's cries were common in the early nineteenth century. It is only in present days, however, that the question of noise abatement has assumed social and industrial significance. In some countries there are now severe prohibitions against noise, while under Acts of Parliament in this country the production of certain noises at certain times and in certain places has become an indictable offence.

In considering ways and means of abating the noise received in a particular place we may take notice of one or two guiding principles. First of all we must consider if there is more than one contending source of noise; if so, owing to the characteristics of the ear, the overall noise will be dominated by the loudest component, and no appreciable improvement will follow until that particular one is reduced. This is illustrated by the fact that a similar second component equally intense would only add 3 phons to the overall loudness, while a noise 10 decibels less intense would contribute less than half a phon to the total. As soon as the loudest component is sufficiently quietened then the next loudest (or the next most important path of communication) must be tackled, and so on.

The second principle is that the extent to which a particular noise needs to be abated depends, as already remarked, on the background. It is no use spending time and money unduly quietening a particular machine intended for use in a somewhat noisy environment. Almost any machine will sound quiet in a forging or punching metal shop for instance.

General Methods of Noise Abatement

There are two main methods of attacking the problems of noise abatement.

(1) Reduce the noise at its source.

(2) Render less effective the lines of communication:

- (*a*) By introducing discontinuities in the paths of structure-borne noise and vibration.
- (*b*) By interposing barriers in the paths of air-borne noise.
- (*c*) By applying absorbents to both the source and receiving regions.
- (*d*) By eradicating resonance effects.

The first method, that is to reduce substantially or completely a noise or vibration at its source, particularly when it is unnecessarily excessive, is much the most effective plan when it can be carried out, which unfortunately is not always possible. The engineer is, however, beginning to realise that when a noise is clearly a common nuisance he should stop generating it and not rest content with merely smothering it. His achievements with the engine and gearing of high-class motor cars show what he can do by scientific design and careful manufacture. We can now buy not only silent cars but silent typewriters, silent electric motors, silent switches and so on, but there are many other things which the Anti-Noise League is trying to make the public realise it has the right to demand.

As regards the second method, we can best illustrate the principles in question by their application to quiet housing.

Quiet Housing

One of the great social inconveniences in present-day life for large numbers of people is the lack of quietness which modern building design and materials have brought in their wake. The modern house does not protect us from noises, whether from within or without, like the more solid houses of a generation ago. One cannot escape from the neighbour's wireless, gramophone, vacuum cleaner or bathroom or even his conversation. The problem is accentuated in the case of the many large blocks of flats which are being erected all round us. The question of acoustic isolation for the occupants receives little or no attention in the majority of cases; and it is a common source of complaint that purely local noises, such as tapping, can be heard throughout the length and breadth of large buildings constructed on modern lines. Yet architectural acoustics is no longer shrouded in mystery and empiricism, but is a science of which some of the physical principles are well established, and the practical outcomes are often predictable. A good deal can, in fact, be done to mitigate the noise nuisance in buildings, but such steps are best taken during the designing. Remedies applied after erection are likely to be less satisfactory.

Despite his present predilection for homes contrived in the closest three dimensional packing we must concede the inalienable prerogative of the flat dweller to make a reasonable amount of noise on reasonable occasion in his own domain. He must be allowed to turn on his wireless and gramophone but he should be under strict obligation not to annoy his neighbours. His children

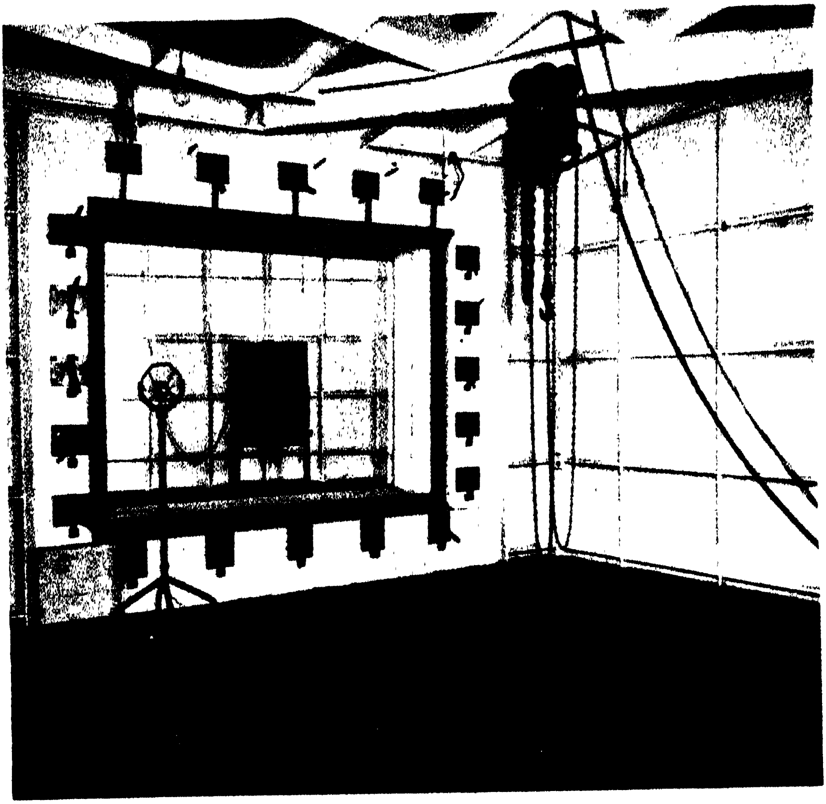


FIG. 1.—Transmission Room at the National Physical Laboratory for measuring the sound-proofing value of walls and partitions.

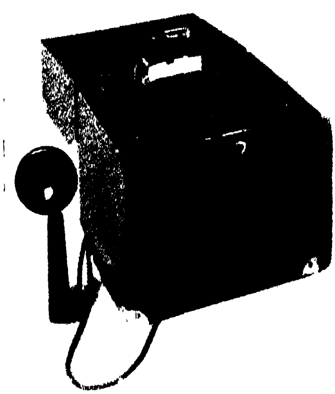


FIG. 2.—Barkhausen Subjective Noise Meter.

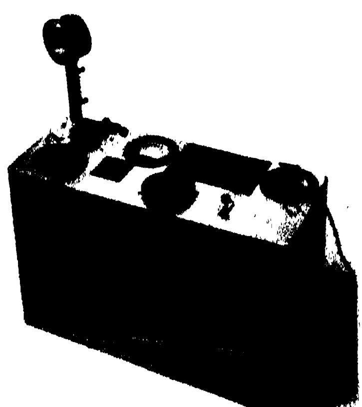


FIG. 3.—N.P.L. Objective Noise Meter.

must be allowed some of that acoustic exuberance which is part and parcel of the life of the happy and thriving adolescent.

The situation lies largely with the architect who, in his desire to keep step with progress, has discarded vital discontinuity of structure and poorly conducting materials in favour of the monolithic structure and good conducting materials which moreover have been decreased in thickness and weight. The steel-framed and ferro-concrete building, cement mortar, very hard bricks and hard plaster, to say nothing of central heating, running water and other piping, have replaced the softer brick-work, lime mortar, lime plaster, wooden beams, joists and studding and localised piping of the older houses. No one pretends of course that we can go back to the old methods, but we can, at any rate, graft on the new structural designs certain features which, however undesignedly, were present in the old.

The noises which reach a room, whether by direct or circuitous paths, consist of two classes:

(1) those which travel by air;

(2) those which arrive through the structure of the building.

In the case of walls, air-borne sounds are of the greater significance, but in the case of floors, structure-borne noises, such as arise from impacts, constitute the major nuisance. Both classes of noise are being studied in the new Acoustics Laboratory at the National Physical Laboratory.

Air-borne Noises

Plate II, Fig. 1, shows the transmission room at the National Physical Laboratory for studying the transmission of air-borne sounds by partitions. In the case of a single homogeneous barrier, it is established that the insulation to air-borne sounds is roughly proportional to the logarithm of the weight per sq. foot (Fig. 2). High notes are easier to stop than low. The relation is of course vitiated by the presence of cracks or badly fitting joints.

We see from the shape of Fig. 2 that an air-separated double partition is likely to have advantages over a single partition of the same total weight. Recent experiments at the National Physical Laboratory show that some 5 to 10 decibels superiority may result if the double partition is properly designed.

Structure-borne Noises

It is a matter of common experience how well most solids conduct sound; and many present-day building materials are no

exception. According to Meyer, the intensity of a medium pitched note is only damped by about 1 decibel after conduction through 1000 yards of steel. Concrete is about 20 times a better insulator and wood about 50 times.

The arresting of structurally transmitted noises and vibrations

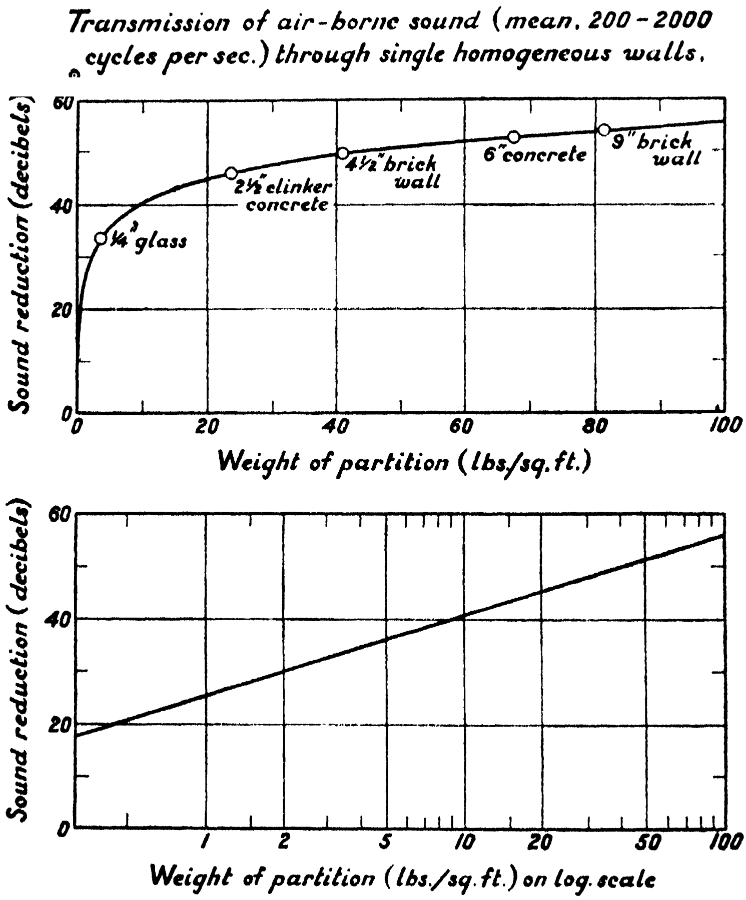


FIG. 2.

is not an easy matter, the accepted remedy being discontinuity either complete or more usually in the form of insulation of some kind. The ideal insulator is free from damping and of such resilience when correctly loaded that its natural period is several times that of the disturbance it is desired to arrest. The principle finds application in the quietening of pumps, refrigerators, etc., in the basements of buildings, commercially available isolating devices

being obtainable of cork, rubber or insulated metal springs for heavy machinery. For still heavier loads like the foundations of buildings, anti-vibration pads of lead, asbestos and steel have been used.

An even more important aspect from the point of view of the flat dweller is the insulation afforded by the floor above him against footsteps and similar impact noises. Recent observations at the National Physical Laboratory show that a blow on a bare hollow-tile or solid ferro-concrete floor sounds virtually as loud below the floor as above it, and that quite trifling impacts on the floor can readily be heard in a room below.

It would appear that the most promising solution is likely to result from the addition of a second barrier, much as in the case of the double partitions. Such a " floor finish " might take the form of

(1) a superposed or " floating " floor supported on the structural floor by properly designed and correctly loaded insulators or insulating material ;
(2) a suspended ceiling supported from the structural floor or the side walls by properly designed insulators ;
(3) the common but less effective expedient of a resilient floor covering such as rubber, cork lino, or carpet.

For the purpose of floor measurements, an impact machine has been designed at the National Physical Laboratory in which weights, faced with any desired material, fall freely under gravity and so impart a succession of steady blows to the floor under test. To give the observations greater practical significance, the energy of each blow is of the same order as that of the heavy footstep of a man, *i.e.* about $\frac{1}{4}$ ft. lb. The value may be no more than $\frac{1}{30}$ ft. lb. for a girl. It does not appear, however, that the force of the impact is very critical, as the degree of attenuation introduced by a floor, although dependent on the character of the blow, is not greatly affected by its energy. These researches are not yet complete, though progress is being made in the direction of floating floors.

Absorption of Noise

In the case of noise which is generated within a room or has succeeded in entering it, the sound waves are reflected by the walls and other solid surfaces in all directions until they are finally absorbed. A hard-plastered wall is a better reflector of sound than most mirrors are of light ; and so, sound waves may travel for miles in large and empty rooms before dying away. This process takes an appreciable time—several seconds in extreme cases—and the

effect heard, termed "reverberation," is due to the continued succession of reflected waves which reach the ear. In such rooms the noise level may build up to an uncomfortable degree. The remedy is to increase the absorbent in the room, whether carpet, heavy curtains, upholstery or one or other of the absorbents now commercially available for application to walls and ceilings, the latter position being specially advantageous in many noisy rooms.

The coefficient of absorption, that is the amount of absorption per unit area of a material for a particular frequency, depends upon its size, thickness, backing, distribution, etc. At the National Physical Laboratory, measurements are made in a reverberation room on 100 sq. feet of material, under as strictly specified conditions as possible.

Good sound absorbents are characterised by the presence of small interstices or crevices; and certain materials previously sold as heat insulators have found a second market as sound absorbers. The absorption of high-frequency notes is a relatively easy matter, but low-frequency absorption is more troublesome, requiring substantial thicknesses of absorbent and air spacing behind. Indeed at all frequencies, the effectiveness of sound absorbents is usually increased by mounting them on battens or studding rather than against the actual wall surface.

One other feature of acoustic absorbents is that the same area of absorbent is often more effective when distributed in a number of isolated patches round a room than in one single large patch. It may be noted that certain aerated plasters and tiles are designed to give a masonry finish. Many soft absorbents such as felt, asbestos, slag wool, etc., usually require a surface covering of some kind for appearance sake. This must be sufficiently porous to allow the sound free access to the underlying absorbent. It appears that a perforated hard surface in which the perforations occupy only a few per cent. of the total area transmits practically all the sound incident on it. When a fabric covering is used the perforations may be reduced to pin-pricks occupying less than 1 per cent. of the area without appreciable detriment to the absorption. The pin-pricking may be done after the fabric has been distempered or otherwise decorated.

As regards floor coverings, neither cork nor rubber carpet is a good absorbent of air-borne sound. A thick carpet is substantially more absorbent than a thin, and in either case the use of a felt underlay is well worth while.

The Flat of the Future

If one may venture on prophecy as to the future construction of flats to meet acoustic requirements, it would be something on the following lines:

1. If the steel frame and ferro-concrete types of construction persist, as seems probable, discontinuity or insulation in some form will be introduced either
   (*a*) into the structural framework, which in many cases may be difficult or impracticable; or
   (*b*) into the boundaries of the individual rooms, *e.g.* all walls and floors will be virtually double, the several components being adequately insulated from each other and from the structural framework.
2. Ceilings of rooms will be absorbent; possibly also the upper parts of walls.
3. As the problem of the open window is not likely to be solved satisfactorily, artificial ventilation (with air conditioning) employing sound absorbent ducts will be available whenever conditions render it obligatory to close the doors and windows of a room whether because of noise within or without. Doors and windows will be of a well fitting type and windows will be double or of heavy glass.

The above will serve to illustrate some of the acoustic problems which are being studied at the National Physical Laboratory. It will be appreciated that for much of the work a high degree of acoustic isolation is essential, and to this end the new acoustics laboratory was given unusual structural features. Each of the experimental rooms consists of a double shell—double floors, double ceiling and double walls—the inner room being completely free of the outer room except where it rests on cork pads on concrete piers. The walls are massive and the entrance doors are of thick steel. The inner room weighs some 150–200 tons, and should the cork pads ever deteriorate under the load and require to be renewed the entire room can be lifted for the purpose by hydraulic jacks.

It is with the aid of such facilities that the Laboratory has already been able to make material contributions to the study of sound transmission, reflection and absorption, and to carry out tests of the acoustic merits of commercial materials and structures, in an effort to assist the architect, and to meet the demands of the general public.

THE EARLY HISTORY OF PHOSPHORUS

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THE accounts in the text-books of the discovery and early history of phosphorus are rather unsatisfactory and contradictory; the following statements are based on material which came to my notice some years ago in preparing my paper on the early history of hydrofluoric acid [1] which was then equally obscure and contradictory.

Since I have had all the rather rare documents at hand, I have made use of them in clearing up some disputed and doubtful points in the history of phosphorus. It seems best to begin with and work round the account given by Kunckel (or Kunkel, as he variously wrote his name) in his posthumously published *Laboratorium Chymicum.*[2] Since Kunckel died in 1703, this account must have been written not later than then. He mentions Thurneysser (1530–96) as living in the preceding century [3] and says [4] that he himself had worked at chemistry for sixty years, which (as he was born in 1630, and assisted his father in his youth) would also indicate a date about 1700 for the composition of the book. Kunckel says he was in Hamburg soon after 1677 with a specimen of the recently discovered Baldwin's phosphorus (phosphorescent calcium nitrate). This date is certainly incorrect, since, as will be shown below, Kunckel had in 1676 given an account of the new phosphorus which he saw in Hamburg, and in that year a description of it was published by two different authors, Kirchmaier (who obtained his information from Kunckel) and Elsholtz. The date 1677 is, therefore, almost certainly a mistake for 1675, when the discovery of Baldwin's phosphorus was announced by Christian Adolph Baldwin in his *Phosphorus hermeticus sive Magnes luminaris.*[5] In Hamburg,

[1] *Mem. Manchester Lit. Phil. Soc.*, 1923, lxvii, 73.

[2] Hamburg and Leipzig, 1716, 656 f.; 3rd ed., Hamburg, 1738, 656 f.; 4th ed., Berlin, 1767, 601. An accurate and almost complete translation of this is given by Hoefer, *Histoire de la Chimie*, 1869, II, 194 f., so that only a summary is now given. All the editions agree, including the date 1677.

[3] *Ibid.*, 1716, 527.

[4] *Ibid.*, 1716, 151.

[5] Amstelodamæ apud J. Janssonium, 12mo, 1675; see later under Boyle.

Kunckel was told that another phosphorus, which shone permanently in the dark, had *recently* been discovered by a Dr. Brand, a ruined merchant who had turned to medicine. He obtained an introduction to Brand, whom he calls a *Doctor Teutonicus* who knew no Latin; when his child had fallen on its face he advised him to apply some *oleum cerae*, but Brand did not understand, asking "in good Hamburg," as Kunckel says: "Wat is dat?", and on being told, saying, "Sü, fü, dat is ock wahr, ick bedacht mi nich so balde." Brand was not an educated man, and probably had no medical degree.

Kunckel saw the only small specimen of this phosphorus which was available, which Brand had given to a friend, but between the two persons Kunckel was unable to learn the process. Kunckel then wrote to his friend Krafft at Dresden, and the latter, without his knowledge, came post haste to Hamburg and bought the secret from Brand for 200 thalers (about £37) whilst the latter was still negotiating with Kunckel, on the condition that Kunckel should not be enlightened. Kunckel did not even know then that Krafft was in Hamburg, and Brand, who seems to have felt some compunction for what he had done, told Kunckel, and also a woman, that the substance was obtained from urine. Kunckel, on his return to Wittenberg, set to work, and *in a few weeks* he had obtained phosphorus. He says he had accidentally met Krafft in Hamburg, and the latter advised him not to waste his time negotiating with Brand.

Kunckel prides himself on having first obtained the phosphorus as clear as crystal, but he ceased to make it, as much evil can arise from it. Thus, he says, he leaves the whole story for the time when it will be forgotten and others will dispute who was the first discoverer. He is willing to give to Cassius the credit for the discovery of gold ruby glass, but he will not be denied the honour of the independent discovery of phosphorus. The whole story bears the impress of truth.

The first published account of phosphorus was made in 1676 by Kunckel's friend, Georg Caspar Kirchmaier, a professor of rhetoric at Wittenberg and a copious theological author. His dissertation [1] is in quarto, 25 pages, including the title page and blank *verso*. The preface is dated September 11, 1676, and Kirch-

[1] *Noctiluca constans & per vices fulgurans, diutissimè quæsita, nunc reperta; Dissertatione brevi prævia de Luce, Igne et Perennibus Lucernis, Publicata à Georgio Caspare Kirchmajero, In Electorali Academ. Witteberg. Prof. Publ.*, Wittebergæ, Typis Matthæi Henckelii, Acad. Typogr., Anno M.DC.LXXVI.

maier says the phosphorus was first shown to him by Kunckel about six months before; the preparation was repeated about July 25, when hardly more than half an ounce was obtained by a strong fire continued for several hours.[1]

In the same year, Johann Sigismund Elsholtz, a German physician born in 1623, who had studied at Wittenberg but was then in Berlin, published one quarto sheet in four pages entitled: *Joan. Sigism. Elsholtii: de phosphoris quatuor observatio*, Berolini, Literis Georgi Schultzi, 1676, in which he describes four kinds of "phosphorus", *viz.*: (1) *phosphorus Bononisenis* (phosphorescent barium sulphide); (2) *phosphorus Balduini* (phosphorescent calcium nitrate—"Christ. Adolphi Balduini . . . anno 1675 peculiari libello, cui *Phosphorus hermetico* sivi Magneti luminari nomen, Orbi literato exposuit," as Elsholtz says)[2]; (3) *phosphorus smaragdinus* (which I have shown in a former communication[3] was fluor spar, the "Bohemian emerald"); and (4) *phosphorus fulgurans*, which he says is Krafft's (or ordinary) phosphorus. This sheet has two figures in one, showing phosphorus glowing on a plate and in a tube open at both ends. In 1677 Elsholtz published half a sheet quarto at Berlin with the title *De phosphoro liquido observatio*, which dealt with the aqueous solution or suspension of phosphorus; and in 1681, also at Berlin (G. Schultz), two sheets quarto, entitled *De Phosphoris observationes: quarum priores binæ antea jam editæ, tertia vero prima nunc vice prodit.* The first part deals with the four varieties of phosphorus, as in the 1676 publication, but there is a curious plate with three new figures; the material of the 1677 publication is given as *Observatio secunda, de phosphoro liquidò*, and a new section, *Observatio tertia, de phosphoro stellato, nubiloso & literato*, appears. There are references to Kunckel. None of these treatises by Elsholtz is mentioned by Kopp in connection with ordinary phosphorus, although he refers[4] to the observations on fluor spar. Kirchmaier in 1680 published another tract, called *De phosphoris et natura lucis, nec non de igne commentatio epistolica*,[5]

[1] Sextus propemodum elapsus mensis est, ex quo significavit mihi primum, Luminis perpetui se posessorum esse. . . . Quamdiu artifex collegerit in materia ritè præparandâ jam non explanabo; id tantum contentus memorasse, cum laborem iteraret die XXV Julii haud ita pridem, vix ultra uncia dimid. elicuisse igne valido, per horas plurimas continuato.

[2] Kopp, *Geschichte*, III, 327, gives the date of Baldwin's discovery as 1774, probably a misprint for 1674.

[3] *Manchester Lit. and Phil. Soc., loc. cit.*

[4] *Geschichte*, III, 367.

[5] Wittebergæ apud Johannem Henricum Ellingerum Bibl. Anno cIc Iↄↄ LXXX; one sheet of four quarto pages; there are four copies in the British Museum.

in which, as Beckmann [1] said, he deals with the phosphorescence of fluor spar. This contains some letters, dated early in 1677, from Oldenburg, the Secretary of the Royal Society, in which he refers to Kunckel's phosphorus.[2]

In 1678 Kunckel himself gave an account of the properties of phosphorus in his *Œffentliche Zuschrift von dem Phosphoro Mirabili und Dessen leuchtenden Wunderpilulen.*[3] In this tremendously verbose account, he describes two kinds, a black "soap" and yellow grains, and says it ceases to glow in a closed bottle. He makes no reference to Brand or to the method of preparation of the phosphorus.

Phosphorus seems to have become generally known about 1677, and in that year (perhaps as a result of Oldenburg's correspondence) Krafft came to London with some specimens, which he exhibited to Boyle. A very interesting account of this event is given by Boyle himself in Hooke's *Lectures and Collections.*[4] On the title page of this work is the statement: "Mr. Boyle's Observations made on two new Phosphori of Mr. Baldwin and Mr. Craft," as part of the contents, which is further evidence that Baldwin's discovery was made about the same time as Brand's, and that Krafft was then regarded in London as the discoverer of phosphorus. On p. 57 Boyle says, in an account headed: "A Short Memorial of some observations made upon an artificial substance that shines without any precedent illustration," that, after supper on Saturday, the 15th September 1677, he was visited by Mr. Kraft, a famous German chymist, who proceeded to demonstrate the properties of the phosphorus preparations to Boyle and several members of the Royal Society. Boyle gives a graphic account of how Krafft unpacked and laid out his material, among which was a "solid of the size of a couple of pease, which he seemed to make much more account of than of all the Liquors and which he took out for a few moments to let us look upon it, whereby I saw that it was a consistent body that appeared of a whitish colour." It is clear that Krafft was then in possession of a fairly pure specimen of solid phosphorus. On September 22, Krafft came and saw Boyle alone,

[1] *History of Inventions*, 1797, III, 235.

[2] Letter, from Oldenburg, Sec.R.Soc., 9 Feb. 1677: ut noctilucæ illius specimen, oculis suis usurpare detur: quod quidem in perpetuam (Artificis Kunckelii) memoriam & honorem monumentis suis Philosophicis inferendum curarent.

[3] Leipzig bey Michael Rustwurm Gedruckt. Joh. Wilh. Krüger Herausgegeben in Wittenberg, Anno 1678.

[4] *Lectures and Collections made by Robert Hooke, Secretary to the Royal Society*, London, 1678.

and was then able to show him an experiment which had failed on the 15th, *viz.* the ignition of gunpowder after a short interval in contact with a piece of warm phosphorus taken up on a quill pen. The demonstration made by Krafft on the 15th included several experiments, among them the producing of a glow on the hand by rubbing it with a reddish liquid, which was probably a suspension of phosphorus in water. Boyle does not say here that Krafft told him anything about the method of preparation, although this was probably the case, as will be seen below. This account is sufficient to show (if that were necessary) how inaccurate is the statement by Fulton [1] that: "when Boyle heard of his [Krafft's] activities he had already isolated phosphorus from human urine and proved to his own satisfaction its elemental nature."

Boyle's interest was keenly aroused, and in the account just quoted he already anticipates that the new substance would be of importance in the study of the part played by the air in combustion, a suggestion which he communicated to Krafft, and of which the latter expressed his approval. Krafft was, apparently, a well-educated man: Leibniz (see below) says he was a doctor of medicine, who afterwards visited South America.

Boyle's first publications of his own experiments were made two, and three, years, respectively, after Krafft's visit, in the form of two tracts. The first is entitled *The Aerial Noctiluca, or some new phenomena and a Process of a Factitious Self-shining substance, imparted in a letter to a Friend living in the Country*, London, 1680. In this, Brand is vaguely mentioned, and it is stated that Mr. Krafft had only a small amount of solid phosphorus: "the quantity he had in London scarcely exceeding in Bulk the Kernel of an Almond." Krafft, says Boyle, made no claim in London to be the discoverer of phosphorus. Boyle mentions the paper by Elsholtz (" Elsholez "), which he could not procure (he probably heard of it from Krafft), but does not refer to Kirchmaier. He proceeds to state that: "Having at Mr. Krafft's desire imparted to him somewhat that I had discover'd about uncommon Mercuries, (which I had then communicated but to one Person in the World), he, in requital, confest to me at parting, that at least the principal part of his Phosphorus's, was somewhat that belong'd to the Body of Man." He recounts how he spent much time in fruitless experiments: he experimented for many months without success until "a learned and ingenious stranger (A.G.M.D. Countreyman, if I mistake not, to Mr. Krafft ") told him "something about the degree of Fire, that made me afterwards think, when I reflected on it, that *that* was the

[1] *A Bibliography of Robert Boyle*, Oxford, 1932, 90.

only thing I wanted to succeed in my endeavours." It is possible that "A. G." was Ambrose Godfrey Hankwitz, who certainly assisted Boyle in his work on phosphorus (see below). Boyle now seems to have been in possession of such information as led him to believe that the process, if correctly performed, should yield the desired result.

"I would not believe the skilful laborant when he told me with trouble that what I expected was not at all produced. But going my self to the Laboratory I quickly found, that by the help of Air, or some Agitation of what had pass'd into the Receiver, I could, in a dark place (though it was then day), perceive some glimmerings of light." "I obtained no such consistent Phosphorus as that whereof Mr. Krafft shew'd me." It is clear from this that Krafft did not give Boyle the full details of the process, as has been asserted (see below), and also that some other German besides Krafft and Kunckel, perhaps Hankwitz, then knew of Brand's or Kunckel's process. On p. 95 of the *Aerial Noctiluca*, under the heading "Of the way of preparing the aerial noctiluca," Boyle says he had "deposited, seal'd up, in the hands of the very ingenious Secretary of the Royal Society" the process, which may not have been the same as "that was made use of by the Ingenious German Chymists [note the plural] in their Noctiluca; for some inquisitive men have very lately told me, that the Germans mingle two or more distillable materials; whereas I employ'd but one matter capable of distillation." The last statement is rather refined, since Boyle actually used sand with evaporated urine, but then sand is not a "matter capable of distillation." The alternative possibility, that Boyle originally used evaporated urine alone, the addition of sand given in his recipe (see below) being taken from some information which he afterwards received of Brand's process, is one requiring further investigation.

Kopp,[1] who mentions the sealed note of 1680, states that "it was published in the *Philosophical Transactions* for 1692." According to Thomson,[2] it was published in *Phil. Trans.*, 1693, XVII, 583. Kopp goes on to give a few words on Stahl's account (see below) which seem intended to suggest that Boyle's process was published only after it had been generally made known. The process is actually fully described by Boyle at the end of the *Aerial Noctiluca* in 1680 (p. 105, "The Process"), as Hoefer,[3] who is often better informed than Kopp, already knew. Boyle evaporated human urine to a thick syrup, added three times its weight of fine white

[1] *Geschichte*, III, 329. [2] *History of the Royal Society*, 1812, 491.
[3] *Histoire de la Chimie*, 1843, II, 183.

sand, put the mixture into a strong retort, joined this to the large receiver in good part filled with water, and heated the retort, finally as intensely as possible, when the phosphorus came over.

The second tract on phosphorus published by Boyle is entitled: *Observations made upon the Icy Noctiluca, imparted in a letter to a Friend in the Country, to which is annexed a Chymical Paradox*, London 168½.[1] In this (p. 16) he says: "having then by processes, not unlike that I annexed to the Close of the Aerial Noctiluca, obtain'd a Self-Shining Substance of a Consistent form"—that is, having first, some time after Brand and Kunckel, obtained solid phosphorus—he made a large number of new experiments with it.

This statement seems to destroy any claim made for Boyle as being one of the early independent discoverers of phosphorus, since he had then, as he tells us, received more than one hint from "ingenious" and "inquisitive" gentlemen from Germany about the process used there. Boyle, however, as Hoefer says,[2] was the first to *publish* the process (in 1680), and the experiments with phosphorus which he describes in the *Icy Noctiluca* transcend by far anything previously done with the material. It is no part of my intention to describe Boyle's work on phosphorus: I have already pointed out [3] that his observations, with two exceptions, exhausted the subject until quite recent times.

Whether Krafft actually prepared the phosphorus which he exhibited cannot, I think, be stated: Kunckel, at any rate, says that Krafft made it a condition, when he bought the secret from Brand, that the latter should prepare for him 1 loth (about 35 grams) of phosphorus, and no one, apparently, saw more than this amount in his possession.

The next account of the preparation of phosphorus to be published was that of Lemery, in his famous text-book, *Cours de Chymie*. It is not contained in the English translation published in 1677, made by Walter Harris from the first French edition (1675), but it is contained in full in the second edition of the English translation, published in 1686, made from the fifth French edition of 1683 (which I have not been able to see). Lemery says Krafft was the discoverer, and he mentions the experiments of "Mr. Homberg, a German" (see below).

Lemery gives the preparation by the distillation of putrefied urine, with very minute details, and describes the properties of phosphorus. He says (2nd English ed., p. 530): "the phosphorus

[1] The *Chymical Paradox* has a separate title page.
[2] *Histoire de la Chimie*, 1869, II, 175.
[3] *Text Book of Inorganic Chemistry*, 1921; 4th ed., 1933, p. 609.

being cold did shine when the air was pumped out of the bottle and the light disappeared when the air was let in." This is one of the two capital discoveries on the glow of phosphorus made between Boyle's time and the present day, and Lemery shared it with Slare and Hauksbee. Lemery's account of the properties of phosphorus is full and entertaining, and includes an anecdote which I have previously reproduced.[1]

Although Homberg, as we see, had worked on phosphorus before 1683, his account of the preparation was first published in the *Mémoires* of the Paris Academy for 1692.[2]

William Homberg was born in Batavia, Java, on January 8, 1652, and died on September 24, 1715. He was educated in Germany, and became physician to Philippe II of Orleans. His wife, we are told, was a capable chemist, who assisted her husband in the laboratory; whether she was the woman to whom Brand communicated information, or not, we cannot say. The account, which is written in the third person and mentions Homberg by name, contains a history of the discovery of phosphorus which disagrees in several points with Kunckel's. It says the latter worked for four years after Brand's death before he discovered phosphorus; that in France and England Krafft was regarded as the discoverer, whereas he was really only the distributor; and that Kunckel had demonstrated the process of preparation to Homberg in 1679. It states that Brand's discovery was made in 1669 (which is probably the source of Hoefer's statement of this date),[3] and that Brand was an obscure man of humble origin who, in his youth, had been a glass maker but gave up his trade to study alchemy, a process which he persuaded himself could be effected by means of a liquor extracted from urine. In distilling urine at a high temperature he obtained a luminous matter in the receiver. Homberg's own account of the process (which is accurately reproduced by Hoefer) says he made use of evaporated fresh urine, the residue from which was allowed to putrefy in a cellar for two or three months before distillation. This detail, which would not be consistent with Kunckel's statement that he had prepared phosphorus "in a few weeks," is an addition of Homberg's and is quite unessential, as Lemery knew. Homberg says putrefied urine (used by Lemery) can also be used, but fresh

[1] *Everyday Chemistry*, 1929, p. 457.

[2] *Memoires de mathematique et de physique, Tirez des Registers de l'Académie Royale des Sciences*, Paris, 1692, 30 Avril, p. 74: Manière de faire le phosphore brûlant de Kunkel. Par M. Homberg.

[3] Mr. Fulton, in what was probably an attempt to reconcile this date with that of 1677 given by Kunckel, has given it as 1667, which, as far as I know, is without authority, although I am open to correction.

is better. He gives minute details of the process. Homberg makes it clear that the process he describes was derived from Kunckel: it is the same as that described by Boyle, and, as Hoefer says,[1] but for the fact that we are dealing with men so honest as Boyle and Kunckel, we should be tempted to believe that Brand, the discoverer, and Krafft, the exploiter, of phosphorus, were not so discreet as they have been represented to be. Boyle's own words, in fact, suggest some leakage of information. By 1683 both Brand and Krafft had been selling their secret to various people for honoraria as low as 10 thalers, and one Italian purchaser disposed of it again for 5 thalers, as Kunckel says.

A different account of the discovery of phosphorus is given by Leibniz,[2] who reproduces a Latin translation of Homberg's account, and then proceeds to criticise it. Brand, he says, was not a glass maker, but a soldier who had married a rich wife and sought the particular process in alchemy (*i.e.* the conversion of silver into gold), which he had read in a book could be effected by means of a liquor extracted from urine. In an attempt to make this liquor, in 1669, he discovered phosphorus. Brand, in Hamburg, revealed his secret not only to Krafft but also to Kunckel; the latter on his return to Wittenberg was unable to obtain phosphorus by the process revealed to him by Brand, but as in further experiments he succeeded, he falsely gave himself out to be the real discoverer. Leibniz had some personal knowledge of Brand, on which he based his account, but his statement about Kunckel is contradicted by the later account of the latter, which has been given previously. No one can read the works of Kunckel without concluding that he was a man of the most upright and transparently honest character, and his statement must, I think, be accepted. Leibniz in his youth was an intimate of alchemists and had studied the subject, so that he must be considered an authority; from 1676 he was librarian to the Duke of Brunswick-Luneburg, who was keenly interested in chemistry, and had an extensive library on the subject.

Finally, there is the account given by Stahl in 1731, *i.e.* long after the publication of Kunckel's *Laboratorium Chymicum* and of Kunckel's death. In his *Experimentis, observationibus animadversionibus CCC Numero, Chymicæ et Physicæ* . . .[3] he says Krafft

[1] *Histoire de la Chimie*, 1869, II, 198.

[2] G.G.L., *i.e.* the initials of the Latinised name of Gottfried Wilhelm Leibniz, who was acquainted with Boyle: Historia inventionis Phosphori, *Miscellanea Berolinensia*, 1710, I, 91–98: I do not claim to have exhausted the material contained in this paper.

[3] Berolini, Apud Ambrosium Haude, MDCCXXXI: 8vo: p. 392.

had told him (Stahl) that he (Krafft) had openly described the method of preparation of phosphorus to Boyle.[1]

We are left to choose between the conflicting statements of Boyle and Krafft, and if we are to believe Kunckel (and there is no reason why we should not), we shall have little difficulty in deciding in favour of Boyle, and rejecting Kopp's [2] innuendo in reporting Stahl's account.

Leibniz suggests that Boyle could not have possessed the details of Brand's process, since his phosphorus was inferior to Brand's, and this was certainly the case in 1680.[3] Stahl, relying on Krafft, says that neither Brand nor Kunckel was the discoverer [4]: he adds some interesting details of the later history of phosphorus, saying that when he wrote (1731) one ounce cost in England forty shillings, and in Amsterdam thirty-two Belgian florins ("qui sedecim imperialium numerum conficiunt"). He also says [*op. cit.*, 401 *f.*] that in 1719 Godfrey Hankwitz of Cothen-Anhalt began to make phosphorus for sale; in his youth he had migrated to England, had been Boyle's assistant, and on the death of his patron had settled in London. Stahl suggests that Hankwitz was the assistant of Boyle when the latter prepared phosphorus, which may well have been the case, although Boyle's words suggest that he came into the work later, perhaps when the solid phosphorus was obtained.

Summary

The results of the preceding discussion (in which I have confined the sources to those which are essential) appear to be somewhat as follows:—

1. Phosphorus was discovered in 1674 or 1675, probably 1675, by Brand, of Hamburg, by a process depending on the distillation

[1] Krafftium ejus primum autorem fuisse, tantum abest, ut etiam ipse hic, mihi liberaliter professus sit, se illud artificium, a Brandio illo Hamburgense, numerato pretio, accepisse: Omnino vero Boyleo in Anglia tradidisse . . . siquidem conversatio cum Krafftio, mihi contigit, anno 1691: also p. 402: ab illo tempore, quo Krafftius præparationem Phosphori Boyleo tradidit.

[2] *Geschichte*, III, 329.

[3] Leibniz, *op. cit.*: certe Boylium non nisi imperfectam descriptionem nactum, ostendit ipsius de Phosphoro dissertatio, nam Phosphorus ejus hoc tantum a Brandiano differt, quod est imperfectior.

[4] *Op. cit.*, 393: "neque vero vel ipse inventor, Brandt, neque Kunkelius, cum hoc præparato, aliquod operæ suæ pretium fecerunt; prior quidem, nundinali commercio impar: posterior autem, ut in reliquis suis laboribus, nullam hujus scopi curam gerens:" Stahl, who was of a morose disposition, shows considerable animosity against Kunckel in many of his works.

of evaporated urine. He prepared the solid, but only in small quantity.

2. At the end of 1675, or early in 1676, the process was sold to Krafft by Brand, who also informed Kunckel, and at least one other person (a woman), that phosphorus was obtained from urine.

3. Phosphorus was obtained in the solid form from urine by Kunckel not later than March 1676, after a few weeks of experimenting.

4. The first description of phosphorus was published in 1676 both by Kirchmaier, whose information was derived from Kunckel, and by Elsholtz, of Berlin, who seems to have been in touch with Krafft. The latter publication was known to, but not seen by, Boyle.

5. Solid phosphorus in a pure state was exhibited to Boyle and others in September 1677, by Krafft, who told Boyle that it was obtained from "part of the human body." Boyle succeeded in 1680, after some further information from a German doctor, "A.G.," perhaps Hankwitz, in preparing an aqueous solution or suspension of phosphorus, and in 1681 had obtained solid phosphorus. In 1680 Boyle was the first to publish the method of preparation, which appears to be that originally used by Brand, and is also that described in 1683 by Lemery, and in 1692 by Homberg, the latter obtaining his information from Kunckel.

6. The account of the discovery given by Kunckel in 1716, apart from an incorrect date, appears to be the true one, although it disagrees in part with those given by Leibniz (1710) and Stahl (1731), both of whom knew personally some of the persons concerned, and whose accounts differ from one another.

SOME ASPECTS OF THE PLANT VIRUS PROBLEM[1]

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There appears to be rather a tendency on the part of botanists to consider the study of plant viruses a dull subject and one without any sure foundation in fact. It is hoped, therefore, in this short article to show that, on the contrary, the subject is not only an intensely interesting one, involving problems of fundamental biological importance, but is also of extreme economic importance and that plant virus workers really have a definite problem in hand.

No one at the present time knows what a virus is and this uncertainty as to its nature adds, perhaps, to the interest of the study. In speaking of a virus, stress is usually laid upon certain properties which are mainly negative in character such as inability to see the virus with the microscope, impossibility of cultivating the virus on media outside the host and the fact that viruses cannot be held back by the usual bacteria-proof filters. Improving methods of technique, however, are showing that some of these qualities are merely relative and it is already possible to photograph some viruses by means of the ultra-violet light microscope and to devise filters which will allow viruses to pass or hold them back at will according to the pore size of the filter.

In speculating upon the nature of viruses, whether of animals or plants, as a whole, it is well to remember that they are a heterogeneous collection of disease agents and it is by no means certain that they are necessarily all of the same nature. At one end of the scale is the virus of Psittacosis or parrot fever, the particle-size of which is 250 millimicrons (1 millimicron equals one-millionth of a millimetre) and which is in consequence within the range of the ordinary microscope. This virus appears to have a definite life cycle and is presumably a living organism. At the other end of the scale is the virus of foot-and-mouth disease which has a particle-size of about 10 millimicrons and is only two or three times the

[1] Address to Section K, British Association meeting, Norwich.

size of an oxyhaemoglobin molecule. It is difficult to conceive of this as a living organism. Certain plant viruses are also very small, the particle-size of tobacco necrosis virus is only 20–30 millimicrons and that of a newly described tomato virus is only 17–25 millimicrons. Again, there is the recent claim of Dr. Stanley [14] of the Rockefeller Institute in Princeton that he has succeeded in crystallising out the virus of tobacco mosaic which he considers to be an autocatalytic protein, *i.e.* one which acts upon the cells of the host in such a way as to compel them to produce more of the same substance.

For the present it will perhaps suffice to adopt the definition of viruses given by Gardner [5]—" as agents below or on the border-line of microscopical visibility which cause disturbance of the function of living cells and are regenerated in the process."

In this short survey of the plant virus problem, it will only be possible to deal with one or two of the more interesting aspects of the subject and it is proposed, first of all, to discuss a few of the symptoms produced in affected plants. Since the pathological effect on the plant is almost the only criterion of the existence of a plant virus, the study of symptoms necessarily plays rather a large part. There are various kinds of virus diseases which may be loosely grouped together as follows, the *mosaic* type where attack on the chlorophyll induces the formation of mottlings or rings (see Plate I, Fig. 1); the *destructive* type which induces necroses of the cells in leaves and stems, and a third type which produces *deformities* or *overgrowth* in the affected plants.

Some of the mosaic viruses produce colour changes in the flowers of affected plants. Perhaps the best known example of this phenomenon is the so-called " Tulip-breaking " in which tulips affected with a mosaic virus produce variegated flowers (Plate I, Fig. 2). Certain of these tulips with variegated flowers at one time fetched large sums of money owing to the mistaken idea that they were new varieties, whereas they were in reality only diseased specimens of self-coloured varieties. References to this tulip " breaking " may be found in the literature of very early times. Thus, the first record is a description published in 1576, and other accounts of this variegation in tulips appeared in 1622 and 1670. It was in this latter account that the suggestion was first made that the variegated tulip might be diseased. In the Rembrandt Exhibition recently held in Amsterdam were paintings of tulips by Dutch artists of the sixteenth and seventeenth centuries, and many of these tulips showed a typical mosaic infection. Just recently, growers of the favourite blood-red variety of wallflower have been perturbed

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by the appearance of an ugly yellow stripe or flecking in the red flowers and this has led to many complaints from customers that their colour schemes have been spoiled ; similarly with self-coloured stocks [10]. The variegation in these flowers has been shown to be due to a virus carried to the plants by a species of greenfly from virus-infected broccoli or cauliflowers in the neighbourhood.

In the writer's opinion viruses play a larger part in the production of variegations in flower colours than is usually supposed. For instance, inoculations from the petals of common variegated mauve and white and mauve and yellow violas, picked at random from the garden (Plate II, Fig. 3), to healthy tobacco plants of the White Burley variety, produced in those plants a virulent mosaic disease. The virus is also capable of infecting several other species of Solanaceous plant. Experiment seems to show that the virus causing this variegation is a strain of cucumber mosaic virus (cucumber virus I).

Some of the mosaic viruses affecting ornamental plants may produce little effect on the plant other than the change in the colour of the flowers. It is quite likely therefore that a systematic enquiry into the question would show that other familiar flower variegations may be due in part to virus infection. There seems, however, to be a common element in the appearance of this type of variegation, *i.e.* a pencilling or flecking of the colours and a break in the hard line dividing two colours.

The next question is the important one of how plant viruses are transmitted in nature from diseased to healthy plants. The majority of plant viruses depend upon insects for their dissemination from plant to plant and this relationship between insect and virus is one of considerable interest. The insects concerned in the spread of plant viruses are nearly all of one type, a type of insect which feeds in a particular way which seems to be well adapted for the injection of the virus into the plant. These insects belong to the order Hemiptera and are of the sap-sucking type. The method of feeding of this type of insect is well demonstrated by means of the photomicrograph shown in Plate II, Fig. 4.

Insects are not merely mechanical vectors of the virus but in all probability some kind of obligate relationship exists between the two. The following facts seem to bear this out—certain viruses cannot be transmitted from diseased to healthy plants except by the agency of insects and often only by one species of insect or one type of insect and not by other closely related species ; some insect vectors having fed once upon a virus-diseased plant remain infective for the rest of their lives without the necessity for further

recourse to a source of virus infection. This suggests that the virus actually multiplies in the body of the insect. Further, some insects do not become infective until a minimum time has elapsed after feeding upon a virus-infected plant. This is often referred to, perhaps on insufficient grounds, as the "incubation period" of the virus in the insect. A better term would be "a delay in the development of infective power within the insect." This delay may be as long as ten days in some cases.

It is not possible to deal at length with the question of the insect relationships of plant viruses, but space permits touching upon some recent interesting work on this subject. Storey [15], working upon the leafhopper which transmits the streak disease of maize in East Africa, has found that there exist two distinct races of this insect. One race which can transmit the virus and one race which is unable to do so; these races are termed *active* and *inactive* respectively. There is no visible difference between the inactive and active races and both are of the same species. Further, Storey has shown that if a puncture is made with a fine needle in the wall of the gut or alimentary canal of the inactive insect, the insect then becomes capable of transmitting the virus. It would appear from this that there may exist some factor or factors connected with the structure of the wall of the alimentary canal in inactive insects which prevents the virus from passing through into the blood and so reaching the salivary glands whence it is injected into the plant.

The next point concerns the mechanism of movement of the virus in the plant. Since most viruses rapidly become systemic in their hosts, there is evidently an efficient means of transport about the plant. It has been shown by Bennett [1], Caldwell [2] and others that if the phloem in a portion of the stem of a plant is destroyed by steaming, the virus cannot pass over this bridge of dead tissue. In other words the virus is moving in the phloem but not in the xylem. The disease will develop normally in whichever half of the plant is inoculated, but the virus will not pass from the upper to the lower nor from the lower to the upper half, across the bridge of dead tissue.

The general movement of a virus about the infected plant has been well demonstrated by Samuel [9]. His experiments show that there is no movement of tobacco mosaic virus from the inoculated leaf for a period of 3–4 days. The virus then passes out of the inoculated leaf and travels rapidly to the roots of the plant; about a day later it travels with equal rapidity to the top of the plant. In pot plants the more mature leaves become successively invaded

FIG. 3.—Violas showing a colour variegation considered to be caused by a virus. Note the pencilling and flecking of the petals. (*Photo:* J. P. Doncaster.)

FIG. 4.—Section of a potato leaf with the beak of a sucking insect *in situ*. Insects which feed in this manner are most concerned with virus transmission. (Reproduced from *Ann. Appl. Biol.*)

from the top downwards and from the bottom upwards until the plant is completely invaded by the virus.

The movement of the virus in the plant thus seems to be of two kinds: first, a very slow cell-to-cell movement via the connecting protoplasmic bridges until the phloem stream is reached, when the main and most rapid movement about the plant begins. Further confirmation of this is afforded by some experiments with a newly discovered virus known as tobacco necrosis [13]. This virus produces only necrotic symptoms and thus etches out, as it were, its own movement through the plant. Photographs have been taken at two-day intervals of the path followed by the virus in the leaves of cowpea (*Vigna sinensis*). The first six photographs show merely a gradual increase in size of the lesion at the point where the virus has entered the leaf. As soon, however, as the virus enters the phloem it begins to travel rapidly through the leaf, moving in 48 hours over a much greater distance than in the whole of the preceding 12 days' slow cell-to-cell movement.

On another aspect of the subject two interesting discoveries have recently been made: firstly, it has been found that some plant viruses exist in a number of closely allied strains, and secondly it has been shown that infection with one strain of a virus will immunise a plant from infection with another strain of that virus. Space will not suffice to allow of a discussion as to whether these strains actually arise by mutation from existing strains, but the evidence rather indicates that this is the case.

The immunity conferred upon a plant by a virus strain against other strains of the same virus is of the non-sterile type. There is apparently no question of the production of antibodies and it is the presence of the first virus which inhibits the entrance of the second strain. This type of immunity is well shown in the case of potato virus X [8], tobacco [6] and cucumber mosaic viruses [7] and by the virus of tomato streak [11]. All these viruses exist in strains and the "green" and "yellow" strains of the tobacco or cucumber viruses are particularly suitable for this kind of experiment. If a healthy tobacco plant and one systemically infected with a "green" strain of tobacco mosaic are inoculated with a "yellow" strain, the healthy plant develops the yellow spots characteristic of this virus, while the plant already infected with the "green" strain is protected against invasion by the "yellow" strain. A similar protective action is exerted in the case of a plant infected with a "yellow" strain against invasion by the "green" strain. It should perhaps be emphasised that the presence of one virus in a plant is no bar to the entrance of a second virus

of a different type, the cross immunity holds good only for like viruses and virus strains. This kind of immunity therefore is likely to prove a useful tool in the work of classifying viruses and in distinguishing like from unlike viruses in those cases where diagnosis by symptoms alone is unreliable.

A possible practical application of this type of immunity lies in the protection of a crop from infection with a severe virus by previous artificial infection with a mild strain of the same virus. Here, however, lie a number of pitfalls, chief of which is the unfortunate liability of certain viruses, even when in a mild form, to give rise jointly with another virus of a different type, to a much more severe disease than is produced by either virus acting separately.

Mention must be made of a comparatively new method of approach to the plant virus problem, *i.e.* the discovery that the intraperitoneal injection of rabbits with plant virus extracts induces the production of *antibodies* in these animals. These antibodies react specifically with the *antigen* (virus sap) in some observable way. Three types of immunologic reactions have been demonstrated, complement-fixation, precipitation and neutralisation of the pathogenic properties of the virus. Such neutralisation is specific for each virus, thus, tobacco mosaic virus is inactivated only by anti-tobacco mosaic serum, and tobacco ringspot virus only by anti-tobacco ringspot serum and so on. The cross specificity is absolute and the addition to any of the viruses of a heterologous antiserum exhibits no effect. This specificity, however, does not extend to distinctions between virus strains even when the strains produce very different symptoms in the host plants (Chester [3]).

This new technique is likely, therefore, to prove a useful tool in the difficult task of classifying and differentiating plant viruses.

Since viruses are so often spoken of as filter-passing or ultramicroscopic, and described by other adjectives referring to their small size, it may be of interest to give a few details of the actual magnitude of some viruses. The sizes of virus particles can be measured with fair accuracy by means of ultrafiltration through collodion membranes, the pore size of which can be measured. These membranes are prepared by a special technique devised by Dr. Elford [4] of the National Institute of Medical Research at Hampstead and the process of their manufacture is too complicated to describe here. It has been found by the application of this technique that plant viruses vary very much in their particle size, ranging from 75 to 100 millimicrons for a potato virus down to 17–25 millimicrons for a new tomato virus. The comparative chart

shown in Fig. 5 will give some idea of the range of size of different plant and animal viruses.

In conclusion it is proposed to give a short account of an interesting new virus, because it well illustrates the kind of problem

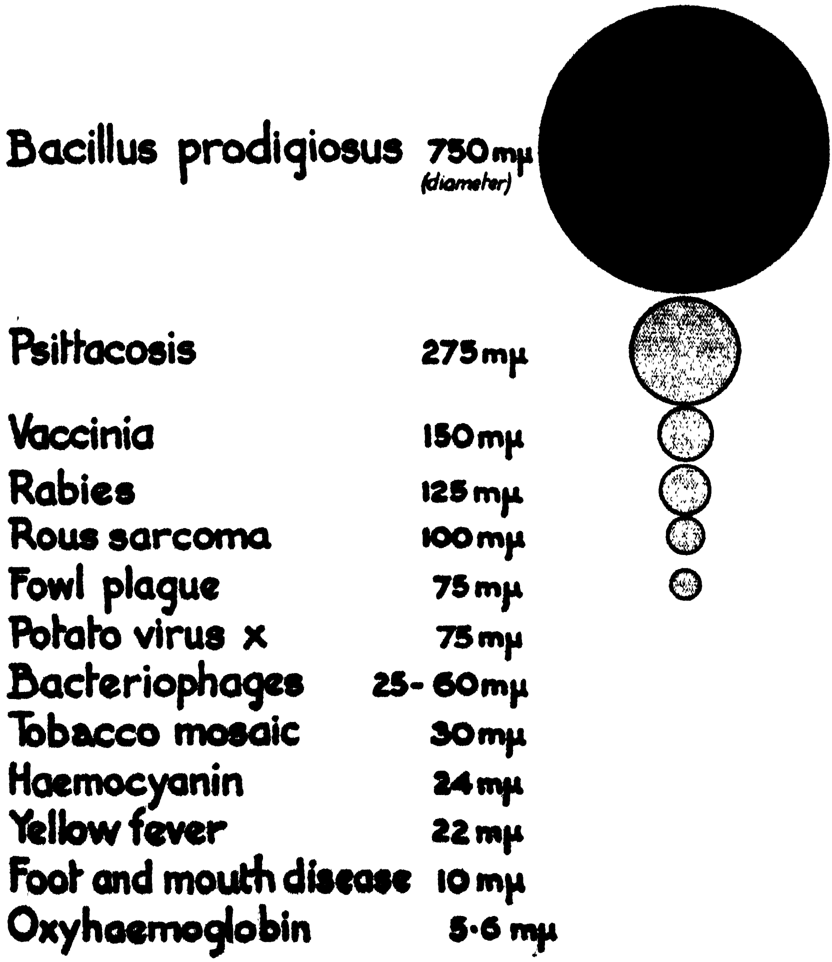

FIG. 5.—Table showing particle sizes of representative animal and plant viruses with those of some bacteria and protein molecules for comparison (1 mμ = 1,000,000th millimetre).

with which the virus worker is sometimes faced. It has been found at Cambridge [12] that a high proportion of the normal stock of healthy tobacco plants carry a virus in the roots but not in the stem and show no signs of disease during the whole of their life. Under certain conditions, however, in the winter and early

spring the virus may pass up into the plant and develop disease symptoms in the lower leaves. Unlike most other plant viruses, this virus does not become systemic in the host. Further, and this is the most interesting point, tobacco seedlings which by available methods of inoculation have been shown to be virus-free, yet contain the virus in their roots in quite large quantities some five weeks later. The following experiment illustrates this. Seed from a White Burley tobacco plant grown in the insect-proof glasshouse was sown in sterilised sand in a "cellophane" cage in the glasshouse. From the resulting seedlings a number of small plants were chosen and all the roots cut off except that one root was left on each plant. The roots of each plant thus removed, were ground up and the resulting paste inoculated separately to three or four cowpeas, a plant which is extremely sensitive to the virus. The tobacco plants were then repotted in sterilised soil and allowed to grow on; from this number 48 plants, the roots of which had given no reaction upon the cowpeas, were selected for a second test. This was made, again to cowpeas, 5 weeks after the first test. The plants were by this time about 8 inches across with a well-developed root system, and showed no unusual symptoms. Of these 48 plants 32 gave a virus reaction. In considering these results certain other facts must be borne in mind; exhaustive tests make the possibility of outside infection by seed-, soil- or water-transmission unlikely, though seed transmission in some form cannot definitely be excluded. The virus is not insect borne.

There seem to be three possible explanations of this problem: first it may be assumed that the virus is present all the time in the stem, but present either in a non-virulent form which requires to gain virulence by concentration in particular cells of the root, or else in a dilution too great to give a positive reaction on inoculation. This theory, of course, involves seed transmission of the virus in undetectable form or quantity. The second possible explanation is that the virus is arising spontaneously within the plant. The third possibility, and perhaps the least likely, is the existence of a mode of virus transmission at present quite unsuspected.

Virus workers have long dallied with the idea that a virus might arise *de novo* within the host. Such a suggestion is attractive in some ways and it would explain many things which are at the moment obscure. If viruses are considered as organisms or at least possessing some of the attributes of life, the suggestion of their heterogenesis is repugnant. If, on the other hand, Stanley's view that a virus may be an autocatalytic protein is accepted, then

there seems no particular reason why the theory of spontaneous development of the virus within the host should not also be accepted. It is, however, at present still an open question and much work remains to be done before this question can be answered.

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THE CONSTITUTION OF THE CLAY MINERALS

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There is, in the Honours schools of chemistry both in this country and abroad, a healthy diversity in the emphasis which is placed on different branches of the science. It is therefore a curious and significant fact that with remarkable unanimity our Honours schools should relegate the chemistry of minerals entirely to the mineralogists, and should devote no small portion of their attention to the detailed structures of minor plant products like the terpenes, the alkaloids and the plant colouring matters. The Honours student in his practical examination would rapidly identify a sample of guaiacol or coumarin, but he would be utterly at sea with a powdered felspar and, indeed, would have some justification for regarding the test as unfair; in spite of the fact that the felspars constitute 60 per cent. of the mineral matter in the crust of the earth. It is worth while considering the reasons for this disparity of emphasis.

The investigation of any new organic compound follows certain well-defined paths. After purification it is analysed; then attempts are made to break up the molecule in as many different ways as possible, and when the fragments have been identified a very pretty exercise in logic enables the chemist to put forward one or more probable structures. These are actively debated, and frequently rival schools set to work to clinch the matter by synthesis. Thus the structure which is finally proved to be correct has behind it not only a logical story, but in many cases also a psychological factor in the clash of opinions which makes a great appeal to student and teacher alike.

Up to very recent times the mineralogist has had little to put alongside the rigid structural formulæ of the organic chemist. Each new mineral has been characterised by its composition and by certain crystallographic constants. The attempts which have been made to assign structural formulæ to the minerals were rightly

regarded by the organic chemists as ludicrously inadequate and speculative since they could be justified by no complete chain of logical reasoning. Moreover, such syntheses as were accomplished added little or nothing to the information from which a structure might be deduced. It is now common knowledge that the handicaps under which the mineralogist laboured have, in the past ten years, largely been removed by the X-ray diffraction technique, which in the hands of the physicists has supplied the basis for valid constitutional formulæ [1]. The chief groups of minerals have already been investigated and in many cases the positions of all the atoms present have been fixed with reasonable certainty. In other cases an incomplete determination only is possible, but the X-ray technique is so powerful a tool that it seldom fails to make clear the main features.

Thus we are now in the position of having for many minerals two sets of data, the older chemical analyses and crystallographic measurements and the newer X-ray diffraction photographs. From the combined mass of evidence it is possible logically to deduce the structural features. Even so, the story has not the same inherent interest as that which the organic chemist can unfold. In the earlier work there was not the intense conscious search for a structural formula characteristic of the organic laboratory and it was often separated by many decades from the newer determinations. There are only a few instances in which interest in structural formulæ has been continuously sustained and where the newer and the older methods have contributed their share at the same time. One of these concerns the chemistry of the clay minerals and it is the main purpose of the present article to show that the proof of the structure of this somewhat recalcitrant group makes a story not unworthy to place beside those which are the chief pride of the organic chemist.

At the present time research on the clays is being actively pursued by groups of investigators having four different objectives. The mineralogists are interested in the characterisation of the clays and in their synthesis in nature from other minerals. The ceramic technologists have their industrial problems to solve and their interest centres chiefly on those clays which provide the raw materials of pottery and brickmaking. The colloid chemists have found in the clays a group of substances with highly developed surface characteristics and they are busily relating these to the internal structure. And finally the agricultural chemists, who for nearly a century have known that a great part of the fertility of the soil is connected with the presence of the clays, have applied them-

selves to the study both of the colloidal and the mineralogical properties of this group and have played no small part in the final elucidation.

The story of the clay investigations falls naturally into two periods, the first up to 1930 and the second ushered in by three contributions from different laboratories in that year. Here we are chiefly concerned with the second period, but it is necessary briefly to review the state of our knowledge at the end of the first.

The vast bulk of the clay work prior to 1930 had been concerned with the kaolin group of minerals, partly because of their technical and partly because of their geological importance. Microscopic methods had differentiated kaolinite ($Al_2O_3.2SiO_2.2H_2O$) from halloysite ($Al_2O_3.2SiO_2.2\text{–}4H_2O$) and allophane ($Al_2O_3.nSiO_2.mH_2O$), since kaolinite frequently occurred in the china clay deposits in the form of crystals whose optical properties could be determined. At this time, however, the microscopic methods were only used for particles larger than 10μ. The finer material was regarded as colloidal and amorphous and was characterised only by its chemical composition and by the shape of the dehydration curve which was of great interest to the ceramic chemists. The difference in dehydration curves between the fine material and the coarse led Mellor to propose the name " Clayite " for the former. The position was not satisfactory, however, and the discordant optical constants for kaolinite obtained by different workers made identification very difficult. In 1931 Ross and Kerr [2] finally removed the discrepancies by showing that three isomers of formula $Al_2O_3.2SiO_2.2H_2O$ exist, one being kaolinite proper and the others comparatively rare minerals known as nacrite and dickite. More recently, they have extended this valuable diagnostic work to halloysite and allophane [3] and have shown that halloysite is essentially crystalline although more finely divided than kaolinite, whereas allophane is amorphous and of indefinite composition. In the course of this work several names formerly ascribed to distinct species have disappeared ; examples are newtonite, pholerite, leverrierite and collyrite.

Two other clay minerals, montmorillonite and beidellite, had also been investigated by Ross and his colleagues in the U.S. Geological Survey. Montmorillonite had already been described and analysed but its importance was not realised until Ross and Shannon [4] showed that it is the major constituent of most of the bentonites. These are compact clays, derived from volcanic ash, of fairly widespread occurrence in North America. The fuller's earths also

consist largely of montmorillonite. The formula proposed was $MgO.Al_2O_3.5SiO_2.nH_2O$, but it was realised that part of the magnesium could be replaced by calcium, sodium or potassium. The bentonite clays are now assuming considerable importance in industry. They are characterised by a high capacity for absorption of water and indeed disperse spontaneously giving a highly colloidal soil when treated with excess of water. They have a very high base exchange capacity, the exchangeable cations being sodium, calcium and sometimes potassium. These interesting properties have attracted considerable attention from the colloid chemists. Beidellite was first described and analysed by Larsen and Wherry in 1917 and its importance has come to be recognised more and more since that date. Found originally in certain gouge clays, it was noted later that a few bentonites consisted almost entirely of this mineral, and since 1930 it has been identified as one of the major constituents of the clay fraction of many soils. Like montmorillonite it is highly colloidal. The formula originally proposed was $Al_2O_3.3SiO_2.nH_2O$.

A mineral which was formerly described as a mica, namely pyrophyllite, $Al_2O_3.4SiO_2.4H_2O$, is now included amongst the clay minerals. It is found crystalline in slates.

Besides these clay minerals composed chiefly of alumina, silica and water, another class consisting of ferric oxide, silica and water had also been examined. Many distinct species were supposed to exist, ranging from the iron analogue of kaolinite known as hœferite ($Fe_2O_3.2SiO_2.2H_2O$) to the nontronites and chloropals containing a higher proportion of silica. Here again the American workers [5] brought some order into the chaos by showing that there exists a complete isomorphous series ranging from beidellite ($Al_2O_3.3SiO_2.nH_2O$) to nontronite ($Fe_2O_3.3SiO_2.nH_2O$). The nontronites tend, on the whole, to be less highly colloidal than the beidellites.

These studies of clay mineralogy were not so widely known as they deserved, because most of those who were concerned with clay materials had to deal with particles too small for identification by the existing optical methods. This was especially true in soil investigations, where the coarser material consists of a complex mixture of the usual rock-forming minerals whilst the clay minerals are scarcely apparent at all until the clay fraction itself, with particles less than 2μ, is reached. Now that a technique has been devised for dealing with very small particles the painstaking work of the American school of mineralogists is more and more deeply appreciated.

Taking the 1930 papers in chronological order we may consider firstly a combined X-ray and optical study of clay minerals and soil colloids by Hendricks and Fry [6]. Using the powder method they carefully compared X-ray diffraction photographs of the pure clay minerals supplied by Dr. Ross with those of colloidal clays separated from a number of different soils. They came to the conclusion that these colloidal materials were all essentially crystalline and that they consisted of minerals such as halloysite or montmorillonite. They were unable to distinguish between montmorillonite and beidellite by the X-ray method. The crystalline character of the soil colloids was confirmed by optical examination of dried flakes, which showed fairly clear interference figures in polarised light. This was due to a partial orientation of these small crystalline particles in the flakes.

Almost simultaneously there was published from this laboratory [7] an optical study of the orientation of colloidal crystals in the electric field. It was shown that particles of colloidal clay show a pronounced double refraction when orientated in this way, the effect being so marked that it could only be explained by assuming that the individual particles were anisotropic crystals. Further, it was found that when one cation was replaced by another by base exchange the double refraction altered. It was therefore concluded that the cations concerned in base exchange have definite places in the crystal lattice of the clays.

Finally, and most important of all from the point of view of structure, L. Pauling published two papers on the X-ray diffraction shown by various classes of minerals with a highly marked cleavage [8]. The list comprised gibbsite ($Al_2(OH)_6$), brucite ($Mg_3(OH)_6$), β-cristobalite (SiO_2), the micas, the chlorites, pyrophyllite, talc and kaolinite. He showed that in all these cases the dimensions of the unit cell in the plane of cleavage range around 5·1 Å and 8·8 Å, the agreement being particularly good for gibbsite and β-cristobalite. Hence he was led to consider all these structures as layer lattices, built up by the superposition of the gibbsite and β-cristobalite units. Similarly brucite layers could take the place of gibbsite layers, but since the brucite layer was slightly larger the unit cell did not fit so accurately and a strain was introduced which might preclude the existence of unsymmetrical types. The types of unit cell used in building up the layer lattices are shown in Fig. 1. Pauling suggested that kaolinite might have an unsymmetrical structure composed of one silica and one alumina layer, pyrophyllite a symmetrical one with a silica layer on each side of the alumina layer (Fig. 2). Talc ($Mg_3O_3.4SiO_2.2H_2O$) is

similar to pyrophyllite, the gibbsite layer of the latter being replaced by a brucite layer. The magnesium analogue of kaolinite ($Mg_3O_3.2SiO_2.2H_2O$) is not known, presumably because of the strain involved in fitting together the unit cells in an unsymmetrical type. The structures proposed were strongly supported by the good agreement between the calculated and observed intensities of the X-ray reflections.

By the end of 1930, therefore, three promising lines of investi-

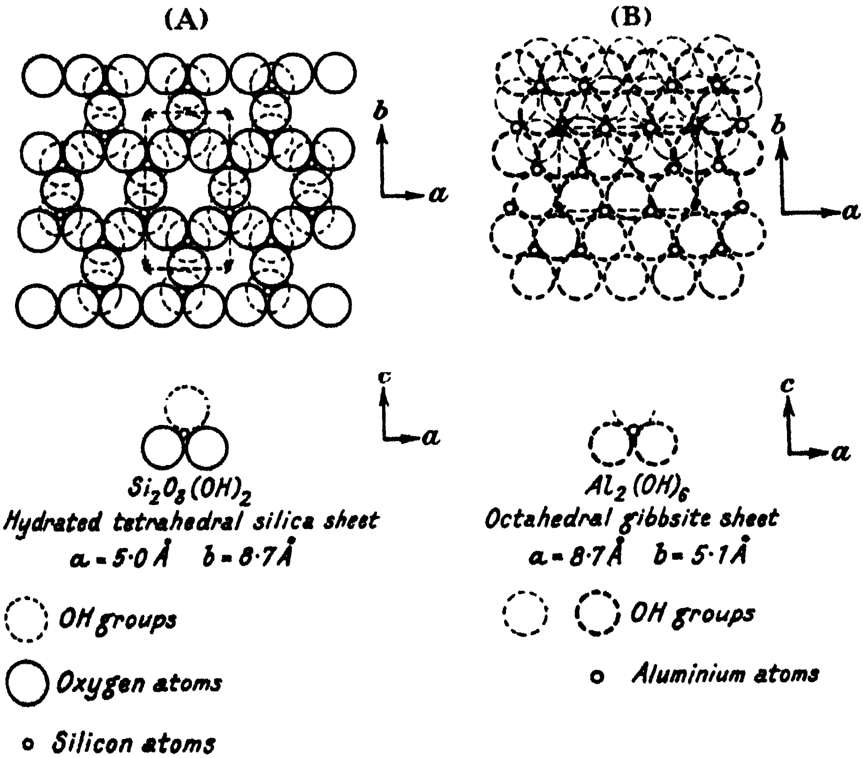

FIG. 1.

gation had been started, and from this time onwards there has been a marked increase in the number of workers engaged. The geologists have attacked the problems of sedimentary petrography with renewed vigour as instanced by the work of Correns and Edelman and their co-workers at Rostock and Wageningen respectively. The soil chemists entered very actively into these new developments as the prospects of solving some of their hundred-year-old problems became clearer. The ceramic chemists also eagerly took up the new weapons. We shall consider the later work

under three headings; the use of the X-ray technique as a means of identification, improvements in the optical methods of identification and finally, determinations of structure by X-ray methods.

The identification of the clay minerals by X-ray powder photographs rapidly developed. Ross and Kerr used it largely in their studies of the kaolin group [2] and halloysite [3]. They were able

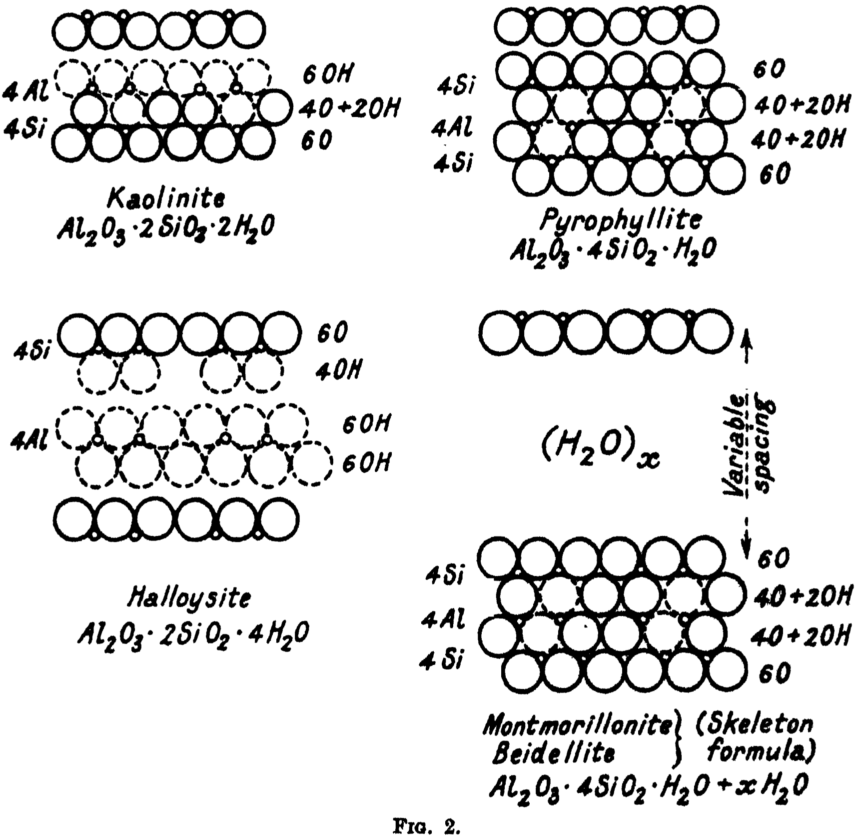

FIG. 2.

Layer Lattices of Clay Minerals.
(Sections perpendicular to the plane of the layers.)

to obtain clear differences between the three isomers kaolinite, nacrite and dickite as well as between each of these and halloysite. Kelley, Dore and Brown [9], like Hendricks and Fry, were able to distinguish different colloidal minerals in soils, and they also broke fresh ground by investigating the effect of fine grinding on various clays. More recently, Nagelschmidt [10] has published accurate comparisons of the X-ray spectra of the chief clay minerals. It is very significant that beidellite and montmorillonite should show

the same lines. Here the optical methods of diagnosis which show clear differences between them are more sensitive than the X-ray methods, which illustrates why the former are never likely to be superseded by the latter. This lack of sensitivity of X-ray compared with optical methods has also been commented upon in connection with other groups of minerals. As regards the relative richness in lines of the various X-ray spectra, this follows the order of crystallinity. The kaolin group shows very sharp lines, halloysite shows fewer, whilst beidellite and montmorillonite have a few more diffuse lines.

The improvements in the optical methods [11] are mainly concerned with the determination of the mean refractive index of small particles by immersion methods, and of their double refraction by electrical orientation. The author has shown that by using a dark ground condenser it is possible to find the refractive index of particles as small as 50 mμ, *i.e.* 0·05 μ.

Coming now to considerations of structure, the impetus of Pauling's work set on foot more detailed determinations in different laboratories. Gruner [12] began with a kaolin group and was able to explain the differences between kaolinite, nacrite and dickite as being due simply to a displacement of the silica layer relative to the gibbsite layer. The positions of all the atoms could not be determined with absolute certainty because of the limitations of the powder method, but there is little doubt that the essential features are correct.

In 1933 was published the first successful attempt to assign a structure to montmorillonite. Hofmann, Endell and Wilm in Berlin made very careful comparisons of montmorillonite from different sources with pyrophyllite [13]. The spectra were identical except for a single line near the axis of the X-ray beam. In pyrophyllite this line had a fixed position and was regarded as a reflection from planes at right angles to the *c* axis; that is, it afforded a measure of the *c* axis of the unit cell. In montmorillonite this line lay much closer to the central beam, its angular deflection being so small that it had been entirely missed by the earlier workers. Thus it appeared that montmorillonite had the same type of layer lattice as pyrophyllite. At any rate, it was certain that it was built up of the same fundamental units, namely the silica and the gibbsite layers, because the measured spacings again gave $a = 5{\cdot}1$ Å and $b = 8{\cdot}8$ Å characteristic of the whole class. Now the innermost interference band changed its position with water content, thereby clearly proving, since the other lines remained fixed, that it was caused by planes parallel to the cleavage whose distance apart

varied with the water content of the clay. By determining the density of the clay and also the spacing at a given water content, it was a matter of arithmetic only to show that the unit cell must contain one gibbsite layer and two silica layers. Since there was such great resemblance to pyrophyllite the symmetrical structure was chosen as the most probable (Fig. 2). This important contribution was followed by a second [14] in which it was shown that beidellite has the same structure, including the variation of interplanar distance with water content. A curious situation was then revealed. The ideal X-ray formula for montmorillonite and beidellite was $Al_2O_3.4SiO_2.H_2O$; the chemical analyses indicated that the former approximated to $MgO.Al_2O_3.5SiO_2.nH_2O$ and the latter to $Al_2O_3.3SiO_2.nH_2O$. The authors suggested that the discrepancy as regards montmorillonite could be removed by assuming a replacement of aluminium by magnesium in the lattice, 3Mg taking the place of 2Al to give a mixed gibbsite—brucite layer, and that the other bases, up to 2 per cent., could be considered as absorbed. The authors were inclined at first to deny the real existence of the beidellite species, but acknowledged that the optical evidence was strongly against them.

It is interesting to note that in 1934 Gruner published an exact study of the X-ray spectra of pyrophyllite and talc [15] which confirmed the structure put forward by Pauling and adopted also by Hofmann, Endell and Wilm.

Mehmel at Rostock completed the series by making a determination of the structure of halloysite [16]. He showed that it was composed of one hydrated silica layer of composition $Si_2O_3(OH)_2$ and one gibbsite layer $Al_2(OH)_6$, the material having at room temperature the composition $Al_2O_3.2SiO_2.4H_2O$ (Fig. 2). At 50° C. however, it loses two molecules of water giving metahalloysite, for which the author also gave a definite structure. The X-ray spectrum was not identical with that of kaolinite, so that we now have four isomers of formula $Al_2O_3.2SiO_2.2H_2O$; kaolinite, nacrite, dickite and metahalloysite.

Thus early in 1935 one could see that the great stumbling block to a general understanding of clay structures lay in the fundamental discrepancy between the X-ray and the chemical formulæ for montmorillonite and beidellite. At this time the present author had just completed a series of complete chemical analyses of clay fractions from a bentonite (chiefly montmorillonite), an American clay soil (chiefly beidellite) and an English clay soil. These clay fractions had been divided into sub-fractions of known particle size and had been carefully studied by the improved optical methods

applicable to small particles [11]. In many cases these sub-fractions consisted of a single clay mineral. What was needed was a method of deducing the layer lattice type from the complete chemical analysis. This could only be done by adopting some reasonably well established rules for the replacement of one atom by another in the lattice. By analogy with the felspars and micas aluminium was supposed capable of replacing silicon in the silica layer, the extra valency required being supplied by an atom of sodium or potassium or half an atom of calcium or magnesium. The replacement of aluminium by magnesium was also regarded as highly probable, as was that of aluminium by ferric iron. Using these as the variables, the most reliable analyses in the literature of montmorillonite, beidellite and the nontronites were recalculated so as to show the range of possible lattice types. When this was done it was found that one type only, the 2 : 1 structure of Hofmann, Endell and Wilm, was common to the montmorillonite and beidellite analyses. Hence the chemical evidence supports the X-ray formula when one takes into account the possible replacement of silicon by aluminium + cations, aluminium by magnesium + cations and aluminium by ferric iron. Using the 2 : 1 structure the replacements per unit cell were then calculated and it was found that beidellite could be regarded as the member of an isomorphous series in which the replacement of silicon by aluminium predominates. Montmorillonite shows a predominating replacement of magnesium for aluminium, but alongside it there is often that of aluminium for silicon. The nontronites are derived by the replacement of aluminium by iron [17]. When this work was carried out there was no X-ray evidence on the structure of the nontronites, but Gruner has very recently studied this group and has again found the 2 : 1 pyrophyllite structure [18]. It is deeply interesting to know that nontronite also shows this variation in the c axis spacing with water content, which Hofmann, Endell and Wilm found for montmorillonite and beidellite.

From the colloid-chemical and agricultural point of view the most important feature of these investigations is the light thrown upon the property of cation exchange in the clays. It has been realised for some time that the base exchange capacity was higher for the montmorillonites and beidellites than for clays of the kaolinite and halloysite types. In the clays with variable c spacing there is room, at high-water contents, for cations to move freely in and out of the lattice. The exchangeable cations present between the lattice layers help to neutralise the charge induced on the lattice by the replacement of Al for Si and of Mg for Al [19].

A valuable opportunity for the discussion of these views with other workers in the same field was afforded by the meeting at Oxford early in August, 1935, of the Third International Soil Congress. Volumes I and II of the *Transactions* (Murby, Vol. I, pp. 428, 28*s*.; Vol. II, pp. 194, 13*s*.) were already printed in time for the meeting and a third volume will follow. These volumes, which cover every aspect of recent work on the soil, were published too early to allow of the inclusion of all the most recent developments in soil mineralogy, although they contain several important contributions. In Volume II mention should be made of the summarising papers by R. Bradfield and F. Hardy.

Besides papers read at the various sessions of the Congress a number of contributions to soil mineralogy were discussed in unofficial meetings arranged amongst those interested. Dr. W. P. Kelley (California) discussed the increase in base exchange capacity and the alteration in the shape of the dehydration curves due to fine grinding of the clay minerals. Professor C. H. Edelman raised some highly interesting points with regard to halloysite. In contrast to many other workers he regards halloysite as a clay of high base exchange capacity and ascribes this to its having free OH groups attached to silicon atoms in the silica sheets. One may suggest, however, the possibility that in the Dutch Clays there may exist a variety of halloysite differing from that characterised by the American workers, in the same kind of way as montmorillonite differs from pyrophyllite; that is, by having a wide spacing along the *c* axis and possibly also a variable composition. There is here considerable room for further research.

In the past ten years the clay group of minerals has risen from a lowly if not neglected position amongst its fellows to one of equal rank. The structural features which have been revealed, some of which, like the expansion of the lattice with increasing water content, are unique in mineralogy, bid fair to ensure that the interest which has been aroused will be maintained for many years to come.

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PHOTOGRAPHY: THE DIRECT REVERSAL PROCESS

By P. C. SMETHURST

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THE popular method of producing a photographic result by making a "negative" image and printing from it a "positive" has very much over-shadowed the possibilities of the direct reversal photograph. "Reversal," as it is called, was until recently the process reserved for amateur films and colour transparencies, but the advent of a professional-size colour film based on the process shows that workers in other branches of photography will have to take more notice of it.

Photography, so far as its scientific basis is concerned, is ultimately a question of producing a system that will absorb energy in the most efficient way possible, and translate it into a permanent result. The energy is that of light, and the absorption system is essentially an emulsion of silver halide particles in gelatine, though organic dyestuffs (notably those of the cyanine series) are added in small quantities to most emulsions in order to stimulate sensitivity to special parts of the spectrum.

The exposure (E) given to an emulsion is defined as $(I.t)$ where I is the light intensity and t the time during which it acts. After exposure, the emulsion is treated with a specially compounded reducing bath, called a developer, and all those particles of silver halide that have been affected by light are reduced to metallic silver, forming a black image. The density (D) of this image at any point is defined as $\log_{10}\frac{I_o}{I_t}$, where I_o is incident intensity and I transmitted intensity. It should be remarked that the relation $D = f(I \times t)$ does not hold good for all values of t, and that the effect of a series of intermittent exposures is not equal to that of one longer one with $t = t_1 + t_2 + t_3 + t_4 \ldots\ldots$ In the cinema these effects may be disregarded, since t remains constant at about $\frac{1}{50}$ of a second, and intermittent exposures are not given.

The "characteristic" of any emulsion is given by plotting

D against log E, and such a curve is shown in Fig. 1. All such curves show a "threshold" effect, in which $dD/d(\log E)$ increases fairly rapidly, a straight centre section, where $dD/d(\log E)$ is constant, and a saturation section where $dD/d(\log E)$ decreases in value to zero. As the Density here reaches a maximum any further increase in log E reduces its value, and the curve slopes once more toward the log E axis, passing through a fairly straight section as it does so. (It is interesting to compare the curve here with that of the grid potential/anode current of a thermionic valve. The similarity in shape is striking, although the two apparently have no connection whatever with each other.)

The straight section of the curve is the most interesting practically, and $dD/d(\log E)$ is measured in this section by taking the tangent of the angle at which this section cuts the log E axis when

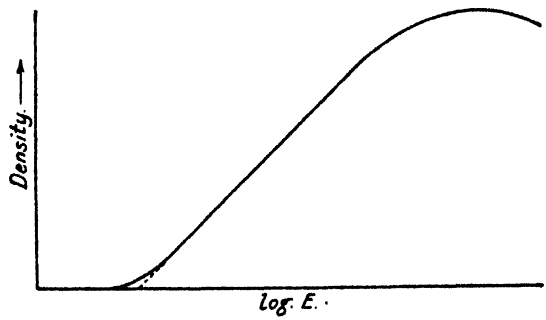

FIG. 1.—Negative characteristic curve.

produced. This value is called "Gamma" or "Contrast Factor" for the particular emulsion and development. This factor increases exponentially to a fixed maximum (Gamma Infinity) as development increases, an approximate relation being: Gamma at time t = Gamma infinity $(1 - e^{-kt})$.

From this curve it will be clear that a series of light intensities reflected from a subject in front of the camera produces a series of densities reversed in sense from the original tones, and that the image will be a "negative" one. In ordinary photography this is "printed" on to a second emulsion and a correct-toned image (the positive) results.

In order to get a direct positive from the original emulsion it might seem possible to expose the latter in the right-hand region of its characteristic curve, where $dD/d(\log E)$ is negative, but the enormous energy required for this makes the suggestion entirely impracticable where exposure times of $\frac{1}{16}$ second are in question. The direct reversal process attacks the problem in quite a different

manner. The exposure given in the camera by no means affects all the silver halide in the emulsion, so that the following procedure is possible:

(1) The emulsion is exposed in the ordinary way in the camera.
(2) The negative image is developed to a high value of $dD/d(\log E)$.
(3) This image is then bleached by treating the emulsion with potassium bichromate or potassium permanganate, in acid solution (H_2SO_4 or HCl is usual). This bleacher entirely removes the negative image, yet does not affect the residual silver halide.
(4) The emulsion is then cleared of decomposition products by washing and by treatment with sodium sulphite solution.

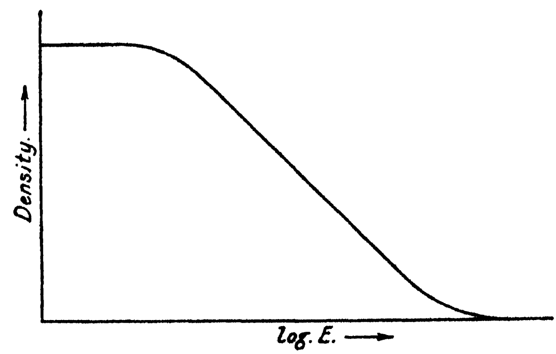

FIG. 2.—Reversal characteristic curve.

(5) Light is now allowed to reach the residual silver halide and, finally,
(6) All the remaining halide is reduced to metallic silver by treatment with a developing solution.

The result of this sequence of operations is clearly to reverse the original densities in the emulsion, and quantitative experiments show a characteristic similar to that of Fig. 2. The "reversal" and correspondence between the two curves is obvious.

Since the reversal process is used almost exclusively for transparencies, such as colour exposures and cinema films, it is clear that the brightest tones of the original subject should have minimum density in the emulsion, so that as little light as possible is lost in transmission. Densities in these "high-light" areas should thus have very low values, but the exigencies of practical considerations make it extremely inconvenient to expose the emulsion to the point where the highest light intensities reach the region of low density at the right-hand end of the curve in Fig. 2. (Time t remains con-

stant, of course.) The fact that it is extremely difficult to expose absolutely regularly to this standard further complicates the issue, and it will be seen that the problem of reversal is that of compensation for inaccurate or insufficient exposure. This consists, essentially, of arranging to push the curve in Fig. 2 toward the left by special treatment, so that consistent results may be possible even under poor conditions of camera exposure. At the same time, such special treatment will be able to correct inconsistencies in camera exposure that are liable to be found even with the most careful determinations of correct exposure.

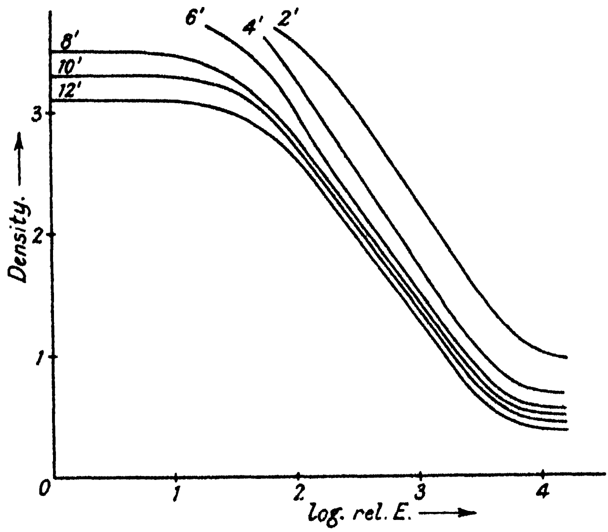

FIG. 3.—Compensation by variation of negative development. Times in minutes.

Though this is more a problem of physical and chemical research than of pure photography, the basic facts already indicated in brief will show the direction in which compensation is possible. In the first place, the quantity of metallic silver produced by reduction in operation (2) may be altered by increased or reduced time of development. The density produced varies approximately as:

$$D_t = D_{\text{infinity}}(1 - e^{-kt})$$

and since there is a finite quantity of silver halide available for reduction, the using-up of more than normal for negative image will leave less than normal available for reduction in (6). This will cause a shift of the curve to the left. Fig. 3 shows the position of the curve for various development times, and it should be noted that $dD/d(\log E)$ remains extremely constant in value throughout.

It is clear from these curves, however, that only a limited shift can be produced by variation in (2), but fortunately the process may be carried to its logical conclusion in another way. If a solvent of the silver halide in use is incorporated in the solution used for first development, the image progressively increases in density as the time increases, while silver halide is dissolved from the emulsion simultaneously. Solvents used in commercial practice for this process are sodium thiosulphate, ammonia, and some of the cyanides, though the first two named are in most general use. The effect on the curve of such treatment is shown by the group of characteristics in Fig. 4. It will be noted that $dD/d(\log E)$ remains reasonably

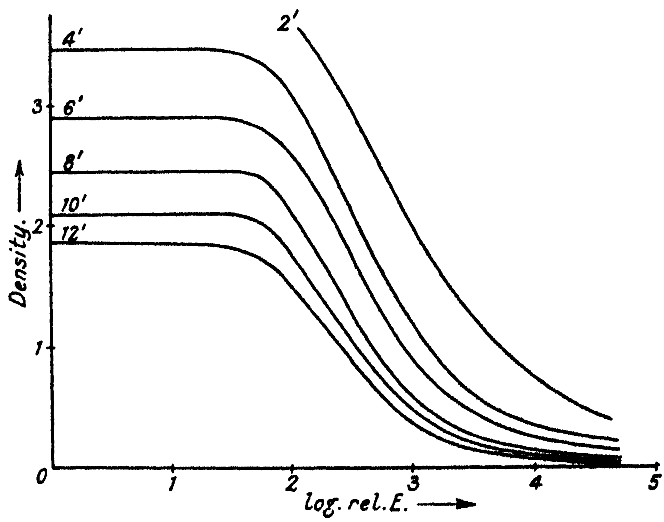

Fig. 4.—Variable negative development plus halide solvent. Times in minutes.

constant, except when extreme compensation is applied, as in the case of the curve marked 12 minutes. The reduction of maximum density in this last curve to slightly less than 2 units may appear to prejudice the appearance of the image, but it must be remembered that a density of 2.0 implies a transmission of only 1 per cent. In practice this does not affect seriously the pleasantness of the image. It should further be noted that the minimum densities in Fig. 4 are less than those in Fig. 3. This implies a more transparent image, which is a practical advantage.

Both of these compensation processes have dealt with variations in first development, but there are other possibilities inherent in reversal as a whole. For example, when the negative image has been reduced and bleached out, it is quite practicable to treat

the emulsion with a solvent for the halides, and to remove excess, thus lightening the final image, and shifting the curve to the left. The curves in Fig. 5 apply to this method, and it will be seen that the limiting factor is the maximum density which it is required to produce in the positive image. On the other hand, the minimum densities are not very low unless a considerable treatment with the solvent is given. In spite of these disadvantages, however, this process is commercially used by at least one firm, and excellent results are possible.

An even more ingenious method of compensation is provided by altering the second exposure. There is no need whatever

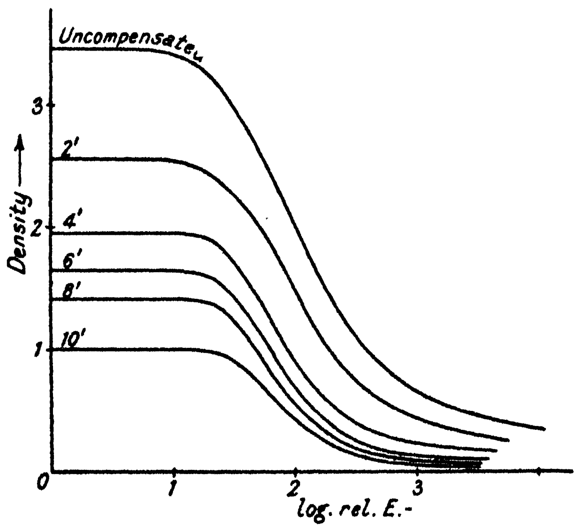

FIG. 5.—Removal (by sodium thiosulphate) of excess halide after bleaching negative image. Times in minutes.

to expose all the remaining halide in the emulsion, since after development that which is unreduced can be removed by sodium thiosulphate in the normal photographic manner. A patented process for automatically giving the correct exposure according to the quantity of residual halide has proved to be entirely practical, and in spite of theoretical objections to the scanning of the emulsion by photo-cell, films processed in this way are probably more consistent than those treated by other methods. The curves for variation of second exposure are given in Fig. 6. The particular advantage given by such automatic scanning and adjustment of second exposure is that various degrees of inaccuracy in exposure are compensated within a single length of film. This is most useful

in practice, since it is seldom convenient to cut a strip into short lengths for individual treatment in process.

The reason why such an automatic compensation is most valuable

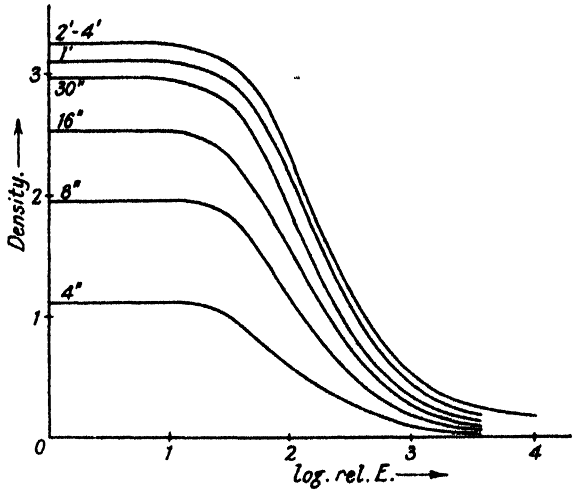

FIG. 6.—Variable second exposure. Times in minutes and seconds.

may be seen from Fig. 7, where fictitious curves are shown for one particular film. The normal exposure meter finds the average intensity of light reflected from subjects in front of the camera,

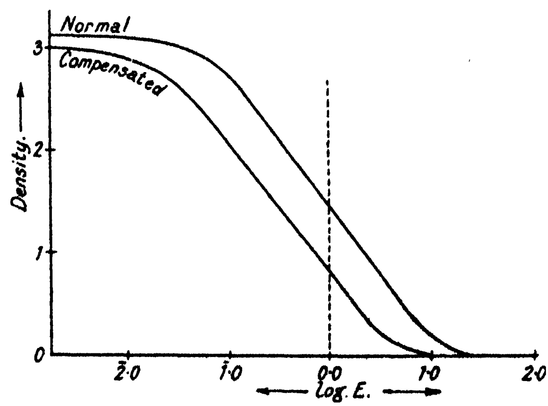

FIG. 7.—Effect on subject on exposure.

and arranges to give a reading whereby this average intensity can be placed on the centre of the straight section of the characteristic. This is shown by dotted line in the figure. Suppose now that two subjects are to be recorded, each with the same average reflected

intensity, but varying in *range* of intensities by a considerable amount. The subject with intensities 3 times average to $\frac{1}{3}$ average will clearly be under-exposed if placed on the normal curve shown in the centre, for the brightest intensities should reach to the minimum density possible, and thus compensation should be applied for this particular exposure, when the curve on the left would be operative. The long-range subject, however, with intensities varying between 10 times normal and $\frac{1}{10}$ normal, would be quite correctly exposed for "normal" processing. Yet according to the meter, both these subjects have been given accurate exposure. Were they developed together, either the short-range subject would be very much too dense, or else the long-range subject would be far too transparent, as regards the positive image.

This simple fact shows that the measurement of the average intensity of a subject cannot apply to reversal work. Instead, it is essential that the high-lights of the subject should be measured, and from the result these maximum intensities may be placed consistently on the right-hand end of the curve. If this is done, exposure is always nearly correct, and processing of the film may be standardised. Unfortunately, at the present time there is little appreciation of this point among those who are used to working with the negative-positive system (where the average intensity is a reasonable measure of correct exposure) and until practical "cameramen" study the reversal process and realise the special measurements it involves, the unfortunate developing staff will find that standardisation is impossible.

Once, however, the mechanics of the process are grasped, the rest follows without trouble, and the reversal process will be found as simple in work as the more ordinary negative-positive system. In the above treatment of the subject I have to acknowledge the kindness of Dr. Lummerzheim, of I. G. Farbenindustrie, Berlin, for allowing the reproduction of the curves in Figs. 3–6, which were originally printed in his article "Das Umkehr-Problem."

THE BIOLOGICAL DECOMPOSITION OF LIGNIN

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One of the many controversial subjects in soil microbiology is the question of the decomposition of lignin. It is a peculiarly intriguing problem because the experimental evidence is so contradictory, and often is apparently in conflict with the rational deduction that unless lignin decomposes or undergoes some modification it would, with the passage of time, have accumulated in all soils receiving plant remains, to such an extent as to be easily recognisable by chemical means. On the other hand, merely as a matter of observation some " woody " or highly lignified materials decompose but slowly, if at all, even in relatively favourable circumstances. Sawdust, for example, may remain in heaps in the open under moist conditions and undergo no obvious change for many months or years, save, perhaps, a blackening of the outer layers. There is no doubt that the physical and environmental conditions of the material enormously affect the rate and extent of microbial attack, but even under controlled and optimum conditions in the laboratory, there is uncertainty as to the availability of lignin.

It is proposed here briefly to review the more recent work bearing on the decomposition of lignin and lignified materials. A distinction must be made between lignin in plant materials, and lignin in intact timber, for in the latter special conditions prevail.

Aerobic Decomposition of Lignin in Plant Materials

In his detailed and extensive experiments on the decomposition of plant materials and the formation of humus in soils, S. A. Waksman has been responsible for much of the newer information on the availability of lignin. Waksman and Tenney (1927) in examining the effect of the age of the plant on the rapidity of its decomposition stated that lignin decomposes " less rapidly than any of the major plant constituents and tends to accumulate in the soil." The same authors in 1928 emphasised the great resistance

of lignin to decomposition under normal aerobic conditions. In 28 days under optimum conditions the lignin of rye straw suffered very slight loss, and that of oak and lucerne was unaffected, even though the materials themselves underwent extensive decomposition. Rege (1927) expressed the opinion that lignin is an "inhibitory factor" in decomposition, being the most resistant of the tissue constituents, and therefore acting as a physical barrier to the attack of micro-organisms. His views have been misconstrued by later workers, who have read into his statements an active inhibitive effect of lignin, whereas a passive rôle is certainly all that was implied. Waksman, Tenney, and Stevens (1928) studied the transformation of organic matter in forest soils, and analysed oak leaves which had been allowed to rot for 6 and 12 months. Lignin was found to be the most resistant constituent. After six months' decomposition, there was only a very limited reduction in the lignin content, but after 12 months the decomposition was "more definite," a loss of 45 per cent. being recorded when $CaCO_3$ was added, or 35 per cent. in its absence. Tenney and Waksman (1929) gave analyses of materials such as corn stalks, rye straw and oak leaves, which had been allowed to decompose under aerobic conditions for a much longer period than in their earlier studies. Definite losses of lignin were observed in all cases, but much less in proportion than any other constituent. Nevertheless, this paper records the first satisfactory evidence of anything approaching an extensive decomposition of lignin. Their findings are summarised in Table I.

TABLE I

DECOMPOSITION OF LIGNIN *

[*from Tenney and Waksman* (1929)]

	Per cent. of Apparent Lignin Remaining.			
	27 Days.	68 Days.	205 Days.	405 Days.
Corn stalks only	102·9	79·4	60·9	42·9
+ added nutrients	82·7	69·5	56·0	42·5
Lucerne plants only	97·7	80·4	78·1	57·4
+ added nutrients	—	89·5	70·3	61·2

	66 Days.	143 Days.	386 Days.
Rye straw only	99·5	85·5	79·7
+ added nutrients	98·7	76·7	69·6
Oak leaves only	102·5	99·7	85·0
+ added nutrients	101·4	94·2	74·7

* Lignin determined by Schwalbe (1925) method.

In the preparation of composts it was stated [Waksman, Tenney

and Diehm (1929)] that losses of lignin are slight, and similarly that in horse manure allowed to rot for ten months the lignin was reduced by only 20 per cent. [Waksman and Diehm (1929)]. Rather different conclusions were arrived at by Waksman and Gerretsen (1931) in an extensive investigation of the effect of environmental or climatic conditions on the decomposition of straw in composting. They found a marked influence of temperature on the decomposition of lignin ; at low temperatures the losses were small, but at 37° C. they were quite extensive, as seen in Table II.

TABLE II

EFFECT OF TEMPERATURE ON THE DECOMPOSITION OF LIGNIN IN OAT STRAW *

[*from Waksman and Gerretsen* (1931)]

Initial moisture content, 80%. Nutrient salts added

Temperature of Incubator.	Per cent. of Apparent Lignin Remaining.			
	16 Days.	48 Days.	105 Days.	273 Days.
7°	—	—	94·0	104·4
18°	110·0	99·5	72·5	86·5
27°	101·9	85·3	82·9	78·0
37°	86·5	76·4	49·7	41·6

* Lignin determined by Schwalbe (1925) method. (?)

Other workers who have found an extensive decomposition of lignin under aerobic conditions are Phillips, Wiehe and Smith (1930). Their experiments were only comparatively brief in duration, and the percentage losses of lignin recorded were in some cases higher than those of cellulose. It is impossible to regard their results and conclusions as being in line with the majority of the work carried out on similar materials. Table III summarises their findings.

TABLE III

DECOMPOSITION OF LIGNIN IN VARIOUS MATERIALS *

[*from Phillips, Wiehe and Smith* (1930)]

Materials.	Per cent. Loss of Original Lignin.		
	36 Days.†	25 Days.‡	62 Days.‡
Corn stalks	0·2	25·9	29·0
Oat hulls	5·4	8·1	9·3
Corn cobs	17·0	41·0	45·1
Wheat straw	39·8	48·3	34·3

* Lignin determined by Willstatter method. † 10 g. material taken originally. ‡ 25 g. material taken.

In the same year Osugi and Yoshie (1931) followed the decomposition of a number of materials such as soy bean cake, rape

seed cake, young vetches, rice straw, and in a period of 45 days found no significant losses of lignin. Martin (1933) found no large loss of lignin in decomposing lucerne and clover, tops and roots, even after 270 days. Allen, Abel and Magistad (1935) studied the rotting of pineapple trash in soil for periods up to 15 weeks and found no apparent loss of lignin.

Quite recently Smith and Brown (1934) took samples of oat straw composted with added nitrogenous salts over a period of fifteen months. They state that at the end of this time the cellulosic and furfuraldehyde-yielding constituents had largely disappeared and that 60–90 per cent. of the lignin had decomposed. Losses almost as great were recorded by Smith, Stevenson and Brown (1930) on similar straw composts. The temperature conditions of Ames, Iowa, where these experiments were performed are in summer much higher than in this country, and it is possible that the results fall into line with those obtained by Waksman and Gerretsen (1931) on the effect of higher temperatures.

In all the investigations described hitherto the active microbiological agency has been a mixed flora. Some information of a rather interesting nature has been obtained on the action of certain specific fungi on the lignin of plant materials. Waksman (1931) showed that *Coprinus radians* inoculated on to raw horse manure removed about 22 per cent. of the total lignin and 70 per cent. of the cellulose in 51 days. The common mushroom, *Agaricus*, utilised 18 per cent. of the lignin and only 14·5 per cent. of the cellulose in an equal period. Further examinations of the action of *Agaricus campestris* on the straw or manure compost upon which it grows were made by Waksman and Nissen (1931, 1932) and Waksman and McGrath (1931). These confirmed the previous observation that this fungus develops to a considerable extent at the expense of the lignin of the compost. The actual loss of dry matter may not be great, since the mushroom synthesises considerable quantities of mycelium. An earlier paper by Falck (1930) claims that fungi may act in two ways on plant materials. Either they may attack celluloses and hemicelluloses, leaving the lignin unaffected, or they may attack all three groups. The residual material will in the first case be of a higher carbon content than in the second. *Agaricus nebularis* was shown experimentally to attack lignin and consequently to fall into the second category. Other evidence will be given later, when dealing with the rotting of timbers, to support the view that the Basidiomycetes as a group are capable of decomposing lignin.

Very few pure cultural studies have been made with other

organisms and this is a field of work which would repay investigation. Waksman (1931) inoculated pure cultures of a few organisms, among them *Zygorhynchus*, *Trichoderma*, a *Penicillium*, *Humicola*, an *Actinomyces*, and *Spirochæta cytophaga*, on to sterile wheat straw and none of these showed more than very slight lignin-decomposing ability. The time of incubation was, however, too brief in the light of the observations made earlier. Of the action of bacteria on lignin nothing is known.

Aerobic Decomposition of Isolated Lignin

A number of attempts have been made from time to time to study the action of micro-organisms on lignin preparations. It must not be assumed that the results obtained with isolated lignin are necessarily applicable to lignin *in situ*. All methods of preparation involve the use of drastic reagents, usually concentrated acids, which may effect considerable change in the lignin molecule. There are reasons for supposing that lignin is originally in an enolic form, stabilised by linkage to carbohydrates, and that when isolated it undergoes tautomeric change to the keto-condition [Harris, Sherrard and Mitchell (1934)]. Many lignin preparations, including some used for constitutional work, have not been free from carbohydrates, and in consequence some caution has to be used in interpreting results obtained with such material. For example, the acceleration in the fermentation of cellulose by *Microspira agar-liquefaciens* produced by the addition of lignin [Gray and Chalmers (1924)] was probably due to the presence of a small amount of pentose in the lignin, since xylose was also shown to have a favourable effect.

Pringsheim and Fuchs (1923) prepared lignin by acidification of an alkali extract of wood made under pressure. Their product contained over 5 per cent. pentosan. On suspending in water and adding nutrient salts, gas production followed inoculation with soil. At the end of 8 days only about 65 per cent. of the lignin could be recovered whereas in control experiments 90 per cent. was reprecipitated. The material precipitated after fermentation was significantly lower in methoxyl content, and contained only 0·5 per cent. pentosan. While this would seem to justify a claim that isolated lignin is attacked by micro-organisms, the question does not seem to have been pursued further. No mention was made of the types of organism present.

Waksman and Tenney (1926) prepared lignin from straw and lucerne meal by treatment with 72 per cent. sulphuric acid, to which had been added a little HCl [Schwalbe (1925)]. Small quantities

were added to fresh soil, and the CO_2 evolved on incubation determined. From the barley straw lignin in 32 days, only 3 mg. CO_2 per 1 g. lignin in excess of the control experiments was obtained even in the presence of added nitrogen. Lucerne lignin yielded only 2 mg. per g. in an equal period. They concluded that isolated lignin is unavailable to micro-organisms. Recently, Smith and Brown (1935) have undertaken a similar investigation. Quantities of lignin equivalent to 300 mg. C. were added to 100 g. of soil, brought to a moisture content of 25 per cent., and sterilised on three consecutive days. These flasks were inoculated with pure cultures of *Trichoderma lignorum*, *Aspergillus terreus*, and *Penicillium vinaceum*, and incubated at 30° C. The CO_2 output was determined at intervals up to 21 days, and was not significantly greater with these three organisms than from the controls of uninoculated soil. In sand cultures neither *Aspergillus niger* nor a mixed soil infusion produced any apparent decomposition of lignin as measured by CO_2 output. In solution cultures a small output of CO_2 was obtained from a soil infusion in 15 days, representing only about 2·3 per cent. of the added lignin. Oxidised lignin preparations were found to be slightly more available to *Stereum purpureum* in sand culture, and some evidence was obtained that the addition of this lignin to cellulose stimulated the decomposition of the cellulose, or possibly vice versa. From the work of these investigators normal isolated lignin must be regarded as biologically very resistant to the ordinary soil microflora.

Anaerobic Decomposition of Lignin in Plant Materials, and of Isolated Lignin

Studies on anaerobic decomposition have frequently been concerned with gas production, and the utilisation on an industrial scale of cellulosic waste materials. Fermentations have usually been carried out in the presence of a large excess of water. Lymn and Langwell (1923) carried out many fermentations with thermophilic organisms at 60–80°, finding lignified cellulosic materials to be the most resistant, and expressing the opinion that lignin is unaffected by anaerobic organisms.

Waksman, Tenney and Stevens (1928) in considering the formation of forest soils carried out anaerobic fermentations of oak leaves at 28° C. At the end of 6 months, the maximum loss of lignin was less than 1 per cent., an insignificant amount, and at the end of 12 months did not exceed 10 per cent. Tenney and Waksman (1930) extended these experiments by following the decomposition of waterlogged materials, such as corn stalks, rye straw, oak leaves,

and lucerne plants. Some variation in the behaviour of the lignin was observed. Only in the case of the corn stalk was there any considerable loss of lignin in the experimental period of nearly a year and a half.

TABLE IV

ANAEROBIC DECOMPOSITION OF LIGNIN *

[*from Tenney and Wakeman* (1930)]

Material.	Per cent. of Apparent Lignin Remaining.		
	27 Days.	135 Days.	498 Days.
Corn stalks	103·6	92·3	67·3
Lucerne plants	101·2	98·6	99·6
	84 Days.	168 Days.	491 Days.
Rye straw	91·3	87·6	86·2
Oak leaves	101·0	105·5	107·5

* Lignin determined by Schwalbe (1925) method.

Boroff and Buswell (1929) described conditions under which corn stalks could be rapidly fermented, and later claimed that lignin was unquestionably decomposing and furnishing a portion of the gases obtained [Boroff and Buswell (1930)]. The same workers (1934) concluded that isolated lignin cannot be completely or extensively fermented by a natural anaerobic flora, and stated further that they had evidence of a bacteriostatic action, since, on its addition to an actively fermenting glucose medium, gas production instantly ceased and could not be revived by further additions of glucose. They held, however, that the resistance of isolated lignin is a result of some modification in its preparation, and that in the natural conditions it is available.

Very recently Levine, Nelson, Anderson, and Jacobs (1935) have made attempts to isolate organisms capable of attacking lignin under anaerobic conditions. By adding isolated lignin to active fermentations in which lignin was presumably being decomposed, they hoped to obtain a substrate with a rich specific lignin flora. Their attempts were unsuccessful, because the addition of isolated "alkali" lignin to actively digesting cornstalk flour or to packing house sludge immediately checked the fermentation as indicated by gas production. The lignin had apparently a marked restrictive action, interfering and checking the progress of fermentation. This inhibitory effect was shown to be due to the lignin itself, but not due to any toxic action on the bacterial flora. They prefer the

view that this effect may in part be due to a precipitating action of lignin on protein, as shown by Waksman and Iyer (1932) with the production of complexes, resistant to microbial attack. The final conclusion reached by Levine *et al* (1935) is that they were unable to substantiate the claims made by Boroff and Buswell that under anaerobic conditions lignin decomposition proceeds as rapidly as that of cellulose or hemicelluloses.

Acharya (1935, i) at Rothamsted has investigated the anaerobic decomposition of rice straw by mesophilic organisms over a period of several months, and has found clear evidence of the fermentation of lignin, though not to such an extent as claimed by Boroff and Buswell (1934). Typical results are given in Table V.

TABLE V

ANAEROBIC DECOMPOSITION OF LIGNIN IN RICE STRAW *

[*from Acharya* (1935)]

	Percentage Losses.									
	1 Month.		2 Months.		3 Months.		4 Months.		6 Months.	
	Org. Matter.	Lig.	Org. Matter.	Lig.	Org. Matter.	Lig.	Org. Matter.	Lig.	Org. Matter.	Lig.
With $(NH_4)_2CO_3$	18·6	12·9	21·3	15·4	28·2	21·5	32·8	25·5	43·4	27·9
With $NaNO_3$.	18·6	19·8	24·3	20·2	—	—	37·3	20·3	39·4	23·1

* Lignin determined by Norman and Jenkins (1934) method.

The same worker [Acharya (1935, ii)] has given a particularly interesting series of results comparing the losses of constituents, including lignin, of rice straw rotted under anaerobic, waterlogged, and aerobic conditions at 30°. The greatest loss of lignin occurred in the waterlogged straw. The lignin figures are summarised in Table VI.

TABLE VI

COMPARISON OF LOSSES OF LIGNIN FROM RICE STRAW UNDER ANAEROBIC, WATERLOGGED AND AEROBIC CONDITIONS AT 30° *

[*from Acharya* (1935, ii)]

	Percentage Losses of Lignin.			
	2 Weeks.	1 Month.	3 Months.	6 Months.
Anaerobic	6·7	13·2	21·8	28·5
Waterlogged	17·4	22·2	30·4	33·1
Aerobic	15·0	21·6	26·0	29·8

* Lignin determined by Norman and Jenkins (1934) method.

In a subsequent survey of the anaerobic fermentation of a number of other materials under optimum conditions, Acharya (1935, iii) found no more extensive losses of lignin than in rice straw, and indeed, in some cases, considerably less, as seen in Table VII. The weight of this evidence appears to be in conflict with the view of Boroff and Buswell (1930) on the extensive fermentation of lignin under anaerobic conditions. For reasons which will appear later this work must be given special consideration.

TABLE VII

ANAEROBIC DECOMPOSITION OF LIGNIN IN PLANT MATERIALS *

[*from Acharya* (1935, iii)]

Material.	Original Lignin Content. %	Percentage Loss of Lignin in 6 Months.	
		Without added N.	With added N.
Rice straw	9·12	27·6	28·5
Oat straw	13·31	23·3	26·3
Wheat straw	12·60	15·4	23·0
Barley straw	11·88	11·6	16·3
Bracken	21·72	3·8	4·0
Young grass	10·46	10·1	11·7
Rape cake	7·47	7·5	7·5

* Lignin determined by Norman and Jenkins (1934) method.

DECOMPOSITION OF LIGNIN IN WOOD

No attempt will be made to present in detail an account of the biological decomposition of timber. Wood is on the whole resistant to the attack of most common bacteria and fungi, but is susceptible to many of the members of the Basidiomycetes, and, of course, to boring by insects. Bray and Andrews (1924) were the first to give accurate analyses of the changes produced in wood on decomposition, and to show that there was considerable loss of cellulose, and some loss of lignin after attack by a number of fungi. The lignin content of rotted wood is considerably higher than that of sound wood, because of the greater removal of cellulose. Certain organisms, particularly *Trametes pini*, have been stated to attack the lignin even more rapidly than the cellulose. As a result, wood-destroying fungi were divided rather arbitrarily into those producing " brown rots " and those producing " white rots." The former were stated to attack the cellulose primarily, leaving lignin, and the latter to remove lignin, though not exclusively. Campbell (1930) has carefully investigated the very interesting " white rot " produced by

Polystictus versicolor and shown that this fungus attacks first the lignin and hemicelluloses, but later removes some cellulose as well as lignin. He has shown further (1931) that it must not be assumed that the lignin has been removed if the rotted wood is white in colour. *Polyporus hispidus* and *Stereum hirsutum* act in a somewhat similar way to *Polystictus*, but do not appear to have such a selective action on lignin.

The prevailing view therefore is that wood-destroying fungi that attack lignin, invariably remove carbohydrates as well.

THE CONFLICT IN VIEWS AS TO THE BIOLOGICAL AVAILABILITY OF LIGNIN

The account given above is sufficient to demonstrate the very controversial nature of this question. Claims are made that under both aerobic and anaerobic conditions, the availability of lignin varies from nil or almost nil to 100 per cent. Such a variation is, of course, unlikely even if the widely different experimental conditions be taken into account. An explanation has to be found elsewhere, and is provided by the difficulties inherent in the accurate determination of lignin, which are insufficiently regarded by most workers in this field. Methods which under a particular set of conditions might give a satisfactory result have been employed on materials which are quite unsuitable unless special precautions are taken. This is no place to give a detailed review of lignin methods, practically all of which depend on the resistance of lignin to concentrated acids. Cellulose and other carbohydrates are dissolved and subsequently hydrolysed away leaving an insoluble residue weighed as lignin. Both 72 per cent. H_2SO_4 and 42 per cent. HCl have been employed. The former is more convenient to handle, and in a modification suggested by Schwalbe (1925) was used by the majority of workers until the past few years. Recently several alternative procedures have been advocated, which give results by no means identical. Their use accounts in part for some of the divergencies of opinion.

There are two major sources of error in the determination of lignin, and probably other subsidiary ones. Major errors may be due (1) to the presence of certain carbohydrates, and (2) to the presence of protein, in materials upon which lignin estimations are carried out.

(1) The purpose of treatment with strong acid is, as mentioned above, to effect solution and hydrolysis of cellulose and other carbohydrates. However, it has been shown that pentose sugars, par-

ticularly xylose, on standing alone with strong acid yield an insoluble black precipitate [Norman and Jenkins (1934), Hilpert and Littman (1934)]. The amount produced varies directly with both increase of temperature, and time of contact with the acid. It may be minimised by keeping the temperature below 20° C. and the time of contact down to 2 or 3 hours. The addition of a pentose sugar to any material will increase the apparent lignin content unless special precautions are taken. Now practically all plants contain pentose units in the encrusting hemicelluloses, or associated with the cellulose as "cellulosan" [Hawley and Norman (1932)]. In some, *e.g.* corn stalks or cereal straws, the pentose content is of the order of 20 per cent. or more. There is no doubt that this pentose material reacts in the same way with strong acids as does a pentose sugar alone. In consequence a fictitiously high lignin content may be obtained. Some light has been thrown on the nature of the disturbance caused. Norman and Jenkins (1934, i) demonstrated the slow production of furfuraldehyde from pentose groups in the presence of strong acid, and, in later work, the immediate condensation of aldehyde with lignin to give a stable lignin-furfuran resin. The lignin has only a limited combining power for aldehyde. Once this is satisfied further furfuraldehyde condenses with loss of water to give dark brown insoluble derivatives, as observed on treatment of xylose with acid. In ordinary circumstances the amount of lignin present in mature materials could combine with more aldehyde than would be produced from the pentose.

To avoid error due to this cause, one of two courses may be followed. Either the plant material may be submitted to a preliminary hydrolysis with dilute acid to remove pentose-containing hemicelluloses, as suggested by Norman and Jenkins (1934, i) or else the time of contact with the strong acid must be as brief as possible, and not more than 2 hours, as proposed by Ritter, Seborg and Mitchell (1932). Neither treatment is yet accepted beyond doubt; the former because the action of the dilute acid on lignin itself is still not clear, and the latter because the pentose effect is not entirely excluded. Either method gives a much more satisfactory figure than the Schwalbe (1925) method, or that later proposed by the U.S. Forest Products Laboratory and modified by Sherrard and Harris (1932). It may be noted that the procedure suggested by Waksman and Stevens (1930) and used by Waksman since that date, is not subject to any error from pentose groups since a prehydrolysis with acid forms a part of the method.

(2) The disturbance introduced by the presence of protein in

materials on which lignin determinations are carried out is at present more obscure. If proteins are allowed to stand for 16 hours with 72 per cent. H_2SO_4, and the solution is then diluted and boiled, no precipitate is obtained. If they are added to pure cellulose, and similarly treated, the precipitate is negligible. But if added to a lignified material, the apparent lignin content is increased and the lignin residue contains nitrogen. This was recognised by Paloheimo (1925) who stated that all lignin figures obtained on nitrogenous materials should be corrected by subtracting an amount equal to the nitrogen content of the lignin multiplied by the protein factor, 6·25. This recommendation was subsequently adopted by Waksman and Stevens (1930) in their method. It, however, involves the assumption that the nitrogen in the lignin residue is present as protein, an assumption which has been shown by Norman and Jenkins (1934, ii) to be incorrect. The increase in apparent lignin is probably due to the linkage of protein fission products with the lignin. No regularity or proportionality in effect was found, small quantities of protein producing increments of greater magnitude than larger quantities. To confuse the issue still further, it was found that if both protein and pentose are present there is an interaction which enhances the disturbance. To apply a correction by calculating the nitrogen in the lignin as protein is likely to introduce, in some cases, an error greater than that it is desired to correct. So far, no satisfactory method of avoiding these difficulties has been devised. Acid pretreatment, as recommended for minimising the pentose disturbance, is partially successful in that the protein content of the original material is considerably lowered, and any possibility of protein-pentose interaction avoided by the removal of the latter.

Now bearing in mind these two sources of error in the lignin determination, it is possible to see why divergent conclusions have been arrived at as to the availability of lignin. Information on this point has naturally been sought by carrying out lignin determinations from time to time during decomposition. Because of the fermentation of other constituents, this involves determinations on materials which are changing in composition. Under aerobic conditions the changes are particularly concerned with just those two groups which introduce errors into the lignin determination. Hemicelluloses, containing pentose units, are rapidly attacked and removed, whilst protein is synthesised by the micro-flora, and accumulates. Unless special precautions are taken on the lines mentioned above, the lignin determination is affected by errors which, to some extent, may be compensatory as decomposition proceeds. In general, however,

the pentose disturbance outweighs the protein disturbance, and without there being of necessity any real change in lignin content, a wholly illusory apparent loss of lignin may appear merely as a result of the fermentation of the hemicelluloses. Under anaerobic conditions this is even more likely, since protein synthesis by the flora is small. This fact renders the results of Acharya (1935) the more significant.

It is not implied that all the results recorded in the previous pages are valueless, because in many cases the losses of lignin obtained were far larger than the possible experimental errors in its determination. It is, however, certain that small differences cannot be regarded as significant, and that in future work very much more attention must be given to the method of determination of the lignin. The need for this has been pointed out recently by Levine *et al* (1935). Their determinations of lignin were made by a method [Peterson, Walde and Hixon (1932)] which does not avoid the disturbance due to pentose. They found considerable difference in the apparent lignin content of fermented cornstalk flour before and after aqueous extraction, which is probably in part due to the effect of removal of a portion of the pentose material. They ascribe it to a change in the physical or chemical state of a part of the lignin, rendering it extractable or so colloidal as to escape recovery.

The importance of a proper consideration of the method of estimation of the lignin in studies on its decomposition may be seen from Table VIII, in which is given the results of analyses carried out by four different methods on samples of oat straw rotted aerobically under optimum conditions for periods up to 1 year. It will be noticed that as a result of the rapid synthesis of microbial protein, and the interference which this causes in the lignin determination, that there is up to six weeks an apparent increase in lignin by all methods. Over a longer period the apparent lignin content falls by all methods to a somewhat similar extent. Because of the disturbance introduced by protein which is at least as great in the long rotted samples as in the short rotted samples, the actual loss of lignin is no doubt greater than the figures recorded. There is no known method of ascertaining the magnitude of the error, and hence the actual degree of decomposition of the lignin. The losses recorded, however, are outside of the range of error, and must be taken as evidence of the slow aerobic fermentation of this substance.

TABLE VIII

Aerobic Decomposition of Lignin in Oat Straw

[*from Norman* (1935)]

Results expressed on basis of original straw.

Time.	Loss of Dry Matter. %	Method 1.* Appar. Lig. %	Method 1.* Lig. as % of Orig.	Method 2.* Appar. Lig. %	Method 2.* Lig. as % of Orig.	Method 3.* Appar. Lig. %	Method 3.* Lig. as % of Orig.	Method 4.* Appar. Lig. %	Method 4.* Lig. as % of Orig.
—	—	17·6	100	19·8	100	13·7	100	14·3	100
1 week . .	14·28	18·6	106	19·3	98	15·9	116	16·3	114
2 weeks . .	28·40	20·0	114	20·1	102	16·4	119	17·1	119
3 weeks . .	36·67	19·1	109	19·5	99	16·3	119	15·3	107
6 weeks . .	44·41	19·3	110	19·0	96	15·9	116	13·9	97
3 months .	51·79	17·4	99	16·8	85	14·5	106	12·1	85
6 months .	57·32	14·9	85	14·7	75	12·3	90	9·9	69
9 months .	59·26	13·5	77	12·8	65	11·5	84	9·8	69
12 months .	62·03	11·2	64	10·9	55	8·8	64	7·1	50

* Method 1—2 hr. with 72 per cent. H_2SO_4, $< 20°$ C., diluted to 3 per cent., boiled 2 hr.

Method 2—16 hr. with 72 per cent. H_2SO_4, $< 20°$ C., diluted to 3 per cent., boiled 2 hr.

Method 3—Prehydrolysis with 5 per cent. H_2SO_4, 1 hr., then as method 2 [Norman and Jenkins (1934)].

Method 4—Pre-extraction with hot water 3 hr., then as method 1, but boiled after dilution for 4 hr. [Ritter, Seborg and Mitchell (1932)].

Conclusion

From the many conflicting results presented above, it is not possible to present dogmatically any clear-cut conclusions. If the difficulties of estimation be taken into consideration and the methods employed are reviewed in the light of possible errors, then the following would seem to be a fair statement of the position.

Lignin under aerobic conditions and *in situ* in plant materials, is by far the most resistant major constituent, and over brief periods suffers little or no loss. If, however, fermentation is prolonged for months or even years, a slow decomposition does, in fact, occur. Nothing approaching complete removal has ever been obtained.

Lignin in woods, and in materials such as straw, may be attacked fairly readily though not very rapidly by fungi of the Basidiomycete type, but its removal is always accompanied by losses of cellulose and hemicelluloses.

Lignin under anaerobic conditions is slowly attacked by both mesophilic and thermophilic organisms, but is much less available than the other constituents. The only report of extensive fermentation has not been authenticated.

Normal isolated lignin seems to be unavailable both to aerobic and anaerobic microfloras, and indeed, evidence of a definite bacteriostatic action has been obtained.

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SYNTHETIC ANTI-MALARIALS

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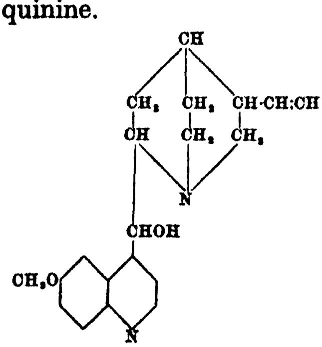
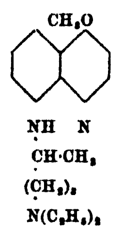
MALARIA is one of the major destructive diseases attacking the human race. In India alone, it is reckoned that 80–100 millions out of 370 millions contract this disease every year. Amongst the vast populations of south-eastern Asia as well as of the continents of Africa and America it exacts its heavy toll. The fight against it is carried out on many fronts. Control of the disease is attempted by measures directed against the mosquito, which, as everybody now knows, is the carrier conveying the disease from man to man. The drainage of land in order to remove suitable breeding places and the oiling of ponds and lakes so as to destroy the mosquito larvæ are amongst the measures taken by public authorities in many countries, whilst personal precautions, designed to minimise the number of mosquito bites, are taken for granted as but prudent common sense in malarious regions. With all this work the name of Sir Ronald Ross will always be associated.

But in recent years a vigorous attempt has been made to attack the malaria problem from a new angle. The efficiency of quinine as a specific drug against malaria has been known to Western Europe since the seventeenth century. It is in the large majority of cases really efficacious and its use is widespread, but even so it has certain important drawbacks. Large doses are required over considerable periods and this not infrequently gives rise to disturbing and even alarming toxic symptoms. Occasionally, where an extreme degree of sensitiveness to the drug seems to exist, these untoward effects may be so severe as to exclude the use of quinine absolutely. Though relatively cheap, the quinine alkaloids are still too dear to allow of their free use by the poverty-stricken multitudes amongst whom the disease is so rampant. The cure of the symptoms, which may be effected within a few days, is often followed by a relapse when the treatment is stopped, and this is particularly unfortunate where economic reasons prevent the patient being kept for more than a few days away from work or even under medical observation. It is not surprising, therefore,

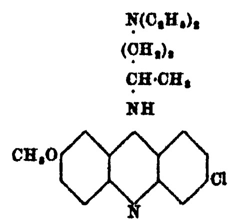
that in this age of synthetic Organic Chemistry, attempts should have been made to manufacture compounds more or less closely related to quinine, in the hope of obtaining a drug with all the efficiency and none of the disadvantages of the natural alkaloid. The search has been a long and difficult one. The problem is complicated because the malarial parasite has curious and complex ways. There are three distinct types of parasite, sub-tertian, benign tertian and quartan, and each type goes through an elaborate life cycle assuming various forms in the mosquito and in the human host. What is really wanted is a drug which will attack each of the forms of each of the parasites with complete and certain effect and at the same time be so specific as to leave the tissues of the host quite unharmed. Four lectures were delivered by Dr. Green in May 1934 at the course on Malariology at Singapore under the auspices of the League of Nations and give a very interesting and valuable account of the progress so far made.[1] Naturally they are concerned in the most part with the two synthetic anti-malarials which have become best known during the last ten years, namely plasmoquin and atebrin. Both of these are the result of prolonged research carried out by the Bayer Co. at Elberfeld. The former is a quinoline and the latter is an acridine derivative and for purposes of comparison their structural formulæ are given below beside that of quinine.



QUININE.



PLASMOQUIN.



ATEBRIN.

The first lecture gives a general survey of the history and chemical properties of the new anti-malarials. In the second lecture the practical advantages and disadvantages of plasmoquin are discussed, special attention being directed to the results obtained with it in the Malay States. The third lecture deals similarly with atebrin, whilst in the fourth and last lecture the present position

[1] *Lectures on the Development and Use of Synthetic Anti-malarial Drugs.* By Richard Green. [Pp. 50.] [Bulletin from the Institute of Medical Research. Federated Malay States.]

is surveyed and the rôle of the synthetic compounds in the treatment of malaria is summed up.

Plasmoquin, it would seem, has not fulfilled its early promise. In the case of quartan and benign tertian malaria it is doubtful whether it has any advantage over quinine. In the case of the important sub-tertian type of malaria it would appear to be without action on the asexual form—unlike quinine by which this is destroyed—but on the other hand it does seem to attack the gametocytes, the crescent forms which frequently persist in the blood and which, by infecting biting mosquitos, make possible the spread of the disease. Atebrin, on the other hand, in its chemotherapeutic action, would seem to be very similar to quinine, only the dose required is smaller, and the toxicity would seem to be usually quite low. Like quinine, it does not seem to possess the power of sterilising the sub-tertian crescent carrier. Incidentally this result illustrates the very complex nature of the relationship between chemotherapeutic action and chemical constitution, for a glance at the formulæ will show that, chemically speaking, atebrin is far removed from quinine and yet, chemotherapeutically, these two seem more closely related to each other than either is to plasmoquin. The fourth lecture deals with the present position with regard to the use of anti-malarial drugs. Plasmoquin is definitely not suitable for the general treatment of malaria. It is rather treacherously toxic, and, moreover, fails to deal with the asexual form of the sub-tertian parasite. Atebrin, on the other hand, would seem to be in almost every way as effective as quinine and to be definitely superior in reducing the number of relapses. Being a relatively new medicinal it is doubtful whether the optimum conditions for its use have yet been completely ascertained. Even as it is, serious toxic effects would seem to be quite rare.

For the treatment of the great malarial stricken masses of the tropical and sub-tropical regions of the world a drug is required which is very cheap, very efficient and very safe. Nothing so far known quite meets all requirements but the development of active synthetic compounds clearly points to the possibility of still better ones being obtained if only sufficient energy and money is given to the problem. It is stated that 12,000 different compounds were prepared and tested by the I. G. German chemical combine in the course of the development of plasmoquin and atebrin. This indicates the magnitude of the effort which is required. But the objective is one of world-wide importance and it is to be hoped that the resources of Medicine on the one hand, and of Chemistry on the other, will be combined in ever-increasing efforts to solve successfully this grave and important problem.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., F.R.S.E., Royal Observatory, Edinburgh.

Stellar Parallaxes.—A second edition of the *General Catalogue of Stellar Parallaxes* has now been issued by the Yale University Observatory. It has been compiled by Frank Schlesinger with the collaboration of Louise F. Jenkins, and obviously could not have been entrusted to better hands. It contains all the trigonometric, spectroscopic and dynamic determinations which were available through publication or correspondence in January, 1935. The parallaxes given have all been reduced to a homogeneous system, and special care has been devoted to making the probable errors correspond to the reliability of the results.

Part I, containing the trigonometric and spectroscopic parallaxes, comprises 7534 stars, many of which have been observed by both methods. The trigonometric determinations are restricted to those with a probable error not exceeding 0″·015. The series from ten different observatories have been carefully compared, both with each other and with the Mount Wilson spectroscopic values, and in only one case does the correction necessary to reduce them to a uniform system exceed 0″·003, this exception being the Dearborn series in which no rotating sector was employed in securing the photographs, and which show a marked correlation with the brightness of the stars. The spectroscopic parallaxes have all been adjusted to the Mount Wilson system, which, in its turn, was calibrated with trigonometric determinations, so the results obtained by both methods are on a nearly uniform basis. The average probable error of the absolute magnitudes obtained at Mount Wilson is only 0·4 magnitude, and determinations at other observatories have nearly the same accuracy, though needing corrections, in some cases as great as 1·2 magnitude, for reduction to the basic system.

The dynamic parallaxes, 2444 in number, have been computed from the orbital motions of double stars on the assumptions (1) that the masses in any specified system are in accordance with the mass-luminosity law; and (2) that the perspective foreshortening

of the orbital motion is equal to the mean foreshortening. The list includes computations by Russell and Moore of all the objects in Aitken's and Burnham's catalogues for which sufficient observational material was available, supplemented by a number of new discoveries by Aitken computed by Miss Moore at Princeton, while all the stars south of declination $-19°$ have been computed by W. S. Finsen at the Union Observatory, Johannesburg. Uniformity has been secured by the use of the same constants and processes throughout. The resulting dynamic parallaxes are divided as to quality into three classes, good, fair and poor. According to Russell and Moore the probable error in terms of the true parallax after allowing for both observational and statistical errors is for the "good" $\pm$ 28 per cent., for the "fair" $\pm$ 35 per cent., and for the "poor" $\pm$ 50 per cent., giving their weights as approximately 3 : 2 : 1. In the case of the Johannesburg parallaxes, these southern stars have been under accurate observation for a much shorter time, and Finsen has divided them into two groups. For the former Schlesinger estimates the probable error as $\pm$ 35 per cent. and for the latter $\pm$ 50 per cent. of the true parallax of the system.

Dr. Schlesinger was able to include in his catalogue a considerable number of recently determined parallaxes which were supplied to him prior to publication. One important list thus included has now appeared as Vol. II of *Observations of Stellar Parallax from photographs taken and measured at the Royal Observatory, Greenwich* (London, H.M. Stationery Office). This completes determinations for 516 stars, of which 488 are B.D. stars and the remainder are fainter objects with annual proper motion exceeding $0''\cdot20$, the programme being confined almost entirely to the Greenwich Astrographic Zone, $+64°$ to the Pole. The photographs were all taken with the Thompson refractor of 22 feet 6 inches focal length, giving a scale of 1 mm. to $30''$. The programme included observation of each star at not less than 5 epochs, with 4 plates at each epoch, the length of which was approximately a month. The magnitude of the parallax star was controlled by a neutral coloured filter mounted 7 mm. in front of the plate, and, by making a second exposure with the filter and plate turned through 180°, any displacement by the filter was eliminated in the mean. Generally six comparison stars distributed as symmetrically as possible and of average magnitude 11·5 were used in each determination.

The greatest parallax in this catalogue is $0''\cdot247$, eight reach or exceed $0''\cdot10$, and 48 lie between $0''\cdot05$ and $0''\cdot10$. The probable error of an average parallax is given as $\pm 0''\cdot0085$. This varies slightly with Right Ascension owing to the high latitude of Green-

wich and the consequent long duration of twilight in summer, but even at R.A. 18 hours the probable error is only $\pm 0''{\cdot}0105$. From the stellar parallaxes and apparent magnitudes absolute magnitudes have been calculated, and a table is given showing the stars grouped according to their spectral types, with their mean proper motions, visual and absolute magnitudes, parallaxes and cross velocities all exhibited. For the Ko stars there is a marked run of absolute magnitude with proper motion, the stars of larger proper motion being intrinsically fainter. The same run is apparently present, though less markedly, in other spectral types. It is concluded that the observation of the B.D. stars in the Greenwich Astrographic Zone is now complete down to parallax $0''{\cdot}080$, and probably nearly so down to $0''{\cdot}050$.

The second important list only just published is that of the spectroscopic absolute magnitudes and parallaxes of 4179 stars, issued over the names of Adams, Joy, Humason, and Miss Brayton (*Contrib. from the Mount Wilson Observatory*, No. 511). Although it can claim a definite physical basis, the spectroscopic method of determining parallax is essentially empirical. The absolute magnitudes of a group of stars of the same spectral type are first obtained trigonometrically, and their spectra are then examined for lines which change in intensity with the star's absolute magnitude. The intensity of each variable line is estimated with reference to a neighbouring line of constant density, and, finally, empirical calibrating curves are drawn connecting the intensity differences with the absolute magnitudes. The total intensity of each line is the quantity measured, and not merely its depth or width. The process is repeated in turn for stars of each spectral type, and has proved very successful for classes later than A5, a satisfactory agreement being secured with the trigonometric system. The present catalogue is composed mainly of stars of the spectral classes F, G, K and M, and is practically complete as regards stars contained in Boss's Preliminary General Catalogue north of declination -26° and later than type A. The distribution of absolute magnitudes with respect to spectral class has been determined and is shown in a diagram. The main sequence and the giant series of stars are clearly defined, as is also the gap between giants and dwarfs of types K and M. The lack of F type stars in the giant series is a prominent feature.

A third list, 217 in number, is that just issued by the Royal Observatory, Cape of Good Hope (H.M. Stationery Office, 1935). It represents work done during the years 1926–1930 with the 24-inch Victoria refractor, focal length 22 feet 6 inches. The resulting scale of the photographs, 1 mm. = 30″, is the same as was used at Green-

wich, and, although on the small side for parallax work, is not unsuitable for the poor definition frequently experienced at the Cape. The stars were all selected from Schorr's Eigenbewegungs-Lexikon south of dec. $-10°$ and have proper motions exceeding 40″ per century. The plan of observation, measurement and reduction closely resembled that adopted at Greenwich, but the brightness of the parallax star was cut down by a rotating sector driven by a jet of compressed air directed on to its rim. The catalogue shows a large proportion of significant parallaxes, due to the selection of stars with considerable proper motions. One hundred and forty-nine have parallaxes of 0″·020 or over, fifty lie between 0″·05 and 0″·10, while thirteen exceed the latter figure. A table exhibits the absolute magnitudes of the stars arranged according to their spectral class. For stars of parallax exceeding 0″·02, the mean values give magnitude 4·6 for class F5, 5·1 for Go, decreasing to 7·5 for K5 and 8·8 for Ma. These stars evidently belong to the main sequence. The stars of type Ma have the large average parallax 0″·177.

Parallaxes of Novæ.—The distances of novæ are generally too large to be determined trigonometrically with any accuracy, and other methods have perforce been utilised. In the case of Nova Persei, 1901, several months after its maximum a faint nebulous ring was discovered around it. This was observed to expand at the rate of a few seconds of arc a week, and was interpreted as a wave of light gradually spreading and illuminating in turn more distant parts of a surrounding nebula. On this assumption it was possible, by measuring the angular velocity of the advancing front of the wave, to estimate the distance of the nova as 350 light years. Nova Aquilæ, 1918, was also found to be surrounded by an expanding luminous shell, but in this case the motion was much slower, being only a few seconds of arc a year. It was assumed that here was an actual expanding shell of gas instead of a shell of light, and by comparing its measured angular rate with spectroscopic observations of the displacements of its absorption lines the distance of the nova could be estimated at 1200 light years. But the method has the uncertainty that different elements may give different radial velocities, while in addition all novæ do not develop in the same way.

For Nova Herculis, 1934, a new method has been utilised by E. G. Williams at Cambridge (*Monthly Notices*, R.A.S., 1935, May). It is based on the intensity of the interstellar lines H and K, which are generally easily recognisable in the spectra owing to the large Doppler displacements of the absorption lines of the nova itself.

The measures were made with a microphotometer on 24 plates taken with a 2 prism spectrograph on the 25-inch refractor. The result was finally based on the intensities of the K line, which were compared with the intensity of the same interstellar line in a number of early type stars whose distances had been obtained by the spectroscopic parallax method. The resulting distance for the nova is 1200 light years, giving it an absolute magnitude of − 6·5 at maximum compared with + 6·7 before its outburst, its light having increased some 200,000 times. The distance of the nova from the galactic plane appears to be of the unusually high order of 500 light years. In view of a possible lack of uniformity in the calcium cloud with increasing distance from the galactic plane, some uncertainty is attached to these estimates.

Internal Motions in Spiral Nebulæ.—About twelve years ago (*Contributions from Mount Wilson Observatory*, No. 260), Dr. van Maanen announced his discovery of large internal motions in certain spiral nebulæ, based on measures of long exposure photographs taken at the 25-foot focus of the 60-inch reflector. Special attention was paid to the nebula Messier 33, for which two photographs taken in 1910 and 1922 respectively were compared. The results, based on measures of 24 comparison stars and 399 nebular points, indicated rotatory motions with periods, varying with distance from the centre, of from 60,000 to 240,000 years. Six additional spirals were treated in a similar way, and in each case an internal motion, readily interpreted as a motion outwards along the arms, was detected. Such a motion is in accordance with the theoretical work of Jeans, but the velocities implied by the displacements measured by van Maanen were surprisingly large, being of the order of thousands of miles a second. The extraordinary significance of such large angular rotations has led to the remeasurement of four of the nebulæ by Edwin Hubble (*Mount Wilson Contribution*, No. 514). In this case the photographs were all made at the Newtonian focus of the 60-inch reflector, and the measures comprised two early and two later plates for each nebula, the average interval between the exposures being 19 years. They were measured independently on a double slide machine, using about 20 reference stars and 50 nebular points, while the effect of personality was investigated by check measures being made by different members of the staff. Rather unexpectedly, the results showed no displacements which were not comparable in size with the errors of measurement, and certainly no rotations of the order found by van Maanen were present. Hubble considers that the differences interpreted in 1923 as rotations were really due to

systematic errors of measurement in one of the sets, each of which appeared internally consistent.

In No. 515 of the *Mount Wilson Contributions*, van Maanen mentions several possible sources of systematic error in these measures. They include the length of the exposures which in some cases extended over several nights; the large hour angles involved; the choice of comparison stars which were generally brighter than the nebular points and by no means centrally or symmetrically placed on the plates; and possible effects of differential coma. Owing to his recognition of these difficulties in 1923, he then decided to postpone further investigations until photographs taken at sufficient intervals with the 100-inch telescope became available for measurement. Suitable pairs of plates of two nebulæ, M 33 and M 74 taken with the larger instrument at an interval of nine years have just been measured. Care was taken to make the hour angles equal, and the exposures needed were only about one hour. The measures show angular velocities for both nebulæ in the same direction as before, but the values are small. For M 33 the 1923 rotational component $+ 0''{\cdot}020$ is reduced to $+ 0''{\cdot}013$, while the value for M 74 which was not previously measured is only $+ 0''{\cdot}008$. In view of these differences van Maanen agrees that the difficulty of avoiding systematic errors makes one view the motions with reserve. Further investigation with the 100-inch telescope is taking place under conditions chosen to avoid such disturbing influences as far as possible.

PHYSICS. By L. F. Bates, Ph.D., D.Sc., F.Inst.P., University College, London.

The Properties of Rochelle Salt Crystals.—In recent years the pronounced electrical properties of Rochelle salt crystals have been the subject of a large number of researches, for the dielectric properties of this material appear to be at least as unique as the magnetic properties of iron. Chemical analysis shows the salt to consist of potassium-sodium tartrate with 4 molecules of water of crystallisation. The latter are not very tightly bound for the crystals dehydrate in air, and, according to Valasek (*Phys. Rev.*, **19**, 478, 1922), a crystal may easily be dried to such an extent that it loses 5 per cent. of its weight without suffering change in its appearance. Unfortunately, this means that the crystals are not easy to handle, and, in addition, they are extraordinarily sensitive to thermal and mechanical disturbances; the heat which enters a crystal in contact with one's hand is often sufficient to cause it

to break up, and rapid changes of temperature produce corresponding marked disintegrations.

Such crystals cannot be split in the usual manner, but use can be made of their great solubility in water, and Busch (*Helv. Phys. Acta*, VI, 315, 1933), who describes a convenient method of growing them, has shown that quite small sections may readily be cut from large crystals without damage. This is done by the usual method of causing an endless silk thread, moistened with water, to run over two brass pulleys so that in its passage the thread presses very gently upon the crystal to be cut, thus making a clean, narrow division. The crystals are stable up to 54° C., above which temperature they melt and form two separate tartrates. Well formed prismatic specimens were considered by Müttrich (1864) to belong to the orthorhombic hemihedral crystal class V. It was therefore deduced that the crystal had no direction of polarity and no centre of symmetry, and could thus be piezoelectric but not pyroelectric; the latter deduction is of course very hard to verify by experimental test in view of the above remarks. As far back as 1893, however, Pockels showed that the piezoelectric moduli were extremely high, and Cady later showed that they varied considerably with the magnitude of the applied load.

Important work on the dielectric and piezoelectric properties of the crystals was carried out by Valasek (*Phys. Rev.*, **19**, 478, 1922; **23**, 114, 1924; **46**, 450, 1934). A serious difficulty encountered in these researches was the provision of suitable electrodes. It is always difficult satisfactorily to attach electrodes to a solid dielectric, and when the latter's constant is very high the chances of introducing errors are great. Some workers have used electrodes of saturated Rochelle salt solution. For example, Kobeko and Kurtschatow (*Zeit. für Phys.*, **66**, 192, 1930) cemented glass tubes to the opposite sides of a crystal and filled them with the solution. Busch found, however, that with these electrodes it was impossible entirely to avoid either solution of the crystal surface or deposition from the electrode solution, owing to the great solubility of the salt and its marked dependence on temperature. Other workers have cemented metal foils to the crystal surfaces with solutions of canada balsam in xylol, etc., but in such cases the presence of a film of adhesive material between the foil and crystal was likely to lead to incorrect results. Valasek in his most recent work used silver-plated electrodes formed by evaporating silver and allowing it to condense on the crystal surface in a vacuum. Busch considers that the best electrodes are made in the following manner. A piece of thin paper with a central hole 6 mm. in diameter is pasted on

the crystal surface. The uncovered portion of the surface is then coated with graphite by rubbing it with a 6 B Kohinoor lead pencil. Glass tubes, one being straight and the other U-shaped, are attached by shellac varnish to opposite faces of the crystal concentric with the pencilled portions. Finally, when dry the tubes are filled with mercury and provided with platinum leads.

Valasek found that the dielectric constant, ε_{11}, and the piezoelectric modulus along the a-axis in the crystal were unusually high between $-30°$ and $+30°$ C. Thus, ε_{11} rose from 140 to 1380 and then fell to 423 as the temperature was raised. Sawyer and Tower (*Phys. Rev.*, **35**, 269, 1930), who were interested in the possibility of commercial applications of the crystals, found that clear, flawless half-crystals 45 cm. long and weighing 2 Kgm. could be obtained. They found that a section 4·75 mm. thick, cut perpendicular to the a-axis, gave a dielectric constant, ε_{11}, of 18,000 when tested at 15° C. with alternating potentials of 60 volts at 60 cycles per second. Kobeko and Kurtschatow (*Zeit. für Phys.*, **66**, 192, 1930) emphasised the importance of good electrode contact and showed that the higher value 20,000 was obtained with good contacts.

The electric and piezoelectric polarisations provided hysteresis curves similar to the magnetisation curves of ferromagnetic materials, and the saturation values of the polarisation for fields in opposite directions parallel to the a-axis were different. When ε_{11} was plotted as a function of the intensity of the applied electric field the curve showed a maximum much the same as that of a μ, H curve for iron, and this maximum occurred at a definite value of the field. Sawyer and Tower were able to obtain very clear hysteresis cycles using a cathode ray oscillograph. The electric polarisation also exhibited fatigue phenomena, so that the longer the period for which a condenser with a Rochelle salt dielectric was left connected to a charging battery, the smaller might be the quantity of electricity obtainable on first discharge. For example, Valasek found that with a crystal section 0·15 cm. thick the charge measured in the usual way by a ballistic galvanometer was a maximum when the time of charging was 2 seconds, after which it slowly decreased to about one-half the maximum value for a charging time of 24 hours. The fatigue phenomena were confined to the temperature range $-30°$ to $+30°$ C., and further reference will later be made to them. Yet, Valasek found that the remaining physical properties behaved in a more or less normal manner, with the exception of electrical conductivity and electric double refraction. The former showed differences in magnitude in the two directions parallel to the a-axis,

and the latter decreased rapidly above 20° C. The dielectric constants ε_{22} and ε_{33} and the corresponding piezoelectric moduli were relatively small and independent of the temperature in the critical range mentioned above; in other words, only the properties parallel to the a-axis are of major interest.

Now, Valasek concluded because the electric polarisation and the electric conductivity had different values in the two directions parallel to the a-axis, that the latter was a polar axis and that the crystal possessed a natural electric moment, parallel thereto. This would mean a breakdown of Neumann's principle, which states that the physical properties of the crystal have always at least the symmetry of the crystal form. Experiments were therefore made to find whether the crystals exhibited pyroelectricity, but their interpretation is most difficult for the high value of the dielectric constant makes it possible for strains produced on heating to give rise to an electric moment, although they would still be insufficient to produce visible departure from the symmetry of crystal class V.

The striking behaviour of the dielectric constant immediately caused it to be compared with the permeability of ferromagnetic materials, and the idea that an internal molecular field was responsible for the high values was early postulated by Valasek, who looked upon the + 30° point, at which the dielectric constant changed so rapidly, as a kind of electrical Curie point. His view was supported by Kobeko and Kurtschatow who measured the electrocaloric effect in Rochelle salt. One set of a sensitive system of thermojunctions was suitably attached to a crystal while the other was close to it but not in thermal contact. Then, when an electric field was suddenly established in the crystal there resulted a change in internal potential energy, because of the alignment of the elementary electric particles, which was manifested by the liberation or absorption of heat, duly recorded by the thermojunctions. By analogy with ferromagnetism a liberation of heat would be expected. Such an effect is not due to hysteresis, it should be strictly reversible, and any ordinary hysteresis effects ought to be eliminated by taking the mean of the temperature changes observed when the field is switched on and switched off. The electrocaloric temperature change was plotted as a function of the mean temperature of the crystal for chosen values of the applied field, and the curves thus obtained were in many respects similar to those observed by Weiss and by Potter in analogous experiments with iron, a very pronounced peak occurring at + 25° C. Now, when this temperature change was plotted as a function of the square of the electric polarisation, P, the mean temperatures of the crystal being kept constant at chosen

values, there was obtained a series of curves like the ΔT, σ^2 curves found by Weiss and by Potter for iron. These curves gave intercepts on the P^2 axis from which the mean value of the electric moment per molecule and the constant of the internal field could be found; the former was 10^{-18} e.s.u. and the latter 10^7 volt per cm. per unit polarisation.

There remain other features which call for comment. Thus, below 0° C., instead of a rise in temperature on switching on the field, a fall was observed, and there was a well pronounced maximum fall at about − 20° C., the magnitude of the effects being independent of the direction of the field applied parallel to the *a*-axis. As in the case of ferromagnetic materials, the specific heat of the salt was found by Müller (*Phys. Rev.*, **43**, 500, 1933) to rise to a sharp maximum and then to fall rapidly in the immediate neighbourhood of the so-called Curie point at + 25° C. Moreover, the electric polarisation in very weak fields rose to a sharp maximum just before this point, in a manner exactly similar to the variation of the magnetic permeability of iron in weak fields.

The term Curie point is a loose one, for with iron we distinguish between the ferromagnetic Curie point and the paramagnetic Curie point, the former being the temperature at which spontaneous ferromagnetism disappears and the latter the temperature constant or parameter which occurs in the Curie-Weiss expression for the magnetic susceptibility in the paramagnetic state. Now, Müller measured the dielectric constant of Rochelle salt between 23° and 50° C. with the aid of a capacity bridge, supplied with alternating current at 1000 cycles per second. He found that a Curie-Weiss law was followed between 34° and 50° C. with a Curie constant of 128·5 and a Curie point, analogous to a paramagnetic Curie point, at 24·9° C. From these values the Lorentz-Lorenz factor was calculated to be 2·315, while that calculated on the assumption that the dielectric phenomena were due to the free rotation of the molecules of water of crystallisation was 2·314; these calculations do not appear to have been published in extenso.

The analogy between ferromagnetism and the behaviour of Rochelle salt formed the basis of a set of experiments by Staub (*Helv. Phys. Acta*, VII, 2, 1934) who has investigated its properties by means of X-rays. Staub considered that the electric polarisation might arise from displacement of groups of ions in the crystal lattice possessing permanent electric moments, and that, consequently, there should be a change in the intensity of X-radiation reflected from a crystal, on the application of an electric field. He considered, too, that the intensity should exhibit marked changes as the temper-

ature of the crystal was raised, even in the absence of an applied field. He investigated these points by a neat ionisation method in which the ionisation current in a chamber traversed by the incident beam of X-rays was balanced against that in a chamber through which passed the rays reflected from a Rochelle salt crystal. A resistance R_1 in the first circuit was connected in series with a resistance R_2 in the second circuit and the resulting potential difference across $R_1 + R_2$ was supplied to the grid of a valve amplifier. When the potential differences between the ends of the several resistances R_1 and R_2 were equal, the resultant was zero. The amplifier was used in conjunction with a sensitive galvanometer arranged as a null instrument by suitable connections to neutralise the anode current through the valve. The deflections of the galvanometer produced by intensity changes were recorded photographically. In order to carry out experiments on X-ray reflection at different temperatures the whole X-ray spectrograph was enclosed in a box covered with felt, containing a cooling coil and a small electric heater.

The ratio $\Delta I/I$ was measured, where ΔI was the change in the intensity I observed when an electric field was applied. This ratio was found to be different when the field was applied in opposite directions parallel to the a-axis. When the ratio was plotted as a function of temperature for an applied field of 666 volts per cm. it was found that for the field in one direction the ratio rose to a maximum between 21·2 and 21·6° C. and then fell to zero at 24·5°, while for the field in the other direction it fell gradually to zero at the latter temperature. The ratio for a fixed direction of the field was the same whether reflection took place from the front or rear face of the crystal, and this was taken to mean that surface and space charge phenomena were not responsible for the high dielectric constant. The hysteresis effects observed by Valasek did not appear in quite the same way in Staub's measurements, for the X-ray intensity changes were recorded immediately upon the application of the field, but the time of relaxation as they disappeared on removal of the field was very large, particularly when the field had been applied for a long time. This would account for Valasek's observations described above. The time of relaxation decreased with rise in temperature and fell rapidly in the neighbourhood of the so-called Curie point; there were marked differences between dry and wet crystals, the latter being crystals which had been newly polished by rubbing on a ground glass plate moistened with water.

We may now turn once more to the results of measurements made with alternating potentials. For instance, Valasek (*Phys.*

Rev., **46**, 450, 1934) measured the dielectric constant of crystals over a frequency range of 30 to 30 × 10^6 cycles per second while the temperature was maintained constant at 0° C. It fell steadily from 62,000 at 30 cycles to 220 at 10^7 cycles when there followed a sudden drop to negative values, *i.e.* the reactance of the crystal became inductive, at a critical frequency of about 14 × 10^6 cycles which so persisted up to the limit of the applied frequencies. This critical frequency appeared independent of the dimensions of the crystal and was considered to be due to a different cause than that considered responsible for the critical frequencies observed by Frayne (*Phys. Rev.*, **21**, 348, 1923) and by Errera (*Phys. Zeit.*, **32**, 368, 1931). The latter worker found that the dielectric constant fell more or less regularly from about 80 at 1·5 × 10^5 cycles to a value of − 400, then rose to a maximum of + 700 at 2 × 10^5 cycles and finally fell slowly to values which, while depending considerably upon the intensity of the applied field, still remained much higher than those obtained at frequencies further removed from the above range. Clearly, some kind of resonance was involved, and Busch (*Helv. Phys. Acta*, VI, 315, 1933) showed that this so-called anomalous electrical dispersion was caused by piezoelectric resonance vibrations of the crystal as a whole, and was not due to any property of the individual particles in the crystal, since he found that the frequency at which the maximum dielectric constant was observed depended upon the dimensions of the crystal. Both the value of this frequency and that of the corresponding dielectric constant depended greatly upon the temperature. Unfortunately all the above experiments still leave open the question of the nature of the dipoles responsible for the remarkable properties described in this article, for Müller's suggestion that rotation of the water molecules is responsible would not account for the results of the X-ray intensity measurements.

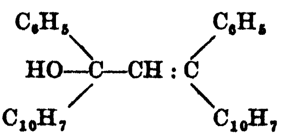
ADDENDUM. Hablützel (*Helv. Phys. Acta*, VIII, 499, 1935) has recently shown that Rochelle salt crystals possess anomalous coefficients of thermal expansion parallel to two axes, provided that the crystal is allowed adequate time in which to make the expansion at any temperature.

GENERAL AND ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

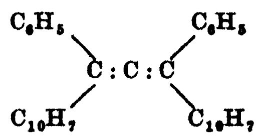
RESOLUTION OF ALLENE COMPOUNDS INTO OPTICAL ISOMERIDES.—As long ago as 1875 van't Hoff predicted that allene compounds of the type *ab* C : C : C *cd* should exist as optically active enantio-

morphs, but experimental realisation has been long in coming. Many attempts have been made to synthesise and resolve suitable allene derivatives (for references, see Freudenberg, *Stereochemie*, p. 804), but experimental difficulties have, hitherto, proved insurmountable. Since the pairs of valency directions of the central carbon atom lie in planes which are at right-angles, the groups *a* and *b* and the groups *c* and *d* lie in planes which are also at right-angles; the compound, therefore, has no plane nor centre of symmetry and it follows that its mirror image is not superposable upon the original and optical isomerism is possible. The same is true when the group *a* is identical with *c* and *b* with *d*. As frequently happens the problem has been solved independently and almost simultaneously by two groups of workers.

Maitland and Mills (*Nature*, 1935, 994) by "asymmetrical catalysis" have obtained diphenyl-di-α-naphthyl allene (II) in optically active forms.



(I)



(II)

When the alcohol (I) is dehydrated under ordinary conditions an optically inactive allene is produced which melts at 242–4°, but if the dehydration is effected by heating with *d*-camphorsulphonic acid in benzene a mixture is obtained of active and inactive material of which the former is much more soluble and can be isolated in well-formed crystals melting at 158–9° with a rotation $[\alpha]_{5461} + 437°$. When *l*-camphorsulphonic acid was used as the dehydrating agent a similar mixture was produced from which the lævo-form $[\alpha]_{5461} - 438°$ was isolated. When saturated solutions of the *d*- and *l*-forms were mixed the less soluble racemic allene m.p. 242–4° crystallised out, thus establishing the enantiomorphism of the isomerides.

Kohler, Walker and Tishler (*J. Amer. Chem. Soc.*, 1935, **57**, 1743) have overcome the experimental difficulties in preparing an allene derivative with a salt-forming group present suitable for resolution by the classical method. They have re-investigated the compound αγ-diphenyl-γ-1-naphthyl-allene-α-carboxylic acid (V) whose resolution Lapworth and Wechsler (*Jour. Chem. Soc.*, 1910, 46) were unable to effect owing to inability to obtain crystalline salts with optically active bases. This compound was synthesised

by Lapworth and Wechsler from β-benzoyl-α-phenylpropionic ester (III) by a series of reactions which probably follow the course :

$$\underset{\text{(III)}}{C_6H_5CO.CH_2.CH(C_6H_5).CO_2Et} \xrightarrow{C_{10}H_7MgBr} \begin{array}{l} C_{10}H_7.\underset{|}{C}(C_6H_5).CH_2.\underset{|}{C}H.C_6H_5 \\ \quad O \text{---------------} CO \end{array}$$

$$\xrightarrow{PCl_5} C_{10}H_7.\underset{\displaystyle Cl}{C}(C_6H_5).CH_2.\underset{\displaystyle COCl}{\underset{|}{C}}Cl.C_6H_5 \xrightarrow{EtOH} C_{10}H_7.\underset{\displaystyle Cl}{C}(C_6H_5).CH_2.\underset{\displaystyle CO_2Et}{C}Cl.C_6H_5$$

$$\xrightarrow{\text{pyridine}} \underset{\text{(IV)}}{\begin{array}{c} C_{10}H_7 \diagdown \qquad \diagup C_6H_5 \\ C:C:C \\ C_6H_5 \diagup \qquad \diagdown CO_2Et \end{array}} \longrightarrow \underset{\text{(V)}}{\begin{array}{c} C_{10}H_7 \diagdown \qquad \diagup C_6H_5 \\ C:C:C \\ C_6H_5 \diagup \qquad \diagdown CO_2H \end{array}}$$

Owing to the failure to isolate all the intermediate products there was some doubts about the constitution of the final product, but Kohler, Walker and Tishler have removed these by oxidation of the ester (IV) with potassium permanganate to phenylnaphthyl ketone (VI) and an oil which hydrolysed to phenylglyoxylic acid (VII)

$$\begin{array}{c} C_{10}H_7 \diagdown \qquad \diagup C_6H_5 \\ C:C:C \\ C_6H_5 \diagup \qquad \diagdown CO_2Eb \end{array} \longrightarrow \underset{\text{(VI)}}{\begin{array}{c} C_{10}H_7 \diagdown \\ CO \\ C_6H_5 \diagup \end{array}} + \underset{\text{(VII)}}{\begin{array}{c} \diagup C_6H_5 \\ OC \\ \diagdown CO_2H \end{array}}$$

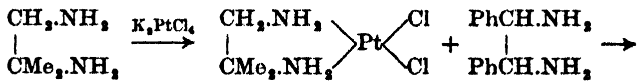
In addition they showed that the ester absorbed two molecular proportions of hydrogen to give a saturated ester. Further attempts to resolve the acid were unsuccessful, but when treated with phosphorus pentachloride it gave the acid chloride (VIII) which, in ether, with pyridine and glycollic acid gave the glycollic acid ester (IX). This formed crystallisable salts with brucine and was resolved by means of this agent into *d*- and *l*-forms with rotations $[\alpha]_D + 29{\cdot}5$ and $[\alpha]_D - 28{\cdot}4$ respectively.

$$\underset{\text{(VIII)}}{\begin{array}{c} C_{10}H_7 \diagdown \qquad \diagup C_6H_5 \\ C:C:C \\ C_6H_5 \diagup \qquad \diagdown COCl \end{array}} \xrightarrow{CH_2OH.CO_2H} \underset{\text{(IX)}}{\begin{array}{c} C_{10}H_7 \diagdown \qquad \diagup C_6H_5 \\ C:C:C \\ C_6H_5 \diagup \qquad \diagdown CO_2CH_2.CO_2H \end{array}}$$

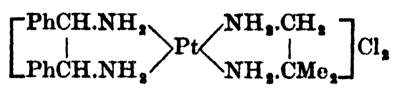
Configuration of the Valencies in Quadricovalent Platinum.—In the designing of experimental proof of the space

arrangement in chemical compounds one expects supreme artistry from Dr. Mills and one is not disappointed in the particularly neat solution of the problem of quadricovalent platinum (Mills and Quibell, *Jour. Chem. Soc.*, 1935, 839). The weight of evidence is that the valency directions of quadricovalent nickel, palladium and platinum are not tetrahedrally arranged as in the case of lighter elements but are uniplanar (Sugden, *Jour. Chem. Soc.*, 1932, 246; Cox, Saenger and Wardlaw, *ibid.*, 1934, 182; Drew and Head, *ibid.*, 221). If such is the case the usual co-ordination compounds would be non-resolvable into optical enantiomorphs, but failure to resolve a compound may be due to lack of a suitable method or to very rapid racemisation and cannot be adduced as complete proof of configuration.

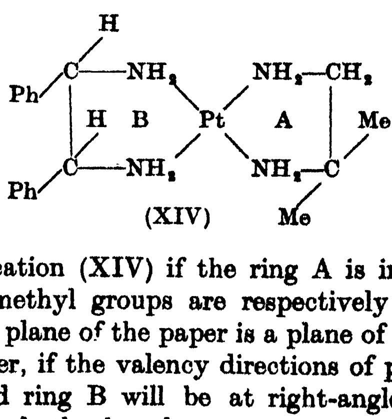
Mills and Quibell have treated *iso*butylene diamine (X) with potassium chloroplatinite and caused the resulting dimethylethylenediamino platinous chloride (XI) to interact with the *meso* form of diphenylethylenediamine (stilbenediamine) (XII). The resulting *meso*-stilbenediamino-*iso*butylenediaminoplatinous salt (XIII) has been resolved into optical isomerides through its *d*-diacetyltartrate.



(X) (XI) (XII)



(XIII)

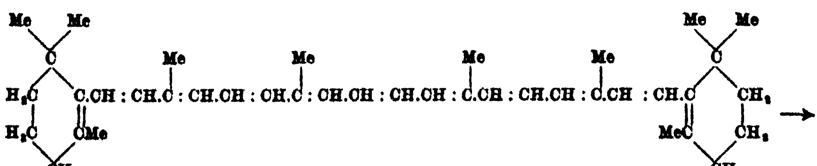


(XIV)

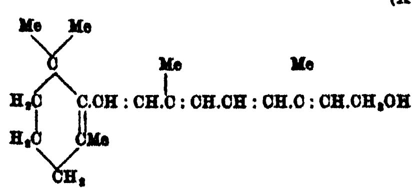
Now in the cation (XIV) if the ring A is in the plane of the paper the two methyl groups are respectively above and below that plane so the plane of the paper is a plane of symmetry through that ring; further, if the valency directions of platinum are tetrahedrally arranged ring B will be at right-angles to the plane of the paper, and as both phenyl groups are on the same side of the ring (since the meso compound was employed) the plane of the paper will be a plane of symmetry of ring B: (XIV) therefore,

has a plane of symmetry and would therefore be unresolvable. If the valency directions of platinum are uniplanar rings A and B both lie in the plane of the paper, but this plane, though one of symmetry for ring A, is not so for ring B, since both phenyl groups will lie either above or below that plane. A plane at right-angles to the plane of the paper will divide ring B symmetrically but will not do the same for ring A since CH_2 would be opposed to CMe_2. The uniplanar arrangement of the platinum valencies in (XIV) therefore gives an asymmetric molecule which should be resolvable.

THE VITAMINS.—There are few branches of synthetic organic chemistry in which more outstanding advances have been made than in that of the vitamins. These substances, of such remarkable physiological potency and present in such small amounts in natural products, but a few years ago seemed very elusive and there appeared little immediate prospect of their isolation in the pure state. The determination of their chemical constitution and their synthesis in bulk seemed unlikely for many years to come, yet, at the hands of a number of brilliant workers great success has been achieved. The fat soluble anti-keratinising vitamin A has been shown to be closely related to the yellow pigment, carotene, present in carrots and green vegetables, which is converted in the body into the vitamin. Carotene is a hydrocarbon of structure (XV) which is probably converted to vitamin A (XVI), which has an alcoholic group, by rupture at the central double bond.



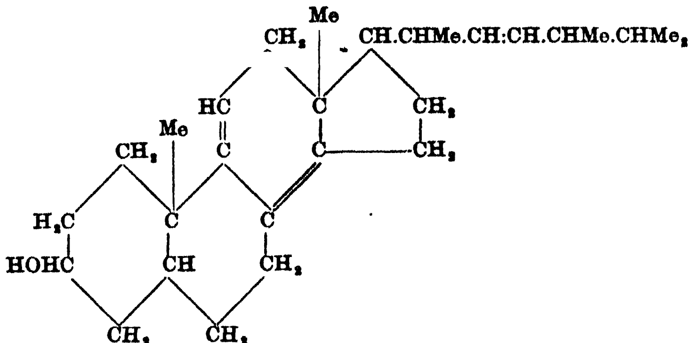
(XV)



(XVI)

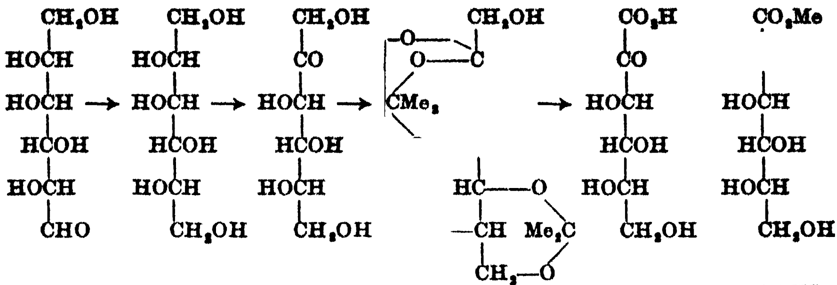
The fat soluble, anti-rachitic, vitamin D, sometimes known as calciferol and present in fish-liver oils, can be separated as a pure crystalline material from the product of the irradiation by ultraviolet light of ergosterol, isolated many years ago from ergot. This compound is present in the human body and the cure of rickets

by sunlight or ultra-violet light treatment and by cod-liver oil have thus been directly connected. Vitamin D has been assigned the structure (XVII).

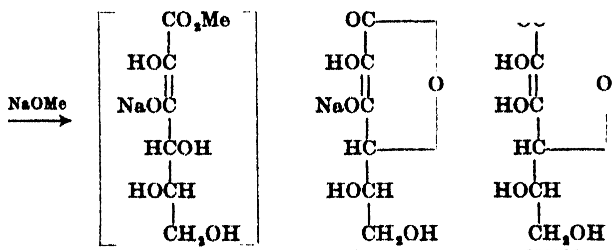


(XVII)

The water soluble, anti-scorbutic, vitamin C, present in oranges, lemons, etc., and now isolable in quantity from Hungarian pepper (Banga and Szent-Györgyi, *Biochem. J.*, 1934, **28**, 1625), has been identified by its chemical, physical and physiological properties with ascorbic acid (Haworth, Hirst and Zilva, *Jour. Chem. Soc.*, 1934, 1155). Its constitution was established and its synthesis effected by Haworth and his collaborators as described in a recent article (SCIENCE PROGRESS, 1934, **28**, 478). The original synthesis was troublesome owing to the difficulty of obtaining the starting material, *l*-xylosone, but in the last twelve months alternative methods of preparing ascorbic acid and its analogues have been devised which makes the preparation of these compounds in quantity a comparatively simple matter. *l*-Sorbose (XX) can now be obtained readily by the action of *B. xylinum* on *d*-sorbitol (XIX) which, in turn, is prepared by the catalytic reduction of *d*-glucose (XVIII) (Schlubach and Vorwerk, *Ber.*, 1933, **66**, B. 1251). In order to protect groups from oxidation *l*-sorbose was condensed with acetone to give the diacetone compound (XXI) which with alkaline potassium permanganate suffers oxidation of the primary alcoholic group, subsequent hydrolysis giving 2-keto-*l*-gulonic acid (XXII) which is converted to its methyl ester (XXIII) and this heated with sodium methoxide in methyl alcohol when the sodium salt of *l*-ascorbic acid (XXIV) is obtained from which the free acid (XXV) can be obtained. The acid (XXII) on heating with water in the absence of oxygen is itself partly converted to ascorbic acid (Reichstein and Grüssner, *Helv. Chim. Acta*, 1934, **17**, 311).



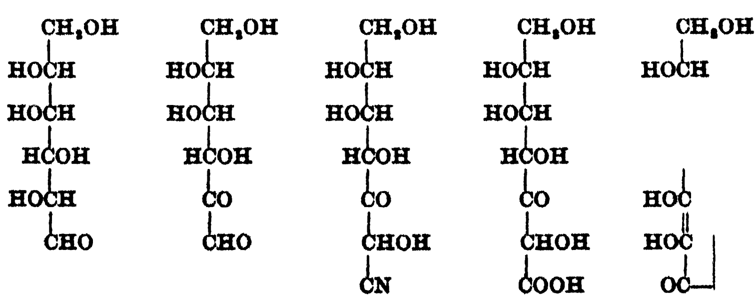
(XVIII) (XIX) (XX) (XXI) (XXII) (XXIII)



(XXIV) (XXV)

l-Sorbose (XX) can be oxidised directly to 2-keto-*l*-gulonic acid (XXII) by the regulated action of nitric acid, thus reducing the number of stages in the synthesis (Haworth, *Nature*, 1934, **134,** 724). Using similar methods Reichstein (*Helv. Chim. Acta*, 1934, **17,** 996, 1003) has synthesised *l*-erythroascorbic acid through adonitol and *l*-adonose.

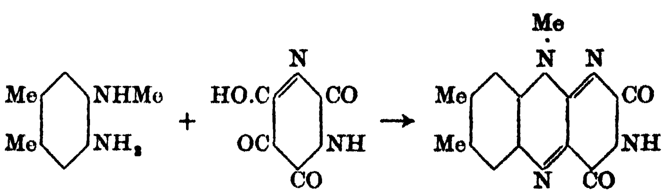
The original method of Haworth, that is, the addition of hydrogen cyanide to an osone followed by hydrolysis to the acid, has proved very fruitful in the synthesis of analogues of ascorbic acid when the osone is readily available, for example *d*-gluco-ascorbic acid (XXVIII) has been prepared from *d*-glucose (XXVI) through glucosone (XXVII) (Baird, Haworth, Herbert, Hirst, Smith and Stacey, *Jour. Chem. Soc.*, 1934, 62).



(XXVI) (XXVII) (XXVIII)

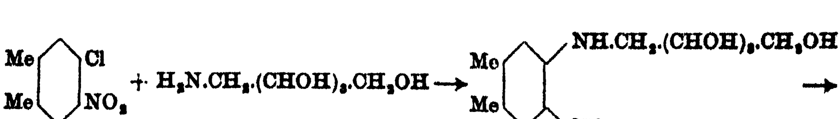
The above authors have also prepared by this method *d*-galacto-ascorbic acid from galactosone and *l*-araboascorbic acid from arabosone, whilst Reichstein, Grüssner and Oppenaur (*Helv. Chim. Acta*, 1934, **17**, 510) have prepared the above compounds and also *l*-guloascorbic acid and Reichstein, Schwarz and Grüssner (*Helv. Chim. Acta*, 1935, **18**, 353) have synthesised *l*-rhamnoascorbic acid.

The water soluble vitamin B_2 seems to consist of at least two factors, one an anti-pellagra factor and the other a growth promoting factor (György, *Biochem. J.*, 1935, **29**, 741, 760, 767 ; Harris, *ibid.*, 776). The latter has been identified with the crystalline orange-brown dye, lactoflavin, isolated originally from milk whey by Kuhn and his collaborators (Kuhn, Rudy and Wagner-Jauregg, *Ber.*, 1933, **66** [B], 1950) and also present in liver and in lucerne (Kuhn and Kaltschmitt, *Ber.*, 1935, **68** [B], 128). Lactoflavin has the formula $C_{17}H_{11}O_6N_4$ and on protracted irradiation in neutral aqueous solution gave a compound $C_{13}H_{12}O_2N_4$, lumilactoflavin (XXXI), which has been synthesised by Kuhn, Reinemund and Weygand (*Ber.*, 1934, **67** [B], 1460) by condensing 4-amino-5-methyl-amino-*o*-xylene (XXIX) with alloxan (XXX).

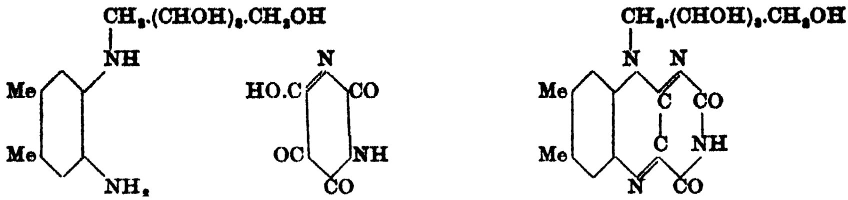


(XXIX) (XXX) (XXXI)

The presence of four hydroxyl-groups capable of acetylation in lactoflavin, not present in lumilactoflavin, suggested a pentose side chain in the former. Kuhn and Weygand (*Ber.*, 1934, **67** [B], 1939) by treating 4-nitro-5-chloro-*o*-xylene (XXXII) in the presence of a tertiary base with the amines (XXXIII) obtained by the reduction of the oximes of *l*-arabinose and *d*-xylose and reducing in the presence of a large excess of alloxan obtained leucoflavins (due to excess of the reducing agent) which were oxidised by air to flavins (XXXIV) which closely resembled lactoflavin in their chemical reactions.

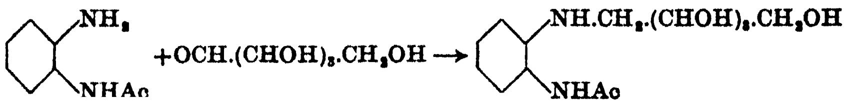


(XXXII) (XXXIII)



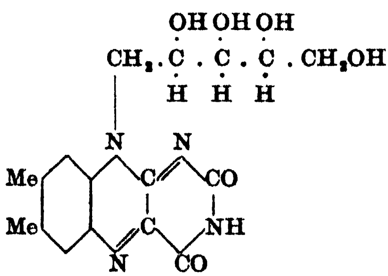
(XXXIV)

Karrer, Schöpp, Benz and Pfaehler (*Ber.*, 1935, **68** [B], 216) improved the yields by condensing a monoacylated *o*-diamine direct with the sugar and reducing thus :—



(XXXV)

They then condensed (XXXV) with alloxan as before. Finally Karrer and his co-workers (*Helv. Chim. Acta*, 1935, **18**, 426, 522) by using *d*-ribose as the sugar have synthesised 6 : 7-dimethyl-9-*d*-1′-ribityl*iso*alloxazine (XXXVI) and shown it to be identical with lactoflavin in chemical and biological behaviour.



(XXXVI)

The Origin of Petroleum.—An interesting series of papers has thrown new light on the vexed question of the origin of petroleum (A. Treibs, *Annalen*, 1934, **509**, 103 ; **510**, 42 ; 1935, **517**, 172). Treibs finds that porphyrins, deoxophylloerythroaetioporphyrin predominating, are widely distributed in coals, oil-shales and crude petroleums from many parts of the world. A Trinidad oil contained one-twentieth of the amount present in dried green leaves. Since the porphyrins are closely related to chlorophyll this implies that plants are far more important than, for example, marine creatures in petroleum genesis. Triebs considers that, for the porphyrins to have survived, coals and petroleums have not been exposed to a temperature higher than 200° C. which renders the distillation

theory of the origin of petroleum untenable but does not exclude the formation of the oil by a low temperature cracking of cellulose (compare Berl, *Annalen*, 1933, **504**, 38). The porphyrins were found to be present in the form of a vanadium and of a ferrous iron complex, but it is suggested that these are the result of a secondary change when less firmly bound elements are displaced.

PHYSICAL CHEMISTRY. By H. W. MELVILLE, D.Sc., Ph.D., Colloid Science Laboratory, Cambridge.

POLYMERS AND POLYMERISATION.—The word polymer has come to include an amazing variety of substances. Indeed, the term has become so general that it is almost impossible to frame a definition precisely. One definition may be quoted which at first sight seems satisfactory, but on closer examination is found to be inadequate. It is that a polymer is formed by the intermolecular combination of like (or in certain cases unlike) molecules to form molecules of infinite size. Polymer molecules are not however of infinite size, for many polymers such as polyoxymethylene (from formaldehyde), polystyrene, etc., have well defined molecular weights. Again it has been suggested that a polymer consists of groups of molecules joined to each other by primary valencies in one, two or three dimensions forming a large molecule whose molecular weight is determined by the number of elementary groups contained therein. Here difficulties are encountered until a primary valence and molecular weight are further defined. The definition is therefore a matter for further discussion. In this review the definition will be taken in its broadest sense and some account will be given of the constitution and mode of growth of some typical polymers. It is only within the last few years that it has become possible to apply physical conceptions in order to gain some insight into the mechanism of the growth of these complex molecules. For this purpose the kinetical method has been developed with some success, though it is to be emphasised that considerable caution must be observed in the interpretation of the results.

Of paramount importance in the consideration of the kinetics of polymerisation is what is known as a steric factor. In a simple bimolecular reaction the rate is expressible by the formula $Ze^{-E/RT}$ where Z is the collision number and E is the energy of activation of the reaction. In some dimerisations, for example that of ethylene, the rate of reaction is very much smaller than that calculated from the above simple formula. The simplest picture of the inefficiency of the collision is that the molecule must be orientated in a particularly favourable position before reaction will occur or

that only limited regions of a molecule are capable of reaction. Numerical calculation may be made, for the steric factor is the ratio of the observed rate to that calculated by means of the bimolecular formula from the observed energy of activation and the collision number computed by kinetic theory. In this way the following steric factors have been obtained: ethylene 4×10^{-3}, propylene 10^{-3}, 2 : 3-butylene 5×10^{-4}, *iso*butylene 5×10^{-1}, Δ 1 : 3-butadiene 10^{-4}, isoprene 2×10^{-3}. The general rule appears to be that the larger the molecule the smaller the steric factor (it would really be more consistent to term the reciprocals of the figures quoted as steric factors). It will be shown later what consequences may result if this rule applies in polymerisation reactions.

Recently (*cf.* Eyring, *J. Chem. Physics*, **3**, 107, 1935; Evans and Polanyi, *Trans. Faraday Soc.*, **31**, 875, 1935), a more abstract idea of steric factors has arisen. Attempts have been made by utilising the methods of statistical mechanics to calculate *à priori* the magnitude of velocity constants by evaluating the configuration and energy content of the collision complexes. For example in the simple reaction $A + B \longrightarrow AB' \longrightarrow AB$, AB' being the activated complex, the velocity constant k is given by

$$k = \frac{f_{AB'}}{f_A . f_B} . \frac{kT}{(2\pi mkT)^{1/2}} e^{-E/RT}$$

where $f_{AB'}$, f_A and f_B are the respective partition functions and E is the difference in energy between the activated and initial state, that is, the energy of activation. If such expressions be compared for a simple reaction between atoms on the one hand and complex molecules on the other it is found that the first factor in the above expression is much less for the complex molecules. This comes about in the following way. In the association of two atoms, two degrees of freedom in the initial state pass over to two degrees of rotation in the transition state. But for the association of two radicals, two degrees of freedom of translation and three of rotation are transformed to five vibrational degrees of freedom in the transition state.

In applying kinetical ideas to polymerisation it is fortunate that the study of gaseous chain reactions has proceeded so far, for the conceptions current in this branch of kinetics may be applied to the building of polymers. Polymerisation reactions indeed are chain reactions of a special type. We may illustrate most conveniently by considering the following typical examples. The polymerisation of formaldehyde to polyoxymethylene is a well-known reaction and recently it has been found (Norrish and Carruthers, *Trans. Faraday*

Soc., Jan. 1936) that the polymerisation of the vapour is catalysed by small quantities of formic acid. The most surprising feature of the reaction is that the rate of polymerisation increases to an indefinitely large value as the pressure of formic acid rises to a critical value. The phenomenon indeed seems analogous to the critical explosion limits of chain reactions, *e.g.* the oxidation of P_4, PH_3, H_2, etc. But the analogy is more than formal. During polymerisation the formic acid disappears at about the same rate as the aldehyde, the reaction being actually bimolecular with respect to the aldehyde concentration. While the reaction velocity increases with temperature it is essential to have part of the reaction system at room temperature, where the polymer may form, for at higher temperatures, the polymer is decomposed to the monomer. If the reaction system is wholly at a high enough temperature no polymerisation at all can be detected. The process is a heterogeneous one, that is the polymer does not grow in the gas phase but at (not on) the surface of the reaction vessel. The first step in the reaction is probably the addition of formic acid to formaldehyde to form the first chain centre, namely the monoformic ester of dihydroxymethylene

$$\mathrm{H.COOH + H.CHO = OC}\Big\langle\begin{matrix}\mathrm{OCH_2OH}\\ \mathrm{H}\end{matrix}$$

Further aldehyde molecules may then add on so that the CH_2OH group, the chain carrier so to speak, is repeated in a manner similar to that suggested by Staudinger for the polymerisation of formaldehyde in aqueous solution: $H.CO.O.(CH_2O)_n.CH_2OH$. To explain the occurrence of a critical concentration of formic acid the chains must branch, that is *two* potentially active groups must appear in the growing polymer by the addition of *one* molecule of formic acid thus

$$\mathrm{OC}\Big\langle\begin{matrix}\mathrm{O{-}(CH_2O)_n{-}CH_2OH}\\ \mathrm{H}\end{matrix} + \begin{matrix}\mathrm{HO}\\ \mathrm{H}\end{matrix}\Big\rangle\mathrm{CO = OC}\Big\langle\begin{matrix}\mathrm{O{-}(CH_2O)_{n+1}.CH}\Big\langle\begin{matrix}\mathrm{OH}\\ \mathrm{OH}\end{matrix}\\ \mathrm{H}\end{matrix}$$

In this way then the polymer acquires a lattice structure, lying probably on the surface of the glass. Below the critical concentration the chains are not indefinitely long and kinetic analysis shows that rupture must be due to the impact of the aldehyde molecule on the growing polymer. But this is the type of collision responsible for growth. In the termination collision it is probable that water is eliminated, the hydroxyl (chain propagating) group thereby vanishing.

$$\mathrm{OC}\Big\langle\begin{matrix}\mathrm{O{-}(CH_2O)_n.CH_2OH}\\ \mathrm{H}\end{matrix} + \begin{matrix}\mathrm{H}\\ \mathrm{H}\end{matrix}\Big\rangle\mathrm{CO = OC}\Big\langle\begin{matrix}\mathrm{O{-}(CH_2O)_n.CH_2.CH:O}\\ \mathrm{H}\end{matrix} + \mathrm{H_2O}$$

The same kind of polymerisation is brought about by acetic acid, and acetaldehyde is likewise polymerised by formic but not by acetic acid, presumably because acetaldehyde and acetic acid do not form an addition compound capable of propagating chains. It is of interest to note that glyoxal forms an addition compound with formic acid and the process stops there.

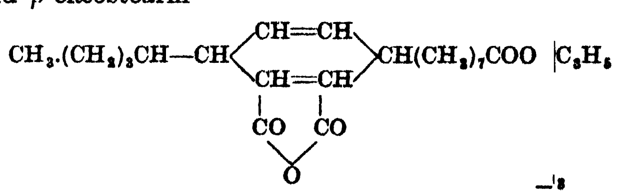
Another analogy between polymerisations and oxidations is given by a comparison of the effect of inhibitors on the photochemical polymerisation of vinyl acetate in ethyl acetate solution at 3000 Å and the thermal oxidation of sodium sulphite, the latter being a well established example of a chain reaction in solution (Jeu and Alyea, *J.A.C.S.*, **55**, 575, 1933). In the case of vinyl acetate the rate of polymerisation is given by the equation

$$-\frac{d[\mathrm{V.A}]}{dt} = \frac{[\mathrm{V.A.}].\mathrm{I}}{k_1 + k\mathrm{C}}$$

where k_1 and k are constants and I the intensity of the incident light, C is the molecular ratio Inhibitor/Vinyl Acetate, k being characteristic of the inhibitor. As is evident from an examination of the above equation, the chain length is independent of the concentration of vinyl acetate, since the rate of initiation of chains is [V.A.]I. This is just what has been found for formaldehyde and acetaldehyde and means that the chains are terminated by collisions of the polymer with the monomer, but of a different kind from those responsible for the propagation of the growth of the polymer. There is a good correlation between the action of inhibitors in the two processes thus supporting the hypothesis that the polymerisation of vinyl acetate is a chain reaction. Pyrogallol is by far the most powerful inhibitor and if it be assumed that every collision between polymer and pyrogallol leads to the deactivation of the former molecule, the chain length may be calculated in the following way. Even if this assumption is not valid, the calculation yields a minimum value for the chain length. The constant k_1 is equal to the probability of the termination of the chain in absence of the inhibitor and the ratio k/k_1 is the chain length assuming that the inhibition efficiency of pyrogallol is unity. To evaluate this ratio it is only necessary to find what concentration ($\mathrm{C}_{1/2}$) of pyrogallol cuts down the rate of polymerisation (or chain length) by one half. From the velocity equation given above it may be shown that $k/k_1 = 1/\mathrm{C}_{1/2}$. This approximate method gives a chain length of 3000 while a value of 1000 has been obtained for the photo-polymerisation of liquid vinyl acetate (*J.A.C.S.*, **53**, 2527, 1931)—a conclusive proof of the existence of chains in this reaction.

The chain carriers in gaseous reactions are either excited molecules, free atoms or radicals, that is, entities of short life. Similarly it is probable that in the majority of polymerisation reactions the active patch (or patches) on the growing polymer is subject to rapid deactivation or removal in some way. In the polymerisation of chloroprene, however, it seems that the growing polymer retains its activity for a very much longer time than that normally found. If chloroprene (Carothers, Williams, Collins and Kirkby, *J.A.C.S.*, **63**, 4214, 1933) is placed at the bottom of an evacuated tube and the upper part illuminated, the growth of the polymer starts at the illuminated spot and the polymerisation continues apparently indefinitely outwards from the spot after the illumination ceases. The polymer consists of globules or cellular crystals, but X-rays prove the substance to be amorphous. If a small fragment of the polymer is placed in liquid chloroprene growth recommences, moreover this occurs after an interval of several days ; in fact there seems to be no limit to the size of the polymer which could be grown from a minute amount of material activated photochemically. The growing polymer must therefore be much more stable than that usually encountered in polymerisation. It certainly cannot be of the nature of a free radical.

The " drying " of unsaturated oils too is believed to be some kind of polymerisation reaction. Natural oils are however somewhat impure and therefore it is not easy to interpret the results in a simple manner. Furthermore it is uncertain to what extent oxygen plays a part in the process. Fortunately a well defined compound β-elæostearin



behaves in a similar way (Morrell and Samuels, *J. Chem. Soc.*, 251, 1932). Further simplification of the operating conditions may be made by spreading the substance as a monomolecular film on aqueous substrates, in which circumstance it sets to an elastic film at room temperature in a few minutes. The kinetics of the drying may be followed by measuring the phase boundary potential or the area of a given quantity of substance at a constant surface pressure (Gee, *Trans. Faraday Soc.*, Jan. 1936). Formally the kinetics of this two-dimensional reaction bear a very close resemblance to those of typical oxidations (such as the hydrocarbons). It is probable

that an unstable peroxide is first formed. This may polymerise or it may revert to a more stable form, which in due course polymerises.

$$\begin{tikzcd} & \text{Polymer} \\ \cdot XO_2' \arrow[ur] \arrow[dr] & \\ & XO_2 \arrow[uu] \end{tikzcd}$$

The former reaction is favoured by high pressures. The interesting point about the polymerisation of XO_2 is that it exhibits an induction period, is subject to acceleration by Co^{++} ions or retardation by hydroquinone in the substrate. By mixing a saturated ester, ethyl myristate, with the film, thepolymerisation is also retarded. Kinetic analysis further shows that the absorption of oxygen is the first slow step, thereafter the polymerisation proceeds rapidly and that the growth is ultimately brought to a stop by the operation of a steric factor alone.

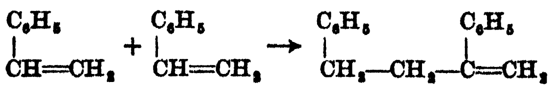
Finally mention may be made of the polymerisation of gaseous acetylene (*Proc. Roy. Soc.*, A **146**, 268, 1934). Light of $\lambda \doteqdot 2000$ Å, excited mercury atoms, α-particles all induce polymerisation to a body of the formula $(C_2H_2)_n$. No matter what stimulus is employed the number of molecules polymerised is much greater, 10–100 times, than the number of stimuli used. In this reaction too it appears that the chains are terminated by a collision between growing polymer and acetylene, the process being similar to that in operation in the aldehydes and in vinyl acetate.

Summarising then it will be seen that polymerisation may be started in a variety of ways. Since polymerisations are chain reactions the polymer is built up by the successive addition of monomers to the growing polymer and not by the interaction of monomer with monomer, dimer with dimer and so on. It may be emphasised here that chain propagation is slightly different from that usually implied by the term. In ordinary gaseous chain reactions there are in general two carriers which alternately combine with the two reactants, whereas with polymerisation only one molecule is involved even though it be growing continuously. As has already been mentioned, branching in polymerisation reactions provides the means whereby cross-linked polymers may be constructed. In so far as termination is concerned the idea that the reactant or monomer may cause cessation of the growth of polymers is analogous roughly to the type of reaction responsible for the removal of oxygen atoms in some chain oxidations, namely $O + O_2 + O_2 \rightarrow O_3 + O_2$ (*cf.* Semenoff, *Kinetics*, p. 176, Oxford). But the introduction of a steric factor (*cf.* Gee and Rideal, *Trans. Faraday Soc.*, **31**, 969,

1935) as being alone responsible for termination does not find its counterpart in ordinary chain reactions. While the idea is inherently possible, there is as yet not much direct experimental evidence in its favour. If the steric factor alone is operative it is evident that the molecular weight of the polymer will increase as the reaction proceeds, whereas if the growth of the polymer is arrested by some kind of collision the molecular weight will remain practically the same throughout the course of the reaction. In order to differentiate between the two mechanisms use might be made of the ultracentrifuge for molecular weight analysis. As yet little progress has been made in this direction.

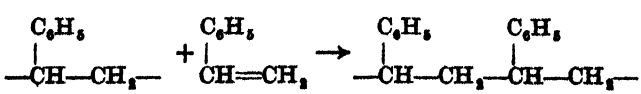
In the development of the subject of polymerisation, the study of the polystyrenes by Staudinger has played an important part. This provides a good example of how more purely chemical evidence gives a clue to the structure of the polymer. Styrene (phenylethylene) polymerises in the liquid state or in solution at room temperature and upwards in absence of a catalyst. By employing catalysts, however, it is possible to produce distyrene and tristyrene. The normal polysytrenes are more highly polymerised and stand in a class apart from the distyrenes and tristyrenes. Some estimates have shown that as many as 5000 styrene units form the polymer, though polymeric properties first come into evidence when only ten styrene units are present. The polystyrenes have been divided into three classes. The hemicolloids contain 20 to 100 units. These dissolve without swelling (in monostyrene and other liquids) forming solutions of low viscosity obeying Poiseuille's law. The viscosity is a function of the molecular weight. At the other extreme there are the eucolloids, the number of units being greater than 1000. These substances exhibit the phenomenon of swelling when immersed in suitable liquids and their solutions are highly viscous. The hemicolloids fill the intermediate range.

The polystyrenes are saturated hydrocarbons and therefore the polymerisation process may be conceived as occurring in two ways. First, two styrene molecules form distyrene, a terminal double bond appearing at each addition. The second process is the



(First process)

production of a polymer which is really a free radical, the first stage in the polymerisation being the activation of the double bond.



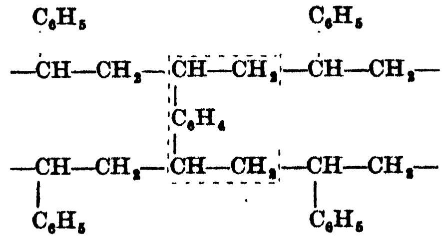
(Second process)

It has been shown by Staudinger and Steinhofer (*Annalen*, **517**, 41, 1935) that distyrene does not polymerise readily. Further, if polystyrene be separated during polymerisation and the molecular weight estimated by measuring the viscosity of its solution, there is no evidence at all that the polymer possesses a low molecular weight in the initial stages of the reaction. High polymers are produced from the start. This is what would be expected if the polymerisation chain is similar to that in gaseous reactions. It has a given length, except in so far as it may be modified by a change in the concentration of the reactants. It is probable that the number of units in the polymer is equal to the number of molecules polymerised per molecule of styrene initially activated. How the chain is stopped is a matter which has not yet been elucidated. That only the double bond of the aliphatic part of the styrene molecule is concerned in polymeric links is shown directly by hydrogenation of the polymer to hexahydro-polystyrene, which still retains the features characteristic of a polymer of high molecular weight.

Besides cryoscopic and ebullioscopic methods of measuring the molecular weight of a polymer, there exists another—namely, the viscosimetric method. The latter depends for its applicability on the assumption that the polymer consists of thread-like molecules, in which case the viscosity of the polymer solution is proportional to the molecular weight, that is, to the chain length. The solution must of course obey the Poiseuille law and therefore the method is not suitable for the eucolloids. The osmotic and viscosity methods yield results in excellent agreement, for example with nitro-cellulose in acetone, methyl cellulose in water and poly-ethylene oxide in water. But with polystyrene, the osmotic values are definitely much larger—3–4 times. Determination of the molecular weight of the polystyrenes by the ultracentrifuge supports the osmotic data. The discrepancy may be explained if it is assumed that the higher polystyrenes have a tendency to form branched chains and even ring polymers. The viscosity technique only indicates the *length* of the polymer chains and cannot yield any information about branching.

The intentional branching of polystyrene chains has been recently accomplished by Staudinger (*Ber.*, **67**, 1166, 1934) in an ingenious way. If a minute quantity (0·01–0·002 per cent.) of *p*-divinyl benzene is added to styrene, a polymer is obtained showing identical properties to ordinary polystyrene except that it does not swell when immersed in organic liquids. By decreasing the amount of divinyl benzene, soluble polymers may be obtained, but they exhibit anomalies when their molecular weight is determined by the viscosity

technique. The deviations are greater the larger the concentration of divinyl benzene. These observations can only be satisfactorily explained by assuming that the divinyl benzene cross-links the straight polystyrene chains thus:



the divinyl benzene taking up the position shown within the dotted lines. This process is exactly analogous to that observed in gaseous chain reactions, where the addition of certain molecules, for example, nitrogen peroxide in the $H_2 - O_2$ reaction, leads to vigorous branching of the reaction chains (Semenoff, *loc. cit.*, p. 234).

While the kinetics of polymerisation reactions are now beginning to throw some light as to how polymers build up, the method is restricted in its scope. Such a method can only provide starting-points for the investigation of the structure of polymers by X-ray spectrography (*cf.* Katz, *Trans. Faraday Soc.*, Jan. 1936). The conclusions arrived at in this complementary method of attack fit in very well with those arrived at by purely chemical evidence. Staudinger was probably the first to suggest that the simplest type of polymer consists of long chains of molecules in which a relatively small unit is repeated indefinitely, the units being united to each other by primary valencies. This in fact has been put forward as a definition of a polymer. The early X-ray evidence showed that the molecular weight of the elementary cell was very much smaller than the molecular weight deduced by physico-chemical methods, in substances such as cellulose, silk, rubber, etc. The anomaly was explained when it was realised that the unit cells could be united to each other by primary valencies to form a chain-like molecule. Perhaps the best example of the agreement between physical and chemical evidence is evident in the smaller polymers of formaldehyde —the polyoxymethylenes. These have the structure

$$HO—CH_2—(OCH_2)_n—O—CH_2OH$$

By acetylating or methylating the hydroxyl groups the molecular weight of the polymer may be estimated. Such polymers, however, give rise to X-ray interference rings, the position of which can be correlated with the size of the polymer, thereby furnishing an estimate

of the length of the polymer. The length of the polyoxymethylene molecule so obtained is exactly proportional to the molecular weight determined chemically. The polymer must therefore consist of long rods of CH_2O units joined together. That this rod-like structure is a characteristic of many polymers of high molecular weight is very directly shown by the fact that they may be spread as monomolecule layers on an aqueous substrate.

Not all polymers give so well defined an X-ray pattern as the polyoxymethylenes. The polymers of substances such as butadiene, isoprene, $\beta\gamma$-dimethyl butadiene all produce different rings. Similarly the monomeric liquids give rise to diffuse rings, thus showing that in such liquids the molecules are not distributed at random but lie in parallel groups, the axes of the *groups* being distributed at random. The most significant point, however, is that the characteristic distance calculated from the ordinary Bragg formula is precisely identical in the liquid monomer and in the polymer. An increase of 0·3 Å is brought about by the substitution of a methyl group for a hydrogen atom as may be seen from the following table.

	Å		Å
Butadiene	4·6*	Polymer	4·6
Isoprene	4·9	Polymer	4·9
$\beta\gamma$-dimethyl butadiene	5·3	Polymer	5·3

* This is the distance between the molecules in the group perpendicular to the chain.

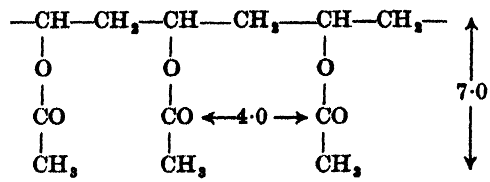
This is just the result to be expected if the polymer is built up, as suggested by Staudinger, by the addition of monomer to the growing polymer resulting in a rod-shaped molecule, for example, isoprene polymer:

$$CH_2.CH{=}\overset{\overset{CH_3}{|}}{C}{-}(CH_2)_2{-}CH{=}\overset{\overset{CH_3}{|}}{C}{-}CH_2{-} \quad .$$

The greater the number of CH_3 groups the greater the separation of the groups. In the same way the phenol-formaldehyde polymers may be correlated with the phenols from which they are formed. This information may be applied to rubbers synthesised by Harries in 1912. Three synthetic polymers were made: (*a*) the so-called Kondakow polymer which grows in liquid butadiene at room temperature, (*b*) the sodium polymer made by the interaction of sodium and the monomer, and (*c*) sodium carbonic acid polymers made by the addition of sodium to the liquid saturated with carbon dioxide. X-ray evidence shows that (*a*) and (*c*) and the thermal polymers are of similar structure, whilst the sodium polymer is entirely different.

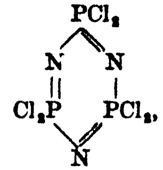
This agrees with chemical evidence afforded by an examination of the reactions of the ozonides.

The similarity in the structure of the liquids and their derived polymers is not always found. Besides the sodium polymer of butadiene, methyl acrylate and vinyl acetate yield patterns quite different from those of the polymer. Again when polymerisation takes place, an extra smaller diffuse ring, indicating larger spacing, appears on the X-ray photographs which may at once be ascribed to a characteristic distance in the polymer. Polyvinyl acetate behaves in this manner. At room temperature it is resinous, but at 100° it becomes elastic and may be stretched. Normally two diffuse rings are obtained, the outer one corresponding to a separation of 4·0 Å, probably the distance between acetyl side chains, and the inner to a separation of 7·0 Å. This latter distance is most likely that between the hydrocarbon chains, for when stretched two equatorial maxima appear on the inner ring, presumably being produced as a result of the parallel orientation of the hydrocarbon chains.



While polymers of rubber-like nature are normally conceived of as being derived from organic materials it is interesting to note the progress that has been made in elucidating the structure of two inorganic elastic substances—namely, elastic sulphur and polyphosphonitrilic chloride. When elastic sulphur (Meyer and Go, *Helv. Chem. Act.*, **17**, 108, 1934) is stretched, the X-ray diagram shows the imposition of a fibre-like structure with long zigzag chains of sulphur atoms arranged parallel to the direction of stretching, each atom lying linked to two neighbours by homopolar bonds. In ordinary crystalline sulphur on the other hand, aggregates consisting of S_8 molecules may clearly be distinguished, as might be expected from the fact that S_8 molecules in the vapour phase are in equilibrium with the solid. The kinetics of the transformation of metastable fluid sulphur to the elastic modification, in a qualitative way, exhibit those characteristics commonly associated with polymerisation reactions, for example, catalysis by small quantities of polymer and inhibition by small quantities of substances liable to combine with sulphur such as halogen. With the polyphosphonitrilic chloride

(Stokes, *J. Amer. Chem. Soc.*, **17**, 275, 1895), obtained by heating substances of the formula



similar phenomena occur. Fibre pattern shows up when the polymer is subjected to tension (Meyer, *Trans. Faraday Soc.*, Jan. 1936).

Further information bearing on points dealt with in the above review is to be found in the following:

(1) Faraday Society Discussion, September 1935, Cambridge, to be published in January 1936.

(2) J. R. Katz, *Die Roentgen Spektrographie als Untersuchungsmethode bei Hochmolekularen Substanzer.*

(3) H. Staudinger, *Die Hochmolekularen organischen Verbindungen, Kautschuk und Cellulose*, Springer, Berlin.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., The University, Glasgow.

Major Tectonics: Stratigraphical and Regional Geology of Great Britain.—R. W. Van Bemmelen's important undation theory, which is deserving of more extended examination than can be given in these notes, is well summarised in the following paper ("The Undation Theory of the Development of the Earth's Crust," *Rept. of XVI Internat. Geol. Congress, Washington*, 1933. Reprint issued 1935, 18 pp.). The gist of it may be given in van Bemmelen's own words: "The cooling of the earth follows from the second law of thermodynamics. This cooling affects mainly the outer 100 to 120 km., where the primeval silicate melt is differentiating. This differentiation disturbs the thermal and gravitational equilibrium of the parent magma (equilibrioturbal action) (*turbare*, to disturb). Subcrustal mass displacements work towards readjustment of this equilibrium (equilibriopetal reaction) (*petere*, to strive for). These subcrustal mass displacements are accompanied by uplifts and subsidences at the surface (undations). These undations are in turn equilibrioturbal for the gravitative equilibrium in the outer crust and cause sliding and folding of strata. Volcanism, earthquakes, and gravity anomalies are other consequences of this magmatic differentiation and these subcrustal mass displacements. The displacements of the earth's crust are thus the result of

the reciprocal effect of equilibrioturbal magmatic differentiation and equilibriopetal gravitative adjustment."

Van Bemmelen also deals with "Das Krafteproblem in der Tektonik" (*Proc. Kon. Akad. v. Wetensch. Amsterdam*, XXXVI, 1933, 197–202); and, in the following papers, has applied the undation theory with some success to problems of the origin of several major earth structures: "Die Undationstheorie, und ihre Anwendung auf die mittelatlantische Schwelle" (*Zeitschr. d. Deutsch. Geol. Ges.*, **85**, 1933, 762–80); "Die Anwendung der Undationstheorie auf das Alpine System in Europa" (*Proc. Kon. Akad. v. Wetensch. Amsterdam*, XXXVI, 1933, 686–94); "Versuch einer geotektonischen Analyse Südostasiens nach der Undationstheorie" (*ibid.*, 730–9); "Versuch einer geotektonischen Analyse Australiens und des Südwestpazifik nach der Undationstheorie" (*ibid.*, 740–9).

Mathematical treatment of geotectonic movements with special reference to the undation theory has been attempted by R. W. van Bemmelen and H. P. Berlage, Jr. (*Gerlands Beitr. z. Geophysik*, **43**, 1934, 19–55).

E. M. Anderson has published two interesting papers on "Earth Contraction and Mountain Building" (*Gerlands Beitr. z. Geophysik*, **42**, 1934, 133–59; **43**, 1934, 1–18). His own summaries may be here given: "The surface flow of heat at present is probably less than has been assumed by some recent writers. . . . The low value is in favour of low crustal temperatures, such as are assumed by supporters of the Contraction theory, but other considerations are against this. Reasons may be given for assuming that the 'granitic layer' of the earth's crust has somewhat the same composition as the Scottish Lewisian Gneiss. It must in that case have a lower radioactive content than normal granite. The sedimentary layer which overlies it is in places at least 6 km. in thickness, and this layer is also poor in radioactive constituents. By an apparent paradox, these facts make it possible to deduce higher temperatures for the deeper continental layers.

"The resistance of rock-masses to very long-continued pressures is at present unknown, and it cannot be tested in the laboratory. If the Contraction Theory is true, the upper layers of the earth's crust must be able to resist a very large stress for millions of years, without being deformed beyond the elastic limit. These layers must however 'creep' over a substratum which can only have about a thousandth part of the strength which exists above. This is a necessary inference, which appears to tell against the validity of the theory."

In a closely argued paper P. R. Thompson (*Geol. Mag.*, LXXII,

1935, 377–80) discusses the relations of thermal contraction, land bridges and geosynclines, assuming that the elevation of the land areas is due to lateral compression. The compression and the correlated stretching "would constitute a system of differential crustal movements involving extensive continental drift."

C. R. Longwell raises the question: "Is the 'Roots-of-Mountains' Concept dead?" (*Amer. Journ. Sci.*, XXIX, 1935, 81–92). Airy's original hypothesis seems to be shaken by the fact, recently demonstrated, that granite weakens and melts at lower temperatures than basalt. Nevertheless, Professor Longwell pleads for suspension of judgment until the evidence is fully reviewed. He favours a view based on a combination of the hypotheses of Airy and Pratt.

Recent reconnaissances in south-western Persia have convinced J. V. Harrison and N. L. Falcon (*Geol. Mag.*, LXXI, 1934, 529–39) that certain problematical folding structures in massive limestones are due to the action of gravity on steep folds where the limestone rests on shale. When the crowns of the anticlines are broken by erosion, the outer sheet of limestone tends to become "unstuck" and collapses in backwardly-directed folds down into the synclines. These structures, which are produced at the surface under no cover whatever, are termed "collapse structures."

R. Willis offers a detailed discussion of the mechanism whereby thrust faults originate and develop (*Bull. Geol. Soc. Amer.*, **46**, 1935, 409–24).

In a paper on "The Influence of Earth Movements on Climate" R. G. Lewis (*Geol. Mag.*, LXXII, 1935, 64–73) cites numerous instances of coincidence in local change of climate and local crustal movement, and suggests that there is an unexplained connection between the phenomena. Glaciation may be a local matter, and the Pleistocene glaciations may not be due primarily to a general lowering of temperature.

M. Y. Williams, discussing the "Distribution of Life around the Pacific" (*Proc. Fifth Pacific Sci. Congr., Canada*, 1933, 1934, 3107–14), shows that the distribution of fossil and living land life around the Pacific demands land connections between existing land masses at various periods in the past, and it is possible that every part of the greatest of oceans has been bordered by land at one time or another.

P. E. Raymond's Presidential Address to the Palæontological Society of America deals with "Pre-Cambrian Life" (*Bull. Amer. Geol. Soc.*, **46**, 1935, 375–92), and is an attempt to explain the general absence of fossils from Pre-Cambrian strata. He suggests

that there is an inverse relationship between activity and hard parts in animals. Activity means getting rid of surplus lime. "That the Pre-Cambrian animals were all motile, seems, therefore, to explain their lack of hard parts, and hence to solve the question as to why so few Pre-Cambrian fossils are found."

Of especial interest to British stratigraphers is Professor S. von Bubnoff's extract from his monograph, *Geologie von Europa*, entitled "Die Epeirogenese Westeuropas" (*Geologie von Europa*, II, 2, 1935, 1124–32). The work is illustrated by an illuminating map of the epeirogenetic and tectonic subdivisions of the superstructure of Western Europe.

In his useful review of "Europäische und Nordamerikanische Gebirgszusammenhänge" Professor F. E. Suess (*Rept. of XVI. Internat. Geol. Congr., Washington*, 1933, 1935, pp. 14, reprint) supports Bailey in coming to the conclusion that, "The angular crossing of Caledonids and Hercynids on both sides of the Atlantic Ocean furnishes, in conjunction with a variety of stratigraphic relationships, the most significant indication of the former connection of North America with Europe in the sense of Wegener." The Hercynian Appalachians were bordered by a land mass on the site of the present West Atlantic. By pressure the chain was welded to Laurentia just as the Caledonids were welded to Fennoscandia.

Professor C. Schuchert (*Amer. Journ. Sci.*, XXX, 1935, 394–6) reviews another monumental work by A. W. Grabau (*Palæozoic Formations in the Light of the Pulsation Theory: Part* 1, *Lower Cambrian Pulsations; Sci. Quart. Nat. Univ. Peking*, IV, No. 1, 1934, 27–184; *Part* 2, *The Middle Cambrian or Albertan Pulsation; ibid.*, No. 4, 1934, 355–832; *Parts* 3 *and* 4, *The Cambrovician* (*sic*) *Pulsation; ibid.*, V, Nos. 1 and 2, 1935, 1–316) and says: "In these thousand pages Professor Grabau gives a detailed survey of the stratigraphy and faunas of the Cambrian and Lower Ordovician formations throughout the world, in the light of his Pulsation Theory, which he thinks will give a natural basis for the classification of the marine rhythms into systems of strata or periods of time. It is well known that everywhere the stratigraphical record is more or less interrupted by breaks in sedimentation, the disconformities and unconformities, and it is Grabau's purpose to pick out from among these the ones that are widely spread or are of major import, since these delimit the oceanic 'pulsations' between the close of the regressive and transgressive halves of the systems." The Pulsation Theory holds that "the major transgressions indicated by the progressive overlap of the strata were essentially synchronous in all continents, while the major regressions

indicated by progressive off-lap of strata, also took place concurrently in all parts of the world, and that moreover transgression and regression alternated in rhythmic succession throughout at least the Palaeozoic era."

In his paper on "Gotlandian Vulcanicity in Western Europe" J. S. Turner (*Geol. Mag.*, LXXII, 1935, 145–51) shows that Gotlandian volcanic rocks within this region occur in a narrow belt which runs nearly west to east from South-western Ireland to Bohemia. They include rhyolites, andesites, keratophyres, diabases and basalts. Their age varies from early Valentian in Brabant to Upper Salopian in Bohemia. Gotlandian volcanicity seems to have been independent of the preceding Taconic orogeny, and its distribution is not parallel to the main Caledonian folding. The Irish–Belgian section corresponds closely to the northern boundary of the Westphalian zone of Variscian folding.

In another paper the same author discusses Dinantian and Namurian Vulcanicity in North-Western and Central Europe (*Geol. Mag.*, LXXII, 1935, 458–70). The history of igneous events is long and complicated, but granitic intrusions were characteristic of the Variscian fold-mountain zones, while basaltic lavas were erupted in foreland regions.

A detailed review of "The Downtonian and Dittonian Strata of Great Britain and North-western Europe" is given by W. W. King (*Quart. Journ. Geol. Soc.*, XC, 1934, 526–70), on the basis of which a new scheme of correlation in regions as far apart as Spitsbergen and Cornwall is adopted, and the conclusion is come to that the Lower Gedinnian is the stratigraphical equivalent of the English Downtonian.

In his paper on "The Division of the Late Palæozoic Formations of Gondwanaland" A. L. du Toit (*Rept. XVI. Internat. Geol. Congr., Washington*, 1933, 1934, 12 pp. reprint) concludes that the basal glacial and inter-glacial deposits fall wholly within the Carboniferous, the earliest phases being as old as Dinantian. He thinks it impracticable as yet to draw a boundary line between the Lower and Upper Carboniferous glacial deposits.

In his memoir on "Correlations of the more important marine Permian sequences" C. Schuchert (*Bull. Geol. Soc. Amer.*, **46**, 1935, 1–46) includes a valuable section on "What constitutes a [geological] system?" He reviews the various bases of delimitation, and concludes that, in correlating widely-separated regions, the most dependable basis must be fossils, and especially the marine invertebrates.

In an important paper, "The Pleistocene Chronology of Central

Europe," F. E. Zeuner (*Geol. Mag.*, LXXII, 1935, 350–76) states that, as a consequence of twenty years' work by a large number of geologists on the Pleistocene of Central Europe, it is now possible to establish a detailed chronology which proves to be generally applicable. When employed with certain astronomical data it enables horizons, fossils and implements of many localities to be dated in terms of thousands of years.

The main object of Professor E. B. Bailey's paper on "The Glencoul Nappe and the Assynt Culmination" (*Geol. Mag.*, LXXII, 1935, 151–65) is to correct a probable mistake in previous publications in regard to the course and behaviour of the Glencoul Thrust E.N.E. of the head of Loch Assynt. It is now believed that this thrust comes to an end at this point, and does not join up with the Ben Uidhe Thrust at the surface as formerly supposed.

In his Presidential Address to the Geological Society, J. F. N. Green (*Quart. Journ. Geol. Soc.*, XCI, 1935, lxiv-lxxxiv) dealt with the problem of the Moine Series from the standpoint of general geology. Mr. Green showed that, as far as the available evidence goes, there is no proved break in metamorphic facies between the Moines and the neighbouring areas of Lewisian types, except where faulting is concerned. This criterion therefore favours, but not conclusively, Barrow's hypothesis that the Moines are of Lewisian age, and have been brought up by the great thrusts.

Professor E. B. Bailey's new work on "Highland Tectonics: Loch Leven to Glen Roy" (*Quart. Journ. Geol. Soc.*, XC, 1934, 462–525) has led to certain new conclusions and to the rectification of former views. The Appin Phyllite is now regarded as the youngest formation. The Appin, Aonach Beag and Ballachulish Folds are found to be recumbent synclines closing to the south-east; and the main slides, namely the Fort William and Ballachulish Slides, are shown to be lags. The Stob and Kinlochleven Recumbent Anticlines, with their corresponding great inversions, are described in some detail. All these structures have been intensely refolded.

In her paper on "The Loch na Cille Boulder Bed and Its Place in the Highland Succession" Dr. G. L. Elles (*Quart. Journ. Geol. Soc.*, XCI, 1935, 111–49) concludes that the evidence is strong for the view that the Volcanic Series is the oldest, and the Quartzitic Series the youngest member of the Loch Awe Group, with the Boulder Bed or Slaty Schists with or without Limestones coming next to the Volcanics and the Limestone-Slaty Schist Series below the Quartzites. This reading of the geology is at variance with recent detailed work in a neighbouring area (see discussion).

Study of "The Dalradian Succession in the Pass of Brander,

Argyll," by J. G. C. Anderson (*Geol. Mag.*, LXXII, 1935, 74–80) shows that the Leven Schists and Ballachulish Limestone dip off Glencoe Quartzite exhibiting uninverted bedding. This agrees with results obtained in the type locality at Ballachulish. Other observations suggest that correlation with the Islay sequence is also probable.

In his paper on "The Metamorphic Rocks of Inishowen, Co. Donegal," Dr. W. J. McCallien (*Proc. Roy. Irish Acad.*, XLII, Sect. B, No. 15, 1935, 407–42) gives a detailed description of the stratigraphy, structure and metamorphism of a series which corresponds with the Dalradian of Western Scotland from the Islay Quartzite to the Loch Avich Group. The Malin Head and Crana Quartzites respectively are correlated with the Islay and Crinan Quartzites of Scotland; but there is no evidence in Donegal to suggest that the two quartzites are one and the same, as has been suggested for their Scottish equivalents.

The remarkable "Ordovician Submarine Disturbances in the Girvan District" which are mainly recorded in the peculiar lithological features of the Ardwell Beds, are fully described and beautifully illustrated by S. M. K. Henderson (*Trans. Roy. Soc. Edin.*, LVIII, Pt. II, 1935, 487–509). Local unconformities of some importance have been found to occur at the bases of the Benan Conglomerate and the Ardwell Beds. The latter exhibit striking examples of intraformational folding and brecciation, graded and current bedding, which are attributed to the effects of a succession of submarine earthquakes.

The classical Old Red Sandstone region of the Orkneys is described in a new Survey Memoir (*The Geology of the Orkneys, Mem. Geol. Surv. Scotland*, 1935, 205 pp.). Professor D. M. S. Watson, Professor W. H. Lang and Sir J. S. Flett provide chapters on fossil fishes, fossil plants and petrography respectively.

Jurassic sediments occupy an unexpectedly large area in the Trotternish peninsula of northern Skye. Dr. Malcolm MacGregor (*Proc. Geol. Assoc.*, XLV, 1934, 389–406) has studied the general geology of these strata, which are much disturbed by the intrusion of Tertiary dolerite sills.

Numerous papers and memoirs on English stratigraphy and regional geology have recently been published. It is only possible to comment on a few of these. A full description of the Cambrian succession of the Rushton Area (Shrops.) has been given by E. S. Cobbold and R. W. Pocock (*Phil. Trans. Roy. Soc., Ser. B.*, **223**, 1934, 305–409). One hundred and one species of fossils, 29 of which are new, have been determined. The Upper Cambrian is

divided into six named zones, the Middle Cambrian into five, and the Lower Cambrian into six. Other papers are:—

E. S. Cobbold and W. F. Whittard, "The Helmeth Grits of the Caradoc Range, Church Stretton; their Bearing on Part of the Pre-Cambrian Succession of Shropshire" (*Proc. Geol. Assoc.*, XLVI, 1935, 348–59); L. J. Wills, "An Outline of the Palæogeography of the Birmingham Country" (*ibid.*, 211–46); T. G. Williams, "The Pre-Cambrian and Lower Palæozoic Rocks of the Eastern End of the St. David's Pre-Cambrian Area, Pembrokeshire" (*Quart. Journ. Geol. Soc.*, XC, 1934, 32–75); O. T. Jones and W. J. Pugh, "The Geology of the Districts around Machynlleth and Aberystwyth" (*Proc. Geol. Assoc.*, XLVI, 1935, 247–300); P. G. H. Boswell, "The Geology of North-Western Denbighshire" (*ibid.*, 152–86); B. Smith, "The Mynydd Cricor Inlier" (*ibid.*, 187–92); D. Parkinson, "The Geology and Topography of the Limestone Knolls in Bolland (Bowland), Lancs. and Yorks." (*ibid.*, 97–120); W. B. R. King and W. H. Wilcockson, "The Lower Palæozoic Rocks of Austwick and Horton-in-Ribblesdale, Yorks." (*Quart. Journ. Geol. Soc.*, XC, 1934, 7–31); J. S. Turner, "Structural Geology of Stainmoor, Westmorland, and Notes on the Late Palæozoic (Late-Variscian) Tectonics of the North of England" (*Proc. Geol. Assoc.*, XLVI, 1935, 121–51).

Dr. W. J. Arkell has studied "The Nature, Origin and Climatic Significance of the Coral Reefs in the Vicinity of Oxford" (*Quart. Journ. Geol. Soc.*, XVI, 1935, 77–110). The resulting map of the Coral Rag shows that this formation is distributed in irregular patches which are interpreted as reefs in position of growth, separated by tongues and strips of the bedded detrital Wheatley Limestone types of deposit, which are interpreted as channels filled with current-bedded detritus.

The fine but comparatively neglected sections of the Portland Beds on the Dorset mainland are now fully described by Dr. W. J. Arkell (*Proc. Geol. Assoc.*, XLVI, 1935, 301–47). He urges that the Portlandian Stage should be reinstated and redefined on a proper historical basis.

A new boring for water at Alford has enabled Professor H. H. Swinnerton to study the rarely-exposed strata immediately beneath the Red Chalk of Lincolnshire (*Quart. Journ. Geol. Sci.*, XVI, 1935, 1–46). A bed of light-coloured marl which contains exceptionally interesting fossils was found beneath the Carstones, and has been christened the Sutterby Marl.

A full treatment of "The Eocene and Pliocene Deposits of Lane End, Bucks," where many new sections have been opened, gives S. W. Wooldridge and C. J. C. Ewing (*Quart. Journ. Geol. Soc.*,

XCI, 1935, 293–317) the opportunity of reviewing the lithological characters and variations of the Reading Beds as a whole.

The object of Miss M. W. Tomlinson's paper on "The Superficial Deposits of the Country North of Stratford on Avon" (*Quart. Journ. Geol. Soc.*, XCI, 1935, 423–62) is to put forward a classification of the superficial deposits of this critical Midland area with a view to the determination of their distribution and relative ages. A description of the drift deposits is followed by discussion of the relation between the Eastern and Western Drifts, and the effects of glaciation on the drainage. The final section shows the suggested chronological sequence of the superficial deposits.

Dr. J. D. Solomon's work on "The Westleton Series of East Anglia: Its Age, Distribution and Relations" (*Quart. Journ. Geol. Sci.*, XCI, 1935, 216–38) shows that this series is a widespread marine deposit contemporary with a glacial phase which brought Scandinavian ice to the Norfolk coast. The North Sea Drift, a boulder clay laid down by this ice sheet, is regarded as an intercalation in the Westleton Beds. Certain glacial sands in Suffolk, Essex, Hertfordshire and Middlesex are correlated with the Westleton Series.

BOTANY. By PROFESSOR E. J. SALISBURY, D.Sc., F.R.S., University College, London.

ECOLOGY.—The ecology of Studland Heath is described by R. Good (*Journ. Ecology*, XXIII, 2, 361). The area has a rainfall of approximately 28 inches per annum and this low amount of precipitation is associated with a porous sandy soil almost devoid of calcium. The chief type of vegetation is heath differentiated mainly on the basis of the supply of soil moisture.

In the same Journal (XXIII, 2, 406) F. W. Anderson and P. Falk describe the vegetation of the central desert of Iceland, which has a soil that is derived from weathered basalt and so porous that, despite an annual rainfall of perhaps some 46 inches, the soil is almost always dry except when snow covered from September to May. The scree and valley slopes bear a sparse vegetation to which *Cerastium alpinum*, *Arabis petræa*, *Armeria maritima*, *Silene maritima* and *Elymus maritimus* contribute. The oases present a swamp flora in which species of Willow are the dominants (*Salix lanata*, *S. phylici folia*, *S. herbacea* with *Poa alpina*, *Calamagrostis neglecta* and *Polygonum viviparum* as frequent constituents). The hill-tops by reason of the greater soil stability also have a richer flora than the slopes, and here occur *Silene acaulis* and *Saxifraga oppositifolia*. In all,

48 species of vascular plants were recorded, 1 equisetum and 19 mosses.

From a study of Wood Fen near Ely, H. and M. E. Godwin and M. H. Clifford confirm Skertchley's views as to the sequence, namely, in order from below upwards, (1) Oak, (2) Oak-Yew, (3) Pine, (4) Pine, (5) Alder-Willow. The Pine stage is held to be post-boreal, possibly of Bronze Age. During the last 200–300 years artificial drainage has caused a fall in the surface level of the peat of about 10 feet (*Journ. Ecol.*, XXIII, 2, 509).

The vegetation of pathways is described by G. H. Bates (*ibid.*, p. 470). The most characteristic species in the examples studied were *Poa pratensis*, *Lolium perenne*, *Trifolium repens*, *Bellis perennis*, *Plantago major*, *P. coronopus* and *Hieracium* spp. The increased density due to treading of the soil appears to be confined to the first three to five centimetres depth and the main factors responsible for the composition of the footpath flora are held to be the mechanical injury due to treading and the puddling effect. On cart-tracks *Plantago major*, *Polygonum aviculare*, *Matricaria suaveolens*, *Senebiera Coronopus*, *Poa pratensis* and *Agrostis stolonifera* were characteristic, all of which appear to be favoured by the diminished competition for light.

H. Jenny in *Soil Science* (**40**, 111–28, 1935) discusses the relation of the clay content of soils to climatic factors. From examination of 151 soils in the Eastern States of America it is concluded that temperature is the chief factor affecting the clay content, which in a variety of parent materials shows an increase from north to south. With increase of temperature the clay content increases exponentially. In relation to moisture when the temperature is constant the clay content is held to follow the equation m (N.S. quotient) $+ n =$ per cent. of Clay, where n and m are constants. For idealised soils derived from gneiss or granite with a temperature of 10° C. the clay fraction with an N.S. quotient of 100 would be about 4·6 per cent., 9·2 per cent. with an N.S. quotient of 200 and 18·5 per cent. with an N.S. quotient of 400. In other words the clay fraction increases proportionally to the N.S. quotient. The clay content for this purpose is the average for a depth of 36 to 40 inches taking the clay-content of the surface as one-quarter and that of the subsoil as three-quarters.

Morphology.—An interesting abnormality in *Arachis hypogea* is reported by J. S. Patel and G. N. Narayana (*Current Science*, IV, 102, 1935). Whilst the vegetative shoots of the earth nut are negatively geotropic, the fruit-stalks which develop after fertilisation are positively geotropic, and thus the pods become buried

in the soil. In the specimen described by the authors, of the twenty-five gynophores fourteen were normal but seven grew erect, whilst the remaining four exhibited a positive response at first but subsequently changed their geotropic response and bent upwards. The abnormal erect gynophores did not produce pods when growing in the air. Experiments in which gynophores were allowed to penetrate various media showed that pods do not develop in sawdust, cotton wool or air, whether dry or moist, but in denser media such as sand, powdered charcoal, etc., set pods were formed alike when the medium was dry or moist. It is suggested that the mechanical resistance is the stimulus inducing pod formation. The only striking difference in structure of the abnormal gynophores as compared with the normal was the scanty development of plastids.

B. L. Gupta in the same Journal (p. 104) records details of the development of the pollen and embryo-sac in *Wolffia arrhiza*. It appears that the spherical anther contains two archisporial groups and dehisces at the apex along a line of weakness formed by disintegration of endothecial cells. The ovule is orthotropous with two integuments and a nucellus which is almost wholly destroyed, except at the apex, by the developing megaspore-mother-cell. An eight-celled embryo-sac is formed in which the antipodals are ephemeral and the polar nuclei fuse at an early stage.

The value of the type of seed in classifying the species of Rhododendron is dealt with by Kingdon Ward in the *Journal of Botany* for September (LXIII, 873, 241–7, 1935). Three main groups are recognised. The first, common to most arboreal types, has an oval flattened form with a membranous border all round, frequently expanded at the extremities into a fin-like lobed structure. The seed is here finely striated and the length is usually less than three times the breadth. By contrast, the second type which is characteristic of the epiphytic species has a spindle-shaped form with a length of four to six times the breadth, the ends drawn out sometimes into long tails and the tips frequently in fin-like expansions. The seed is here too finely striated and is clearly a modification of that characterising the arboreal types. In the third group the seeds are rounded or angular, either striated or pitted, and devoid of both the marginal membrane and terminal processes. This type is found in most of the alpine species and characterises the typically alpine sections, but the alpines belonging to the section *Neriiflorum*, for instance, have winged seeds and are regarded as derived from forest-inhabiting ancestors. Also this type is found in non-alpine sections and is regarded by the author as the most primitive.

Poor root development has been found by T. C. N. Singh (*Current*

Science, IV, 30, 1935) to be associated with sterility, and experimental irrigation with water and added salts was found to remedy the condition, in *Cajanus indica.*

Cytological.—A cytological study of four microspecies of *Erophila verna* has been made by R. Griesinger (*Flora*, **29**, 363–80, 1935). Of these, three exhibited a haploid chromosome number of twelve and the other a haploid chromosome number of about twenty. In all four microspecies there was a normal reduction division in the embryo-sac resulting in an eight-nucleate condition. The fertilised egg develops in a similar manner to that of *Capsella.* The numerous microspecies of this genus and the fact that they produce seeds even when the flower remains closed suggested the possibility that they might be not merely self-fertilised but perhaps even apomictic as in other polymorphic types such as *Taraxacum* and *Alchemilla.* The author however found that in the four microspecies studied seed was never produced by either castrated or normal flowers without pollination and no cytological indication of apomixy was observed.

From an investigation of various Florideæ, M. A. Westbrook concludes that the members of this group have a normal type of nucleus of small size with numerous chromosomes. In nine different genera a spireme stage was recognised in the prophase of the first sporangial division. No evidence was obtained of any relation between the nucleolus and chromosome formation (*Beihefte z. Bot. Centralb.*, LIII, 564–85, 1935).

Cryptogams.—Recent work in this field includes the elucidation of the life histories of several interesting species of algæ. The Floridean fresh-water alga *Chantransia violacea* has been studied by K. M. Drew (*Ann. Bot.*, XLIX, 439–50, 1935) who places it in the genus Rhodochorton. Both tetrasporic and sexual plants are found but either may reproduce through the formation of monospores. The winter phase would appear to be tetrasporic and the summer phase sexual whilst monosporic reproduction is most frequent in the winter phase. The sexual organs are similar to those of marine species of *Rhodochorton.*

Investigation of the Brown alga *Philoeospora brachiata* by W. T. Mathias (*Publ. Hartly, Bot. Labs.*, 13, 1935) shows that during the winter months it is a partial parasite living endophytically in the thallus of *Rhodymenia palmata.* About mid-April shoots extend from the endophytic filaments and develop externally on the host thallus. These shoots give rise to unilocular sporongia from which haploid biciliated zoospores are formed. The zoospores form small filamentous plants, bearing multilocular sporangia, and the gametes

which these produce fuse to form the zygote that develops into the endophytic plant. In mid-September the reproductive shoots die down so that only the endophytic condition is to be found during the winter.

The genera *Sargassum* and *Cystophyllum* differ from most Fucaceæ in the mode of liberation of the oogonia which are here temporarily attached by stalks of mucilage and are fertilised in the attached state. E. M. Delf records the attached condition as also found in *Bifurcaria* and probably also in *Marginaria*. It is suggested that the attached oogonium is associated with the unisexual condition in deep water species (*New. Phyt.*, XXXIV, 245–59, 1935).

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—A useful directory of the Bureau of Entomology and Plant Quarantine has recently been issued by the U.S. Department of Agriculture (*Misc. Pub.* No. 220, 1935, 88 pp.). This contains a statement of the functions of the Bureau with the names and addresses of the administrative leaders. A list of laboratories, officers and field headquarters is arranged alphabetically and, in addition to the name of the person in charge and the address, a brief statement of the work being carried on is given in each case. A personnel index and a division index increases the usefulness of this publication.

R. T. Holway (*Psyche*, **42**, 1935, 1–24) has lately made a study of the pretarsus or claw segment of insects with a view to its possible phylogenetic significance. He has indicated the type of claw segment found in the more important insect orders together with the common basic plan. The author has also outlined possible homologies. This investigation has shown that there are most significant agreements in many cases and there are correlations which fall in with phylogenetic concepts already accepted or debated from the evidence of the comparative morphology of other structures.

A valuable review of the experimental evidence concerning the location of olfactory receptors in insects has been compiled by J. Marshall (*Trans. R. Ent. Soc. Lond.*, **83**, 1935, 49–72).

C. Harukawa, R. Takato and S. Kumashiro (*Ber. Ōhara Instituts. f. Landwirts. Forsch. in Kuraschiki*, **7**, 1935, 1–97), who have studied the population density of the Rice-Borer, have made a notable contribution to the knowledge of insect fluctuations in the field over long periods. For some years the numbers of the moths caught by light-traps have been used as a basis for predicting the abundance as well as the time of appearance of the insect. How-

ever, the present investigation has shown that rice-borers may become very abundant even when the moth population at the beginning of the generation is not very large, *i.e.* the larval population is not proportional to that of the moths caught. The light-trap therefore is not a general index of the abundance of the larvæ in the coming generation. The population density can better be found by collecting the injured rice culms several times during the growth period of the rice-borer and direct counting of the larvæ found. Light-trap experiments conducted by the Okayama Agricultural Experiment Station for twenty-four years (1909–32) showed only two years when the rice-borers were very abundant in both the first and second generations. The authors failed to find from this data any direct or definite relation between the population and weather. The death rate among larvæ and pupæ followed the same trend of change as the population density with a lag of one year and it seemed that the mortality was chiefly governed by the percentage of parasitised larvæ. However, it was not possible to find any definite relation between weather and the mortality and percentage of parasitised larvæ in their growth period.

In population studies differential effects of factors on host and parasite and also on the two sexes are important. In this connection some work (*Bull. Ent. Res.*, **26**, 1935, 315–22) by A. D. Hanna is interesting. He has found that the tissues of the testes of *Euchalcidia caryobori*, a parsite of the Bruchid *Caryoborus pallidus* which infests senna pods, are more sensitive to low temperatures than the tissues of the ovaries. After exposing the pupæ of both the male and female to a low temperature (16° C.) for 10 days, the females still laid the normal number of eggs, while 70 per cent. of the males were sterile.

J. C. L. Bertram (*J. Animal Ecol.*, **4**, 1935, 35–42) has compared the results of some experiments regarding the activity of adult insects at low temperatures in East Greenland with those made by R. Krogerus (*Acta Zool. Fennica*, **12**, 1932, 308) in Finland. It would appear that the East Greenland arthropods are better adapted for successful adult life under adverse temperature conditions than are those of Finland.

A useful paper by K. Mellanby (*Biol. Rev.*, **10**, 1935, 317–33) summarises and discusses the available information on the way in which climatic conditions and insect metabolism affect the rate at which water is evaporated from insects' bodies. It appears that practically all the water evaporated is lost from the tracheal system. At higher temperatures, however, the body wall of the

cockroach becomes permeable to water as J. A. Ramsey (*J. Exp. Biol.*, **12**, 1935, 373–83) has recently shown.

It is generally accepted that the gradually shortening days of late summer bring on the autumn brood of winged migrants among the two-host species of aphids in temperate zones. F. C. Baker (*Canad. Ent.*, **67**, 1935, 149–53) has studied the effect of length of day on resting treehole mosquito larvæ near Ithaca, N.Y. The length of day is a more stable factor in these insects' environment than temperature. He found that both wild and caged *Ædes triseriatus* suspended activities about the middle of September and that wild ones hatch from the eggs during the first week in April, although there is still freezing weather. These two dates are about equidistant from the shortest day of the year, and the hours of possible sunshine in the particular latitude are about 12·6 hours per day.

O. Park (*Ecology*, **16**, 1935, 152–63) has described a recording apparatus for measuring the nocturnal activity of insects and discussed further results of his investigations on this aspect of insect activity. This paper is followed by one by the same author and O. Sejba (*loc. cit.*, 164–72) on the nocturnal ecology of a mycetophagous erotylid beetle.

L. Berland (*Ann. Soc. Ent. France*, **104**, 1935, 73–96) has published the first results of his study of the insects of the air caught by means of nets attached to aeroplanes. He has captured Psocoptera, Coleoptera and Hymenoptera up to 1000 metres; Collembola and Thysanoptera up to 2000 metres; and Hemiptera Homoptera and Diptera up to 23,000 metres. All the insects recorded are small and weak or non-fliers. Berland suggests that they are carried upwards by ascending air-currents on account of their lack of resistance.

The biological control of weeds is more difficult to accomplish than the biological control of insect pests. E. Cameron (*J. Ecology*, **23**, 1935, 265–322) has made a complete study of the natural control of ragwort. In this paper two insects, the Cinnabar moth *Tyria jacobaeæ* and the Anthomyid fly *Pegohylemyia seneciella*, and some of their parasites receive particular attention. In addition, there is an interesting summary of weed control by insects throughout the world and also accounts of the biological control of weeds in New Zealand and the mass collection and shipment of weed-controlling insects.

J. G. Myers (*Trans. R. Ent. Soc. Lond.*, **83**, 1935, 11–22) gives some more instances of nesting associations of birds with social insects. Apparently these associations with Aculeate Hymeno-

ptera and Termites are more widespread than generally supposed and are much more definite and more regular. Among the conclusions reached is that the association can be explained on Müllerian and Batesian lines, all three partners receiving more protection than if nesting alone—the birds the most and the ants the least—while the bird nests form the recognition mark of the ensemble, *i.e.* the advertisement of danger.

Coleoptera.—W. A. McDougall (*Queensland Agric. J.*, **42**, 1934, 43–70) has investigated the reliability of larval length, antennal segment ratios, head width and the greatest width of the ventral mouth parts, as criteria for determining larval instars of *Lacon variabilis.* He found that any of its larval stages can be recognised by the greatest width of its ventral mouth parts. The application of Dyar's law is discussed. The measurements of the last seven instars are approximately in arithmetical progression. The first instar is easily distinguished by the shape of its ninth abdominal segment and the isolation of the ventral mouth part measurement. This knowledge was applied to the recognition of the instars of seven species of *Heteroderes* and nine other *Lacon* species. Here it was successful with the exception of the first instar which however was easily distinguishable. The growth rate for *L. variabilis* was found to be more rapid during the earlier stadia. That of other species was similar. Useful methods of technique are given and it is pointed out that the critical period of all the species under consideration is during the early instars.

R. W. Glaser and C. C. Farrell (*J. N.Y. Ent. Soc.*, **43**, 1935, 345–71) have been inoculating field plots and fields with *Neoaplectana*, a nematode parasite of the Japanese beetle, in an attempt to assess the practicability of using this nematode in controlling the beetle. The larvæ of this beetle become infected by way of the mouth with the second stage nemas. The parasite develops two or three generations within the body and destroys the larvæ by feeding on their tissues. It had previously been found possible to cultivate this nematode on an artificial medium. The first result of the inoculation experiments has been to demonstrate that it is possible to introduce the nematodes into the soil by burying them. Secondly, the nematode became permanently established and produced a high mortality.

The Bruchidæ live in the larval stage mostly in the seed of the Leguminosæ and are of great economic importance. Miss Gwyneth M. Herford (*Trans. Soc. Brit. Ent.*, **2**, 1935, 1–32) has constructed a key to those members of this family which are of

economic importance in Europe. Besides the actual keys there are descriptions. There is also a list showing the distribution and host plants.

LEPIDOPTERA.—J. S. Skoblo (*Bull. Ent. Res.*, **26**, 1935, 345–54) has investigated the effect of intermittent starvation upon the development of larvæ of the meadow moth (*Loxostege sticticalis*). One of the more interesting results is the fact that more than 50 per cent. of larvæ fed only two hours a day and starved for twenty-two hours accomplished their complete metamorphosis and produced moths that were fully capable of living. The nature of the food also influenced the length of development under conditions of intermittent starvation. For example, starving for eight hours a day when feeding larvæ on wormwood resulted in greater increase of development than starving for twelve hours a day when feeding on goosefoot. The length of development of each stage was shown to depend on the feeding during the previous stage.

HYMENOPTERA.—W. Pickles (*J. Animal Ecol.*, **4**, 1935, 22–31) has shown that the ant *Formica fusca* has a feeding territory of 236 sq. yards, while *Myrmica scabrinoides* has one of 140 sq. yards and *Acanthomyops niger* one of 35 sq. yards. The last named has the most populous nest, *F. fusca* the next and *M. scabrinoides* the least.

W. F. Reinig (*J. Genetics*, **30**, 1935, 321–56) has made a study of the colour variation of *Bombus lapidarius* and its cuckoo *Psithyrus rupestris* giving notes on the mimetic similarity. This similarity is concluded to have arisen by an orthogenetic process due to the common descent of host and cuckoo and not by natural selection. The orthogenetic process is influenced by exogenic conditions (climate and food).

In G. Salt's third experimental study in insect parasitism (*Proc. Roy. Soc.*, *B.*, **117**, 1935, 413–35) the question of host selection is considered. Ovipositing females of *Trichogramma evanescens* examined, selected and attacked, in addition to a number of true hosts, several unsuitable ones in which their progeny were unable to develop and also a variety of false hosts such as particles of sand in which they were unable even to lay their eggs. Two strains of this parasite reared exclusively on *Sitotroga cerealella* and *Ephestia kuehniella* respectively for 63 and 43 generations did not develop a preference for their respective hosts. *Trichogramma* uses definite and definable criteria for the selection of its hosts, the most important of which is size.

An important account of biological investigations on some Danish Hymenopterous egg parasites, especially in Homopterous

eggs, has been written by O. Bakkendorf (*Ent. Medd.*, **19**, 1934, 1–134). This includes taxonomic remarks and descriptions of new species.

DIPTERA.—The biology and morphology of the immature stages of the Mycetophilid *Macrocera anglica* have been described for the first time by S. Madwar (*Psyche*, **42**, 1935, 25–34). The larvæ of this fly resemble small earthworms and live under loose bark of oak, ash and elm. They live separately under a hygroscopic web and glide quickly over the surface of fungoid growth covering the bark, moving forward or backward with equal ease, occasionally they reverse their heads and glide on their sides. They are probably partly carnivorous and partly fungivorous. Thus the web-spinning Mycetophilids (Ceroplatinæ, Macrocerinæ and Platyurinæ) can be grouped together as the only flies in the family which are carnivorous. The larvæ are apneustic, the skin being thrown into numerous folds of which the integument is thin and richly supplied with a subcuticular net of tracheoles.

P. Bovien (*Vidensk. Medd. fra Dansk naturk. Foren.*, **99**, 1935, 33–43) has compared the larval stages of *Scatopse fuscipes* with those of *S. notata*. In discussing the respiratory system, Bovien comes to the conclusion that the rôle of the posterior spiracles with their system of arborescent tracheoles in the eleventh and twelfth segments is absolutely predominating. From a physiological point of view these larvæ may be considered as metapneustic.

M. Thomsen (*Proc. Zool. Soc. London*, 1935, 531–50) has described the morphology of the egg, larva and pupa of the blood-sucking Muscid *Hæmatobia stimulans* and compared the developmental stages with those of *Stomoxys calcitrans* and *Lyperosia irritans*. The cephalo-pharyngeal skeleton is very similar in the three species. These flies, belonging to the Stomoxydinæ, possess two asymmetrical mouth-hooks, the left one being much reduced in size and probably functionless. The dorsal epistomal sclerite is furnished with two pairs of sensillæ. The cephalo-pharyngeal skeletons of *Musca domestica* and *Calliphora erythrocephala* are also studied. The epistomal sclerite is again present in both these flies and even the smaller sclerites can be homologised with those of the Stomydinæ.

Mary E. Fuller (*Counc. Sci. and Ind. Res.* (*Australia*), Bull. 82, 1934, 62 pp.) has dealt fully with the inter-relationships of the insect inhabitants of carrion. There is a definite succession of inhabitants, each stage of decomposition being characterised by a particular group of insects. Competition between the insects is the main factor regulating the general blowfly population. Besides

this there is the influence of associated insects and that of climate. There is some evidence to show that competition is the main cause of succession. Miss Fuller besides discussing general biological problems also deals with economic ones.

G. Fraenkel (*Proc. Roy. Soc., B.*, **118**, 1935, 1–12) has proved that there is a hormone which induces pupation in *Calliphora erythrocephala*. It is secreted from 16 hours before pupation at 20° C. The hormone-producing organ is the ganglion or in its immediate neighbourhood. After the hormone has been discharged, pupation can be successfully accomplished without the co-operation of the ganglion. A further interesting result of this investigation is the fact that the atmospheric oxygen required for the darkening of the pupa is brought to the skin through the tracheal system. The results are compared with Kopec's work (1922–4) on *Lymentria dispar* and Wigglesworth's investigations (1934) on *Rhodnius prolixus*. Fraenkel comes to the conclusion that the mechanism of moulting and pupation in the three insect orders—Lepidoptera, Hemiptera and Diptera—includes a very similar principle.

Further papers on sheep blowfly research include the following: on a substance, cholesterol, that is essential for the growth of blowfly larvæ by R. P. Hobson (*Biochem. J.*, **29**, 1935, 1292–6 and 2023–6); on the growth of larvæ in association with bacteria by the same investigator (*loc. cit.*, 1286–91); on the effect of humidity on sheep maggots by W. M. Davies and R. P. Hobson (*Ann. Appl. Biol.*, **22**, 1935, 279–93); and one dealing with the substances which are attractive to sheep blowflies and essential to oviposition by R. P. Hobson (*loc. cit.*, 294–300).

Other Orders.—The presence of wing buds in the pupa of Aphaniptera has been demonstrated by M. Sharif (*Parasitology*, **27**, 1935, 461–4). This investigator has found wing buds on three species in two different families. He therefore presumes that the ancestors of fleas had on the mesothorax, as in the Diptera, functional organs of flight. This discovery throws light on the origin of the Aphaniptera which previously was considered the only order of insects with a complete metamorphosis that was totally devoid of wings.

V. B. Wigglesworth (*Proc. Roy. Soc., B.*, **118**, 1935, 397–419) has studied the anatomy of the tracheal system of the flea *Xenopsylla cheopis*. The closing mechanisms of the spiracles is described. When there is no muscular movement the third thoracic and eighth or last abdominal spiracles show a rhythmic opening and closing; the remaining spiracles are generally closed. During muscular activity the first and second thoracic open and the open-

ing of the third thoracic and eighth abdominal is prolonged. At the height of digestion all the spiracles of the abdomen may show rhythmic movements or remain permanently open. It is concluded that the spiracles are caused to open chiefly by oxygen want but that the duration of the open period is determined mainly by the time taken for carbon dioxide to diffuse away.

The bionomics and anatomy of the archaic but highly specialised stone-fly *Stenoperla prasina* have been investigated by G. A. H. Helson (*Trans. Roy. Soc. N.Z.*, **64**, 1934, 214–48). The chief differences from the generalised insect-type are the nymphal adaptations of the tibio-tarsal region of the legs correlated with the aquatic habitat and the presence of a gas in the crop and gizzard of the adults. As regards venation, it is concluded that M_3 is not present in Stone-flies.

AGRICULTURAL PHYSIOLOGY. By Arthur Walton, B.Sc., Ph.D., School of Agriculture, Cambridge.

The XV International Physiological Congress was held this year in August in Leningrad and Moscow. In a country where production is primarily for use and not for profit, where the phenomenon of " underconsumption and surplus production " is non-existent and where scientific application to agriculture is in its infancy but already outstripping in importance that of any other country, it is not surprising that many contributions to the Congress should be within the scope of applied agricultural science. It will therefore not be out of place in a review of recent advances to devote some space to these contributions. *The Summaries of Communications* is published by the State Publishing House for Biological and Medical Literature Moscow-Leningrad, 1935. M.M. Zawadowski (U.S.S.R.) contributes an important paper " On the Contradictory Interaction of the Organs of the Animal Body." It is shown that while the anterior lobe of the pituitary largely controls the development and activity of the gonads, and the gonads in turn control the development and activity of the secondary sexual organs, there is a corresponding inhibitory influence in the reverse direction ; the secondary sexual characters exerting an inhibitory influence on the gonads and these in turn inhibiting the pituitary. Thus it is shown that removal of secondary sexual organs (*e.g.* the cock's comb) increases the size of the testis and removal of the uterus of young female rats increases the weight of the ovaries and the corpora lutea are more fully developed. Conversely, injection, transplantation or feeding of prostate, seminal vesicles or comb to young rats inhibits

the development of the testis. Thus the writer visualises the system symbolically :

$$\begin{array}{ccccccc} & \uparrow\downarrow & + & \uparrow\downarrow & + & \uparrow\downarrow & \\ \leftrightarrows & \text{Hypophysis} & \rightleftarrows & \text{Sex gland} & \rightleftarrows & \text{Sexual characters} & \rightleftarrows \\ & \downarrow\uparrow & - & \downarrow\uparrow & - & \downarrow\uparrow & \end{array}$$

The arrows indicate interaction with some other system or organ. + indicates stimulation and – inhibition. This " Plus and Minus " hypothesis can, according to the writer, be extended to other developmental systems and to cover such general phenomena as the existence of a growth limit, compensatory hypertrophy, regulation, etc.

Hammond (England) and Mirskaia (U.S.S.R.) both contribute papers on " Œstrus and Ovulation in the Mare." The former gives the average duration of œstrus as 7 days but it varies within the limits of 3–41 days. The luteal phase, however, is much more constant in length. Variation in length of the whole cycle is therefore primarily due to rapid or slow follicular growth. Œstrus ceases 24 hours after ovulation. Cessation of heat can be brought about experimentally by rupturing the follicle per rectum ; if ruptured early in heat (2nd day) a corpus luteum is not formed and a new follicle develops and a new heat follows in a few days, if ruptured towards the end of heat (5th day) a corpus luteum is formed and a normal diœstrum succeeds. Mirskaia states that ovulation occurs 1–2 days, rarely 3 days, before the end of heat. During œstrus the vaginal smear consists only of cornified cells, the mucous membrane is hyperæmic and moist and the cervix relaxed. Pavlenko (U.S.S.R.) states that by use of gonadotropic extracts the normally monœstrous Black Fox can be made to breed twice a year. Hemmingsen and Krarup (Denmark) show that the normal day and night rhythm of sexual activity of the rat can be reversed by artificial light and induced darkness. The action of light on the reproductive system is also referred to in a monograph (see below) where it is claimed (Kabak, p. 84) that during winter, supplementary electric lighting increased the average daily milk yield of cows by $\frac{1}{2}$ to $\frac{3}{4}$ litres per diem. McCay, Cromwell and Maynard (U.S.A.) record that growth-retarded rats have a longer life than normal animals. They do not attain full body weight, but retain capacity for growth longer. Brooks (U.S.A.) concludes that sexual activity of the rabbit can be aroused by stimuli received over any one of several paths. The motor and sensory deficiencies produced by extirpation of the entire neo-cortex fail to abolish mating reactions, provided the olfactory apparatus is functional. Olfactory, visual and auditory

stimuli are not essential for the elicitation of a response from animals possessing at least the frontal half of the neo-cortex. On the other hand animals deprived of their olfactory lobes and all neo-cortex except the caudal half of one side (the region containing the visual cortex) mate normally but do not after visual stimuli have been eliminated by enucleation of the eyes. Barcroft and Barron (England) have found that respiratory reflex movements occur in the fœtus as early as the 39th day of pregnancy in the sheep. The cause of the first breath of the new-born lamb is oxygen want which produces a gasp similar to the dying gasp in cyanide poisoning, but which by drawing oxygen into the lungs initiates the free life of the lamb.

In addition to these communications to the Congress, the Physiological Laboratories of the Institute of Animal Husbandry presented a collection of short monographs by the heads of the various sections, indicating the nature of the work done in the laboratories and the progress made in recent years. E. F. Liskun outlines the organisation of the Institute in relation to the development of Animal Husbandry throughout the U.S.S.R. M. M. Zawadowski reviews various studies on the endocrinology of sex leading up to the "Plus and Minus" Theory mentioned above. A section is also devoted to problems of parasitology. B. M. Zawadowski contributes an article on "Some Problems of Zootechnical Endrocrinology." The diagnosis of pregnancy; the preparation of gonadotropic hormones; the preparation and use of "lysates" (*i.e.* decomposition products of histolysis which are claimed to have specific or general stimulatory functions when injected into the living animal); the use of insulin to increase growth rate of fattening pigs; the artificial production of moulting by thyroid injection into geese; these are some of many interesting applications of endocrinology to practical problems of agriculture which are discussed. O. F. Neuman describes the present position with regard to artificial insemination. Several millions of stock are now artificially inseminated mainly with a view to increasing the usefulness of valuable sires. A record of 2700 ewes inseminated with sperm from one ram during one breeding season has been established (the normal number of ewes allotted to a ram is about 50). The transport of semen to a distance is also receiving attention. Chukicheff, "On the Humoral Realisation of the Sympathetic Trophic," reviews the work of himself and collaborators upon the humoral stimulation of organs innervated by the sympathetic nervous system. Products of the acid hydrolysis of proteins (casein and fibrin) have apparently marked effects upon these organs. The clinical value of these products in both veterinary

and human medicine is stressed. G. I. Azimoff describes experiments on the use of alkaline extracts of anterior pituitary to increase lactation in the cow and sow. Finally there is a paper by N. D. Prianishnikov on the "Production of Feeding Stuffs from Straw." The removal of lignin in order to increase the digestibility of cellulose by various treatments is described.

All the monographs are in Russian with English translations. The book is well illustrated, well printed and well bound; a great advance on some previous publications.

ARCHÆOLOGY. By E. N. Fallaize, B.A.

The Place of Origin of Early Man.—The close association of prehistoric studies in Great Britain with the East Anglian terrain made it inevitable that early man should figure prominently in the programme of the Anthropological Section at the recent Norwich meeting of the British Association. Among a number of communications of no little interest and importance Sir Arthur Smith Woodward's Presidential Address to the Anthropological Section and the joint discussion between that section and the Geologists on the geological relations of early man in East Anglia stand out, each in its respective field, as marking a definite advance in the discussion of the problem of the origin and antiquity of man.

Sir Arthur Smith Woodward's address was a lucid and masterly survey from the point of view of geologist and palæontologist of the arguments which, in the light of recent research, point to Asia as the most probable place of origin of man. The argument in favour of an African origin based upon the existence of a warm fauna in Europe in the Pleistocene Age is, it was pointed out, no longer tenable. M. R. Vaufrey has shown recently that no land bridge existed between Africa and either Gibraltar, Malta or Sicily. In the light of the latest discoveries of fossil mammals in the caves of Palestine and Syria, showing a close connection between Asia and Africa, the similarities between the fossil fauna of Europe and Africa in the pleistocene is to be interpreted as due to a common source in Asia. The only direct communication by land with Europe was from Asia and it is by this route that the early races of man must have reached the western extremity of the continent, the claim for the early occurrence of "modern" man in Kenya being unsubstantiated.

In considering the types and distribution of the fossil apes, notwithstanding the discoveries now being made in South-East Africa, the evidence points to south central Asia as the most probable area

in which links joining up the various forms of ape and man are most likely to be found.

When Sir Arthur turned to the consideration of the early types of man he used a strong argument when he regarded Java man, Peking man, Piltdown man and Heidelberg man as variables of a common stock of approximately the same date, early pleistocene, which were peripherally distributed from a common centre in Asia; and equally cogent was the view that the recent discoveries of skeletal material in Palestinian caves, in which Neanderthal and other characters are conjoined, we appear to have "modern" man in the making.

In reference to Australia and America the significance was stressed of the Australian affinities which have been discerned in the Wadjak and Solo finds in Java and in Rhodesian man and other skulls in South Africa, as an earnest of further discovery which will support the Asiatic origin of man in Australia, and of the evidence of implements of a type similar to those from the Gobi desert, which have recently been found in pleistocene deposits in Alaska. The claim for the discovery of artefacts in association with a pleistocene fauna in the South-Western United States was accepted, but with a certain caution as to the views of American palæontologists.

EARLY MAN IN EAST ANGLIA.—In the discussion on the geological relations of early man in East Anglia, which was opened by Professor P. G. H. Boswell, it was evident that, in the light of a recent reclassification of certain glacial deposits of the area, there was general agreement as between geologists and archæologists that archæological finds fit quite naturally into the framework of the four glaciations, to which these deposits are now assigned. It was also clear, however, that there are a number of doubtful points upon which further agreement among archæologists themselves must be attained, before a full measure of co-operation can be afforded by the geologist.

The first point is the authenticity and cultural position of the various pre-Crag industries. In a communication which preceded the discussion, presented to the Anthropological Section sitting alone, Mr. J. Reid Moir had offered a quintuple classification of these industries, each differing in age, but all of pliocene dating. The opinion was expressed in the discussion that, while the best pieces from this horizon might be accepted by archæologists and geologists alike, much was of a dubious character, Mr. Hazzledine Warren still valiantly holding to his often expressed opinion that nothing belonging to this period was of human origin.

Some doubt has been thrown on the geological position of the Chellean industry by the extremely abraded condition of the imple-

ments found in East Anglia and the discovery by Mr. J. E. Sainty of an unabraded implement in the North Sea Till. This would appear to indicate a pre-glacial date and point to a very lengthy duration for the Chellean, since the Acheulean does not appear until after the second glaciation. On this view Mr. Reid Moir's Acheulean from the Upper Chalky Boulder Clay must be regarded as a sporadic intrusion due to later movement or penetration by frost cracks.

It was further made clear that much greater precision is demanded of the archæologist in the definition of industries. The archæological history of the third interglacial period in particular is a matter of doubt; and the hope was expressed that a detailed study would be made of the industry represented by Mr. Reid Moir's lower floor at Bolton's Pit, Ipswich, also known from Norfolk, the prevalent culture of the period appearing as an assemblage of mixed character, showing what might be a degenerate Levalloisian with traces of Upper Palæolithic. Probably it is peculiar to England (a report of the discussion will be found in *Nature*, Sept. 28, p. 502).

Early Man in America.—Towards the close of his Presidential Address, to which reference is made above, Sir Arthur Smith Woodward touched upon recent accessions to knowledge relating to early man in America, more particularly in connection with the discovery of stone implements of late Quaternary Age in Alaska, said to resemble Asiatic implements from the Gobi desert, and recent archæological and palæontological research in the South-Western United States. With the conclusions drawn from the latter he found himself in full agreement. There are, however, certain reservations, as he pointed out, which make it worth while to deal with the evidence a little more at length, especially as since his address was written, the results of further research have been published in America, which must be borne in mind in any discussion of the question as it now stands. Recent movement of opinion in the United States has been definitely to the side of admitting a much greater antiquity for the existence of man on that continent than was conceded under the view that he appears there only in post-glacial times and probably in an early neolithic phase of culture.

In this connection, however, it must be mentioned that Dr. Aleš Hrdlička, who has consistently shown himself an inflexible critic of the evidence which has been brought forward for the alleged quaternary age of human skeletal material hitherto found in America, now as the result of four years' excavation on a habitation site in Kodiak island, Alaska, has arrived at the conclusion that the earliest occupation—while loess was still being deposited on the glacial till —does not antedate two thousand years, the area not being habit-

able before that period; further that the physique and culture of the early people of Kodiak are identical with those of British Columbia and probably are ancestral to the Indians and the culture of the north-west coastal area. Details of the evidence for this view are not yet fully available.

Dr. Hrdlička in fact would concur in the view recently put forward by certain authors, E. Antefs and others, that man in America penetrated to the south-west by a corridor running down the east side of the Rockies in a post-glacial pluvial period. This sets aside the evidence of a skeleton recently discovered at Pelican Rapids, Minnesota, in pleistocene deposits, its position being attributed to a landslide. In this instance, however, it has to be admitted that other geological experts see no evidence of disturbance.

Discussion of the antiquity of man in America now centres around the character and antiquity of the so-called Folsom "point," a leaf-shaped stone implement of which the most marked characteristic is a double concave in section due to a longitudinal groove running on each face from base toward the tip. The distinctive and differentiating character of this implement was first recognised as the result of discoveries at Folsom, New Mexico, in 1927. The attention attracted by the circumstances of the discovery revealed that "points" similar to the Folsom point had been known for some time and were widely distributed, occurring from Canada to the Gulf of Mexico, and from the Rockies to the Atlantic. Further study, however, indicated the necessity for discrimination. Not all these points were of the Folsom type. The characteristic double groove was lacking in all but those derived from the "high-lands" of the south-west, lying to the east of the Rockies. This area then was regarded as the centre of the Folsom culture, held to be the earliest Stone-Age culture of man on the American continent. Professor E. B. Renaud of the University of Denver, who has made an extended study of the Folsom industry and of the archæological sites of the south-western states, holds that a variant of the Folsom point, the Yuma point, is an earlier form; but his data are derived from surface finds and his argument has not as yet been accepted as proved.

The existence of a Folsom *culture*, as such, has been something in the nature of an assumption; but its existence has now been proved by investigations made by Mr. F. H. H. Roberts, Jr., of the Bureau of American Ethnology, on the Lindenmeier site in Northern Colorado. Here Mr. Roberts found a habitation-factory site in undisturbed deposits, showing all stages of the manufacture of the Folsom point, with a variety of stone implements, the general character

of the complex pointing to the existence here of a primitive hunting people who, judging from the number and variety of the scrapers present, had made a free use of the skins of the animals they had hunted. A preliminary report on the results of the excavations carried out in October, 1934, appears in *Smithsonian Miscellaneous Collections*, Vol. 94, No. 4. Mr. Roberts resumed his investigations in the summer of 1935, and although no report is yet available, according to an official preliminary communication issued by the Smithsonian Institution, additional types of implements were discovered, an example of primitive graving art—the first known of its kind—indicating a use for the numerous gravers found, and a Folsom point embedded in the vertebra of an extinct form of bison. When the site was first discovered it was hoped that as the first discovery of the culture in stratified deposits, its position might be correlated with neighbouring gravels, thus affording indication of date; and accordingly the site during the past summer has been under investigation by a geologist. Beyond the statement that trenches have been dug in various directions no announcement has been made as to the result.

The elaborate technique and form of the Folsom point is not such as to suggest an implement of a primitive type. It has been compared with the European Solutrean type, to which indeed it bears some resemblance; but the attempts which have been made to find a developmental series in the stone implements of the South-West, parallel to that of the European archæological series, are not convincing. Any evidence which would corroborate the early date assigned to the Folsom industry is of capital importance. This, it has been claimed, was furnished by the discovery at Folsom in 1927 of a Folsom point embedded between the bones of an extinct form of bison, now preserved with its matrix entire in the Colorado Museum. This was widely, though not universally, accepted by palæontologists and archæologists at the time as evidence of high antiquity. Subsequently further investigations, and notably those of Mr. E. B. Howard, of the University of Pennsylvania Museum, in New Mexico in the caves of Guadalupe Mountains and in lake beds near Clovis in 1930 and succeeding years, have, it is claimed, demonstrated the contemporary existence of man and an extinct fauna, including the musk ox and two extinct forms of bison. (*See Museum Journal*, Philadelphia, **24**, Nos. 2–3, June 1935.) The occurrence of the musk-ox is taken to be conclusive evidence of a cold climate, which would point to the existence of man here at a time when the country further north was covered by an extension of the ice-cap, presumably the Wisconsin ice-sheet. Elsewhere in

the south-west, notably in Kansas and Nebraska, it is held, the occurrence of stone points with the bones of the mammoth as well as other extinct forms has been proved.

Such evidence both in character and volume might be thought to afford conclusive support to those archæologists in America who are now disposed to allow an antiquity to man on their continent of anything from ten to twenty thousand years. On the other hand not only does the question of accuracy of observation arise in a number of the instances—though many finds are above question in this respect—but it has to be remembered that conditions in America are peculiar. Quaternary palæontology there is obscure, and there is considerable difference of opinion among experts as to the dating and succession of the various types of extinct fauna. Some palæontologists would go so far as to maintain that while they admit the contemporaneity of man and extinct types, these types survived into comparatively recent times and cannot be taken as evidence of high antiquity—as they would, for example, in Europe. In the circumstances an impartial estimate of the weight of evidence must regard the question as still open in so far as the palæontological data are concerned.

In the meantime American archæologists, despairing of an immediate solution on palæontological lines, are turning to the evidence of climate in prehistoric times in the hope of finding a guiding line. This method has been applied by Mr. William D. Strong of the Bureau of American Ethnology to the calculation of the date of certain cultures which he investigated on a mound site at Signal Butte, Nebraska (*Smithsonian Miscellaneous Collections*, **94**, 10). This site is otherwise of importance as the only stratified site in North America as yet known, on which it is possible to trace the development of the stone implement and other elements of culture in stratigraphic succession. Here Mr. Strong found three phases of culture, which he attributed to periods of pluviation, separated by archæologically sterile deposits laid down, he concluded, when the mound was not occupied in a period of aridity. The lowest cultural stratum rested on sands which he attributed to the pleistocene. Correlating the cultural phases with the evidence of climatic variation in North America and basing his estimate on calculations which have been made from the observed drift of sand dunes, he concludes that the earliest occupation of the mound is to be dated at about 10,000 years ago at least. When the chronological and cultural relations of this earliest phase of the Signal Butte site to the earlier stone industries of other sites in Nebraska and the finds from Colorado, especially the Lindenmeier site examined by Mr. Roberts

can be more exactly defined, it should afford a valuable chronological datum line. It is, however, significant that Mr. Strong's estimate may have to be reduced by as much as 2000 years if the view of certain experts are accepted and it is agreed that the sands on which the earliest culture rests are not pleistocene, as held by Mr. Strong, but belong to the succeeding period. (A summary of the evidence from Colorado, New Mexico and Nebraska will be found in *Nature*, October 5, 1935, and the question is further discussed in *Discovery* for December.)

NOTES

The Halley Lecture, 1935 (R. W. W.)[1]

Just as the ring system of Saturn provided inspiration for the famous Nebular Theory of Laplace, so contemplation of the Andromeda and other spiral nebulæ was first responsible for the conception of our galaxy as a system discoidal in shape and rotating in its own plane. This model has persisted in its essentials ever since sidereal astronomy first became a serious study under the leadership of Sir William Herschel, but, while its general form has remained unaltered, its dimensions have shown pulsations comparable to those attributed to the Cepheid variables on which much of our knowledge is based. Herschel's estimates were necessarily rough, for he did not know the actual distance of a single star. His model of the galaxy was 6,000 light years in diameter, 850 times his estimate of the average distance of a first magnitude star. Statistical analysis of stellar distribution by Seeliger and Kapteyn increased the dimensions ninefold, but the shape of the model remained unaltered, and the sun was still placed at the centre. Shapley, by applying the period-luminosity law of the Cepheid variables to the determination of the distances of the globular clusters, found that the diameter of his model had increased to 260,000 light years, while the sun was 65,000 light years from the centre. This represented the maximum size of the model; further modification has been all in the opposite direction.

A correction to the zero point of the period luminosity curve caused Shapley in 1929 to reduce his scale by some 11 per cent., while about 1930 there commenced an examination into the effects of absorbing material in the galaxy, diminishing the light of the stars and thus making former estimates of their distances too large. An independent method had been provided by the recently demonstrated rotation of the galaxy, which enabled the distance of the sun from the centre to be determined dynamically. The two methods, entirely different in their principles and in the observa-

[1] *The Dimensions and Structure of the Galaxy.* By J. S. Plaskett, C.B.E., F.R.S. (Oxford: at the Clarendon Press, 1935. 2*s*. net.)

tions employed, show surprising agreement, and from their results the latest model of the galaxy has been constructed. Its main feature is a great flattened disc of stars, irregularly distributed in groups or clusters, but maintained in its general shape by rotation. Approximately circular in outline, its diameter is nearly 100,000 light years, at which limit the star density decreases rapidly. Diffuse absorbing material, equal in total mass to that of the stars, is concentrated towards the central plane. The globular clusters have a spherical distribution with an effective diameter equal to that of the disc of stars, and move in orbits of high eccentricity and inclination. The total mass is estimated as $16{\cdot}5 \times 10^{10}$ that of the sun.

Side by side with this decrease in the size of the model of the galaxy, estimates of the dimensions of the Andromeda Nebula have been growing, and its diameter is now accepted as at least two-thirds of that of our own system. Disparity in size is therefore no longer an argument against similarity in structure in addition to similarity in constitution, rotation, and surface brightness, and a spiral form for our galaxy, if not yet proved by observation, can be reasonably inferred by analogy. Our system is now regarded as an insignificant unit among a countless host of similar structures which make up the universe.

Food Investigation (E. J. S.)

The report of the Food Investigation Board for 1934 (H.M. Stationery Office, 4*s*. net) contains a number of items of general and scientific interest. Amongst these may be mentioned the successful experimental shipments of chilled beef from New Zealand stored in air enriched with carbon dioxide. This method enables the chilled beef to be stored from 60 to 70 days. Though a number of practical details regarding the application of the method remain to be worked out, its efficiency is no longer in doubt.

With regard to fruit storage, the value of iodised paper as a wrapping has been demonstrated. This method delays the development of fungi which cause rotting by considerably retarding the growth of the germ tubes. It can be employed, without impairing the flavour, for grapes and other fruits, but as yet the method is unsatisfactory with peaches and some varieties of plum.

The popular belief, that an apple when peeled is less wholesome than if eaten with the skin on, is well known. It has now been found that there is some scientific justification for this belief, since the concentration of vitamin C in an apple increases as the skin is approached and is six times as great in the peel as in the region of the core. In the variety "Bramley's Seedling" it has, moreover,

been found that the rosy apples had more than twice the vitamin potency of those with green skins. Canning appears not to impair the vitamin activity to any appreciable degree and successful results have been obtained in the addition of synthetic vitamin C to tinned products such as spinach and runner beans which do not naturally contain it.

Investigations on the storage of eggs have included a study of the yolk membrane, which has an average thickness of about 64/100000 in. The strength of this membrane, which is an important factor in relation both to keeping quality and cooking value, diminishes with keeping and breaks readily when it has fallen to about half its normal strength (·065 lb. per sq. inch). The alkalinity of the white of the egg rises with age, but this change can be checked and the reaction maintained at the same level by storage in air enriched with carbon dioxide.

There are a number of other items of interest in these pages including such a variety of topics as the effect of ozone in checking the ripening of green bananas through the volatile products of ripe bunches; the internal humidity of peapods, etc. It is a report that will well repay perusal.

A New Theory of Decay in Wood (A. G. N.)

The decay of wood and timber has long been ascribed to microbiological attack, and the many processes of wood preservation have been designed to inhibit the growth of organisms. Recently, however, a new theory has been put forward by Rudge,[1] who claims that decay under many conditions is not biological, but purely chemical, and follows as a result of the infiltration of certain inorganic salts. He has investigated samples of decayed wood from piles driven into waterlogged soils, fence posts, telegraph posts, timber shuttering from concrete work, joists and woodwork in contact with plaster, and has shown that in the decayed areas the content of calcium and aluminium is much greater than in sound wood. He has further put forward the view that calcium in the form of calcium bicarbonate is the destructive agent, possibly combining with cellulose to give a carbonate compound similar in type to cellulose xanthate. This cellulose-carbonate is then presumed to be readily oxidisable and to break down to acids probably of the uronic type. Although ingenious, this theory cannot yet be accepted as proven, since no satisfactory evidence of a genuine decay under conditions that would rule out microbiological activity has been produced. Rudge holds that under waterlogged conditions biological action may be

[1] *J.S.C.I.*, 1933, **52**, 284T, 447T; 1934, **53**, 37T, 125T.

deemed to be of a secondary character, negligible or absent entirely, a view that no bacteriologist would accept. In any case, the autoxidation of the hypothetical cellulose-carbonate could hardly proceed in waterlogged soil, in which conditions are reducing not oxidising. The infiltration of salts into wood is to be expected under many circumstances of timber usage, but may be entirely independent of decay. The presence of calcium may even assist microbial degeneration by neutralising acidic products of fermentation.

In an endeavour to show that calcium bicarbonate may effect changes in the composition of wood, experiments were carried out in which moist wood was heated at 65° C. with calcium carbonate in an atmosphere of carbon dioxide for periods up to three weeks.[1] Analyses showed certain obscure changes, but nothing typical of decay that could not also be explained by the prolonged action of water at that temperature. Until direct evidence is produced of the decaying action of inorganic salts on wood at ordinary temperatures and under conditions that preclude the concomitant growth of micro-organisms, the " calcium infiltration " hypothesis cannot be put beside the well-established biological theories of wood decay.

Annual Report of the Calcutta School of Tropical Medicine and the Carmichael Hospital for Tropical Diseases for 1934 (P. J.)[2]

The account of a year's work carried out in this Institute continues the story and shows the same standard of excellence expressed in the report of the previous year. Cholera research is continued by further study of vibrios and types of choleraphage and vibriophage, of which there are now 12 and 15 types respectively. At present the work indicates that the variability in the vibrio is intimately connected with the behaviour of the bacteriophage. Attempts to obtain cholera toxin have yielded encouraging results ; when the nature of this toxin is definitely established and an antitoxin has been prepared we shall possess the only specific treatment of the disease. Bacteriophage damages the causative organism but cannot neutralise the poisons elaborated by the vibrios. Cholera antitoxin combined with bacteriophage " would constitute the ideal method of treatment of cholera." It seems that the value of bacteriophage alone, if any, is very limited in the treatment of the disease. During the year 400 strains of vibrios from different sources have been studied. The hæmolytic property of a strain is extremely variable

[1] *J.S.C.I.*, 1935, **54**, 302T.
[2] Bengal Government Press, Alipore, Bengal, 1935.

and disappears under laboratory conditions. There is no correlation between hæmolytic property and toxicity.

Further studies were made of the bionomics of *Phlebotomus argentipes*; infected sand flies were fed on hamsters and confirmed the evidence that *Leishmannia Donovani* is transmitted by this species of fly. The breeding sites of these insects vary with the season; the positive findings in samples of soil (2 lb. biscuit tinfuls) are less than 50 per cent., and entail much labour. Antilarval measures include clearing of undergrowth from the vicinity of houses, repair to walls of sheds and living-rooms, and spraying breeding sites with a mixture of equal parts of crude oil and kerosene; but the results are not very satisfactory because probably there are other as yet unknown breeding sites. Attempts were made to determine the duration of life of these midges in nature; numbers of flies were marked either by dusting with fine powdered fluorescin or by puncturing one of the wing membranes and liberating them in various living-rooms and sheds. The results were not very profitable as so many were preyed on by lizards and spiders. The mechanism of infection is still to be explained; attempts to do this were made on dogs in which resistance was lowered by heavy hook-worm infection, and also on mice infected with *B. Gaertner enteriditis*, but so far with negative results.

Especial research has been directed to "Epidemic Dropsy," a disease responsible for much incapacity and heavy death rate in Bengal and Bihar during the last two or three decades. Preliminary bacteriological tests suggested that the disease is due to intoxication with Gram positive spore-bearing anaerobic bacilli which were isolated in large numbers from the stools of patients and also from parboiled and badly stored rice; and it was thought that infection was due to the eating of such infected rice. Blood cultures and urine were always sterile; many hundreds of organisms were isolated from fresh and from heated specimens from stools, after aerobic and anaerobic culture. In four cases homologous agglutination was observed with Gram positive spore-bearing bacilli isolated from stools. Reluctance was felt in placing confidence in these agglutination results lest a deposition or sedimentation of the spores or of the organisms be interpreted as agglutination.

The epidemic has not subsided in spite of the use of newly harvested and recently husked rice. Examination of this fresh rice showed that it was as badly infected as the stored rice. The epidemic is said to be imported from a neighbouring village by some one suffering from severe diarrhœa and swelling of the feet. The investigation is proceeding.

Space does not allow detailed reference to the many interesting problems with which this research institute is concerned. For workers in tropical disease this report is full of great interest and essential information. We congratulate the directors and staff on the excellence of the year's work.

Miscellanea

H.M. the King has approved the award of Royal Medals by the President and Council of the Royal Society to Prof. C. G. Darwin for his researches in mathematical physics and to Dr. Harker for his work in petrology. The President and Council have also made the following awards: Copley Medal to Prof. C. T. R. Wilson; Davy Medal to Prof. A. Harden and Hughes Medal to Dr. C. J. Davisson.

The Nobel prize for Physics for 1935 has been awarded to Prof. J. Chadwick and the Chemistry prize to M. and Mme. Joliot-Curie.

Sir Josiah Stamp has been elected President of the British Association for the year 1936. Mr. F. T. Brooks and Dr. A. H. Ferguson have been appointed General Secretaries in succession to Prof. P. G. H. Boswell (who becomes Treasurer) and Prof. F. J. M. Stratton (retired).

Prof. D'Arcy Wentworth Thompson has been elected President of the Royal Society of Edinburgh.

The Lord President of the Council of the Department of Scientific and Industrial Research has appointed Dr. W. H. Mills, Sc.D.,F.R.S., the Rt. Hon. Lord Riverdale, K.B.E., Prof. A. Robertson, D.Sc., and Mr. H. B. Shackleton to be members of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research. Dr. E. J. Butler, C.M.G., C.I.E., D.Sc., M.B., F.R.S., Sir Kenneth Lee, LL.D., and Prof. N. V. Sidgwick, C.B.E., Sc.D., F.R.S., have retired from the Council on the completion of their terms of office.

The Earl of Donoughmore has been appointed Chairman of the new National Radium Commission. The Joint Secretaries are Dr. G. W. C. Kaye and Mr. G. F. Stebbing.

Dr. E. Maitland Wright has been appointed to succeed the late Prof. H. M. Macdonald as Professor of Mathematics in the University of Aberdeen.

We have noted with regret the announcement of the death of the following workers in science during the past quarter: Prof.

Carl Barus, of Brown University, U.S.A., physicist; Prof. Henry Briggs, Head of the Department of Mining, Heriot Watt College; Prof. Max Cremer, physiologist, of Berlin; Dr. H. W. Dudley, F.R.S., of the National Institute of Medical Research, biochemist; Dr. Rhoda Erdmann, of Berlin, cytologist; Mr. E. G. Hooper, chemist; Dr. Walter Hough, of the U.S. National Museum; Dr. C. F. Marbut, chief of the U.S.A. Soil Survey; Sir John McLennan, K.B.E., physicist; Dr. F. C. Shrubsall, sometime Secretary and President of Section H of the British Association; Mr. A. Thorburn, ornithologist; Mr. E. Thurston, C.I.E., anthropologist.

The International Committee of Weights and Measures at a meeting in Paris resolved that the present "International" system of electrical units shall be replaced by the "Absolute" (practical) system on and after January 1, 1940. The change will affect precision measurements only since, at the most, it does not exceed 1 part in 2,000. It is hoped that the ratio between the units in the two systems will have been determined to within 1 or 2 parts in 100,000 before the change is made.

In a letter to *Nature* (October 26), Kellström states that he has made a new determination of the viscosity of air with a view to checking the reality of the difference between the value of e as found by Millikan using the oil-drop method and the "X-ray value." He finds the viscosity (η) at 23° C. to be $(1834{\cdot}8 \pm 3{\cdot}0) \times 10^{-7}$ while Millikan obtained $(1822{\cdot}6 \pm 1{\cdot}2) \times 10^{-7}$. The calculation of the value of e involves $\eta^{3/2}$ so that Kellström's result raises Millikan's value $e = 4{\cdot}770 \times 10^{-10}$ e.s.u. to $4{\cdot}818 \pm 0{\cdot}012$ in agreement with the X-ray value. A dull ending indeed to an intriguing problem if the new result for η is established.

Bulletin No. 2 of the International Tin Research and Development Council (Free, Manfield House, 378, Strand, W.C.2) contains much interesting information concerning the use of solder. It appears that modern plumbers' solder has practically the same composition (1 part tin, 2 parts lead) as the alloy "tertiarium" described by Pliny and used for soldering lead work in ancient Rome. Another alloy, "argentarium," described by Pliny, containing equal parts of tin and lead, is still widely used for many purposes. In 1933 no less than 18,000 tons of tin were used for the manufacture of solder, of which 5,000 tons were used by the motor-car industry and 4,500 tons in the canning and box-making industries. A car radiator of average size may consume seven or eight pounds of solder and

solder is often used to build up details of the contour of a stream-lined body.

The *Bulletin* describes many soldering operations, both in the small workshop and in the large-scale plant. One, in particular, may be mentioned here,—the soldering of glass or porcelain on to metal. To effect this a glaze containing a high content of platinum and gold is fired on to the surface, *e.g.* of the glass, in an oxidising atmosphere. The metal part, tinned with tinman's solder, is then fitted to the glazed surface and the whole is fired again at a temperature rather below the melting point of tin.

Imperfect hearing produced by defects in the middle ear may often be assisted best by transmitting the "sound" through the bones of the skull from a bone-conduction receiver connected to an amplifier type of audiphone. A receiver of this type, developed by the Western Electric Co., is described in the *Bell Laboratories Record* for August 1935. It consists of a *relatively* heavy electro-magnet and an armature to which is riveted a light phenol-plastic diaphragm curved to fit against the mastoid bone. The whole receiver weighs only half an ounce, the overall dimensions being $1\frac{3}{4}'' \times \frac{3}{4}'' \times \frac{5}{8}''$. This type of receiver has the advantage that it does not impede sound entering the ear by the natural route and, being quite noiseless, is peculiarly suitable as an aid to hearing in an auditorium where the faint clicking in an air-conduction receiver may be a source of irritation to near-by listeners.

A brief account of the Empire Film Library is given in the *Bulletin of the Imperial Institute* for July last. (John Murray, 3*s*. 6*d*.) The library has been organised in commemoration of the Silver Jubilee and is based on the films originally included in the Empire Marketing Board Library. It will include a large number of films sponsored by the Government of the Dominion of Canada which has been the pioneer in recognising the vital importance of films to a fuller understanding of present-day life in the Empire. The films are lent to schools and other approved bodies without other charge than the cost of carriage to and from the Institute. It is hoped that the Library will "become a centre for the collection of films which will reveal the life, scenery and industries of the overseas Empire to the general public and particularly to the rising generation in the British Isles."

The Association of Special Libraries and Information Bureaux (A.S.L.I.B.) has commenced publication of a quarterly book-list

containing recommendations of recently published scientific and technical books for the use of public and other libraries. The Editing Committee has secured the help of individuals and specialist organisations who are, for their own purposes, making as complete a survey as possible of the literature of their own branches of science and technology written in the English language, and who are able to assess the relative merits of new publications and thus select those which should be brought to the attention of librarians. The books are assembled under broad subject headings and, within these headings, are further sub-divided as (*a*) books suitable for general readers, (*b*) books suitable as text-books, (*c*) books of an advanced or specially technical character, (*d*) Encyclopædias, Year Books, Handbooks, etc.

The first issue, dated October 1935, comprises some 200 odd books, but the subject headings do not include such important subjects as astronomy, biochemistry, geography, history of science, mathematics and physiology. We can, however, join the editors in the hope that the range of subjects will gradually be extended, for there can be no doubt as to the usefulness of a select list of this kind. The price to non-members of the Association is rather high—10*s*. 6*d*. per annum. The address is 16, Russell Square, W.C.1.

From time to time the National Physical Laboratory collects together papers describing the most important researches that it has carried out, and reprints them under the title of *Collected Researches*. In the earlier days of the Laboratory each volume contained papers from several Departments, but about ten years ago it was decided to publish the researches of one department at a time. Volumes dealing with Physics, Electricity and Radio, Optics, and Naval Architecture have already been published, and a volume of over 400 pages, containing the more important papers emanating from the Metallurgy Department in recent years, has just appeared. (*National Physical Laboratory Collected Researches*, Vol. 25. H.M. Stationery Office, 25*s*. net.)

The equilibrium diagram of an alloy system represents the phases which exist in the system at any composition and any temperature (provided the alloy is in equilibrium). It also indicates the phases which are likely to be obtained on quenching any alloy in the system from any temperature. Experience has also shown that, in a rough degree, certain phases are associated with certain mechanical properties. A knowledge of the equilibrium diagram is therefore fundamental to a study of the age-hardening properties of alloys and, in a lesser degree, to a study of their general

mechanical properties. In addition, it is the starting point for much of the recent theoretical work on the inner structure of alloys. It is thus not surprising to find that, out of the twenty-three papers collected in this volume, nine deal directly with the constitution of alloy systems, in five of which iron is the principal metal. Another refers to the technique necessary in determining the constitution of alloys containing mercury, which have (when they contain a considerable excess of mercury) to be polished, etched and examined under the microscope at temperatures low enough to keep them solid.

In a large number of cases the metals obtainable commercially are not sufficiently pure for the accurate determination of an equilibrium diagram; consequently one paper deals with the effect of an impurity (nitrogen) on chromium and its alloys, while others describe the methods adopted to obtain pure metals. These methods differ in different cases; thus pure silicon has been made by subjecting the commercial metal to chemical treatment, and pure manganese by distilling the commercial variety at about 1,300° C. *in vacuo*. To obtain pure chromium, however, it was necessary to employ a carefully controlled electrolysis of an aqueous solution of chromic acid.

Another point of very great importance in the study of metallic equilibria, especially when it is necessary to use high temperatures, is the choice of suitable refractories for making crucibles, thermocouple protection tubing, etc., and one paper in this volume discusses special refractories for metallurgical research.

The comparatively recently developed application of X-rays to the investigation of alloy systems has not been neglected, and while references to its employment are made in many of the papers referred to above, there are also three contributions dealing specially with this subject.

Of the remaining papers, one deals with the optical determinations of high metallurgical temperatures, describing a new determination of the melting point of iron, one with the properties of metallic cadmium, and another with the surface tension of liquid metals.

ESSAY REVIEWS

NEW PATHWAYS IN SCIENCE. By H. SPENCER JONES, M.A., Sc.D., F.R.S., Astronomer Royal. Being a Review of **New Pathways in Science,** by SIR ARTHUR EDDINGTON, M.A., D.Sc., LL.D., F.R.S. [Pp. x + 333, with 4 plates.] (Cambridge: at the University Press, 1935. 10*s*. 6*d*. net.)

SIR ARTHUR EDDINGTON occupies a unique position as an interpreter of modern physical science to the layman. The reason is not far to seek. He possesses the rare combination of wide and varied knowledge—he has made important contributions to astronomy, relativity theory, quantum theory and wave mechanics—profound vision and philosophical insight and the ability to interpret for the layman with such charm and grace of style and with such apt and striking illustrations that the layman is almost persuaded that he understands. It is, indeed, fortunate that, at a time when new pathways in science are being opened up so rapidly, one of the pioneers should have been willing to find opportunity to explain what it all means, for the benefit of those to whom tensors, matrices, wave operators and non-commutative algebra must for ever remain a sealed book. It is undoubted that *The Nature of the Physical World* has had a profound influence on our common thought. The appearance, after an interval of seven years, of a further book that brings the story up to date is an event of some importance.

Being in the nature of an addendum to Sir Arthur's earlier writings, this book may appear at first sight somewhat disconnected. It is based upon the Messenger Lectures, delivered by the author at Cornell University in April and May, 1934. These were concerned with a variety of topics that had claimed Sir Arthur's attentions during the preceding six years. Thus we have a chapter on the philosophical basis of science; a chapter on the chief results of atomic physics; a chapter on entropy and time's arrow; two chapters on indeterminism; four chapters on various astronomical questions; two chapters dealing with probability and the theory of groups; a chapter on the fundamental constants of nature. Another chapter is devoted to the discussion of some of the criticisms of Sir Arthur's writings and philosophical outlook by various

reviewers and critics. In this volume, therefore, no attempt has been made to provide a systematic introduction to modern scientific thought. Those readers who do not possess an adequate scientific background would be well advised to defer reading this book until after they have read *The Nature of the Physical World.*

Yet, despite the apparently disconnected nature of the subjects, there is a general continuity of treatment and of thought. How is the scientific picture of the world described in physics related to the "familiar story" in our minds? What kind of information about the Universe does science really give us? It is emphasised that there is a fluidity of representation in that science can only give us information about structure, but can tell us nothing about the nature of what it is that possesses the structure. "The mode of interlocking of the operations, not their nature, is responsible for those manifestations of the external universe which ultimately reach our senses."

This viewpoint is developed in the first chapter dealing with the relationship between science and experience. Our sensory experience is likened to a cryptogram and the scientist to a Baconian enthusiast engaged in deciphering the cryptogram. His task is to discover the scheme revealed by the regularities and recurrences in his sensory experience; for this purpose much of his sense data proves to be redundant. So a mutilated observer is imagined, deprived of sense, smell, hearing and touch; also with one eye and all of the retina of the other except one small patch (which is colour blind) removed. Such an observer can recognise neither form nor extension in the external world; he can see in only one direction at a time; he can distinguish only light and darkness. But he can observe coincidences and so all our knowledge of the external world, as conceived by modern physics, can be demonstrated to him. When the cryptogram has been deciphered by such an observer it is couched in an unknown language. "It does not, however, follow that it is unintelligible to the mind. Perception is only part of our mental outfit, and the language of perceiving is only part of the language of knowing. Our reading of the cipher of experience leads to an understanding of our environment, highly abstract indeed and only to be apprehended by the intellect through symbolic expression, but nevertheless satisfying to the urge of the human spirit in its quest for knowledge."

Our general knowledge of atoms, radiation and æther is next dealt with. Starting first with the working conceptions of the experimental physicist, we consider the elementary particles of which matter is built up, protons, electrons, positrons and neutrons; the Bohr model of the atom, with satellite electrons describing orbits

around the nucleus and the electrons jumping from one orbit to another; and the interaction between atoms and radiation. The newer conceptions of wave mechanics are then introduced; each electron is now distributed as a sort of probability haze all over the atom. Wave mechanics examines the laws of propagation of waves through this haze; where the densest part of the haze is situated is where the electron is most likely to be. Wave mechanics has proved more successful than the older quantum theory because it attempts less. It does not predict precisely where an electron will be at a future instant but it does tell us as much about its future position as is actually involved in the recurrencies of our sensory experience. "Thus in saying that wave mechanics corresponds to a profounder level of conception I do not mean that it takes us closer to the objective world behind the phenomenon. I mean that it reveals more fully the source of the regularities in our experience, which are conditioned as much by our mode of acquaintance with the objective world as by the constitution of that world."

Sir Arthur retains the term "æther" to denote something that is everywhere, permeating the interstices of the atoms and extending as far as and beyond the remotest stars. But he uses it in a sense quite different from the sense in which it was used in pre-relativity days. If this distinction is borne in mind, it seems to me that there is no harm in retaining the word. He remarks that "Some distinguished physicists maintain that modern theories no longer require an æther—that the æther has been abolished. I think all they mean is that, since we never have to do with space and æther separately, we can make one word serve for both; and the word they prefer is 'space.' I suppose they consider that the word æther is still liable to convey the idea of something material. But equally the word space is liable to convey the idea of complete negation. At all events they agree with us in employing an army of mathematical symbols to describe what is going on at any point where the æther is—or, according to them, isn't. 'Wheresoever the carcase is, there will the eagles be gathered together,' and where the symbols of the mathematical physicist flock, there presumably is some prey for them to settle on, which the plain man at least will prefer to call by a name suggestive of something more than passive emptiness." He offers as a compromise the term "field," which includes both the electromagnetic field and the gravitational or metrical field. This term seems to me preferable, because of the difficulty of divorcing the term æther from the old conception of a material jelly.

The chapter entitled "The End of the World" is largely concerned with the question of "time's arrow." "Is there everywhere

and everywhen in the physical universe a signpost with one arm marked 'To the Future' and the other arm 'To the Past'?" Sir Arthur admits that for ordinary purposes the signpost is detected by consciousness but, setting aside the guidance of consciousness, he seeks to discover the signpost in the physical world itself. His answer is that if we take an isolated system and measure its entropy at two instants, the instant which corresponds to the greater entropy is the later. This seems to me to be arguing in a circle; that the entropy of an isolated system increases with the time is a law that is based upon experience; numerous experiments have shown that the entropy is always greater at the later instant. But how do we determine the later instant? Surely by our consciousness. Our experience is relative to the background of the existing universe. According to the relativistic formulation of thermodynamical laws by Tolman, it would be possible in a pulsating universe, during a period of contraction, to have decrease of entropy with time and then time's arrow by Eddington's criterion would point in the wrong direction. But Sir Arthur will have nothing to do with a cyclic universe that is continually running down and then rejuvenating itself. To him this idea is wholly retrograde. "Must Sisyphus for ever roll his stone up the hill only for it to roll down again every time it approaches the top? That was a description of Hell." So his picture of the end of the Universe is what is commonly described as the heat-death of the Universe—the final state when disorganisation has reached its maximum value. "When the final heat-death overtakes the universe, time will *extend* on and on, presumably to infinity, but there will be no definable sense in which it can be said to *go on*." Consciousness must have disappeared before this stage is reached and there is no longer any increase in entropy possible, to serve as an arrow for time. Going backwards in the other direction we find more and more organisation until we get back to a time when there was the maximum possible organisation. This we must term the beginning of the world, because to go back further is impossible. Sir Arthur admits that this conception is not entirely satisfactory but he can see no escape from the dilemma.

Indeterminism looms large in modern physics and there has been much controversy as to whether this has any effect on the law of causality in nature. Sir Arthur devotes two chapters to the consideration of this matter. If we suppose that we know all that can be known about an electron at the present instant, then under the most favourable circumstances we cannot predict its position one second later closer than within about 1½ inches. Whenever we deal with the minutest constituents of matter, we find this indeterminacy

coming in. The more accurately we fix the position of an electron, the less accurately we can fix its velocity and conversely; this combination of uncertainty is embodied in the present picture of an electron as a packet of waves. The classical laws of physics are statistical laws; when we consider the average of a large number of individuals, the probability becomes so high that we approach absolute certainty. And so these statistical laws came to be mistaken for causal laws. Sir Arthur quotes from Laplace:

"We ought then to regard the present state of the universe as the effect of its antecedent state and the cause of the state that is to follow. An intelligence who for a given instant should be acquainted with all the forces by which Nature is animated and with the several positions of the entities composing it, if further his intellect were vast enough to submit those data to analysis, would include in one and the same formula the movements of the largest bodies in the universe and those of the lightest atom. Nothing would be uncertain for him; the future as well as the past would be present to his eyes."

As Sir Arthur points out, Laplace makes the proviso that the super-intelligence must be acquainted with all the conditions prevailing *at a given instant*. If this is intended to include all that might be inferred by retrospective inference from what actually will happen in the future, he is using the future to predict the future. Moreover he is imagined to include the movements of all bodies in "one and the same formula." "But to include them in a formula is not necessarily the same as to know them. An algebraic symbol may stand for a known or an unknown quantity. So when we have a formula which professes to give exactly the future position of an object, the question arises whether it is given in terms of known or of unknown symbols. Heisenberg's principle tells us that *just half of the symbols represent knowable quantities and the other half represent unknowable quantities*. The unknowable quantities correspond to retrospective characters. By inverting such characters we make the future appear determinate; but they do not actually predetermine the future because they are themselves indeterminate until the future events have taken place."

The chapter on probability emphasises that probability is always relative to knowledge, actual or presumed. This is too frequently overlooked, particularly in questions involving inverse probability. Probability plays so important a rôle in modern physics that it is well that the meaning of the term should be discussed and our minds freed from preconceived notions. The distinction between wave mechanics and classical mechanics is very well summarised in this passage:

"Broadly speaking wave mechanics pictures a universe whose substance is probability, whereas classical mechanics pictures a universe whose substance is mass, energy, momentum, electric and magnetic force, etc. In wave mechanics we examine the way the probability moves about and redistributes itself; in ordinary mechanics we find the way mass, momentum and electromagnetic field move or are propagated. In the former the waves, which give the subject its name, are waves of probability; in the latter we treat sound waves, electromagnetic waves and gravitational waves. For brevity these may be contrasted as a universe of probability and a universe of entities. They are, however, both aspects of the same universe whose description involves both probabilities and entities. The difference in point of view is that in the first we attach entities (electrons, protons, photons) to the probabilities which we study—only from the nature of the entities treated in classical physics the attached probabilities are all practical certainties. In macroscopic physics the variety lies in the entities—greater or lesser masses, greater or lesser field strength—the probabilities being all similar units; in microscopic physics the position is inverted and the variety lies in the probabilities, the entities generally being all similar units, *e.g.* electrons. It is therefore found to be more businesslike and practical to contemplate a distribution of probabilities; and the entities attached to them tend rather to drop out of sight in our calculations and deductions."

The four chapters devoted to astronomical subjects give a very clear account of those fields of investigation with which Sir Arthur has himself been closely identified. The chapter on the constitution of the stars is the best summary of the subject that I have seen. Most of the work that is here summarised is now several years old. The one important recent contribution is the recognition that hydrogen is very abundant in the stars; most of the stars appear to contain about one-third part by weight of hydrogen. It is pointed out that this result, though apparently well-established, is subject to the reservation that the stars do not contain a significant amount of neutron. If this were the case, the material of the star would be a very good conductor of heat. The high hydrogen content of the stars has been accepted because in no other way does it seem possible to dam back the ætherial heat; this conclusion is necessarily invalidated if there is a sufficient neutron content to allow the material heat to leak out. The possible saving circumstance is that any neutrons evolved in the star are likely quickly to enter atomic nuclei and therefore to have a very short free existence.

The chapter on subatomic energy discusses the question of the

source of the energy that the stars radiate. Two possible sources have been pictured—the mutual annihilation of a proton and an electron, leaving merely a pulse of energy, and the building up of more complex atoms out of the hydrogen atoms. The second process can at the most only furnish about one per cent. of the energy that can be furnished by the first process. We may with some confidence assert that the second process must occur; the complex atoms have been formed somehow or other and what mode of formation is more likely than by building up from the simplest atoms and in what place is it more likely that this can occur than in the interior of a star? The laboratory experiments on artificial transmutation are not in opposition to this assertion. We have no direct evidence that the first process either does or can occur. *A priori*, we should say that the temperature within a star is far too low—though of the order of 20 million degrees—for it to occur. Accordingly as we do, or do not, suppose annihilation of matter to occur within a star, we shall get two widely differing estimates as to the possible lifetime of a star. Several lines of evidence have seemed to point to the long time-scale that annihilation of matter makes possible; other evidence, and in particular the observed rate of expansion of the universe, appears to be irreconcilable with this time-scale. It is a matter of the first importance for theories of stellar evolution to decide between the two alternative time-scales. The balance of opinion in astronomical circles is tending towards the shorter time-scale and the abandonment of the annihilation hypothesis. This is clearly Sir Arthur's view: "I think the time has come to consider whether the hypothesis of annihilation of electrons and protons might not be allowed to lapse." He points out that the positron and not the proton is the true enantiomorph of the electron and that probably the combination of a proton and an electron in a neutron is the nearest they can go to cancelling one another. His final conclusion is that "the present moment, when there is a rush of new discovery only half digested, is not the best time for making up our minds whether the hypothesis of annihilation is worth preserving. It will be apparent from many passages in this book that I have not yet taken the step of retiring it from my own thoughts. It is doubtless best to leave the question in abeyance for a year or two longer, but it has seemed well to call attention to its imminence."

The chapter on cosmic clouds and nebulæ will interest the physicist in that it emphasises some of the advantages that the astronomer possesses over the laboratory investigator. The density at the centre of a typical nebula, such as the Great Nebula in Orion,

is about one-millionth of the density in the highest vacuum that can be created in the laboratory. The density of the interstellar matter is only about one ten-thousandth of that of the matter in the Orion nebula. So the astronomer can study the so-called "forbidden lines" in atomic spectra, which can never be obtained in the laboratory. How such an extremely low density of interstellar matter can produce strong absorption lines in certain stellar spectra and why these lines are due only to calcium and sodium is explained.

The recession of the spiral nebulæ is a well-established observatorial fact. It is this fact that forms the foundation of the conception of the expansion of space. But the conception involves more than the mere expansion of a material system. It involves a closing up of space, so that space has a finite volume ; this finite, but unbounded, space is supposed to be expanding. The discussion of the expansion of space is intimately bound up with the "cosmical constant," generally denoted by λ. It may be recalled that λ was introduced into the generalised relativity theory by Einstein in order to satisfy the boundary conditions in his static universe. It was, therefore, a purely arbitrary constant. Later it was found that a static universe was necessarily in unstable equilibrium and so non-static solutions of the equations of relativity theory had to be investigated. It has been found that there are mathematically possible universes corresponding to positive, negative or zero values of λ ; the various solutions can be grouped into three classes, which were termed by de Sitter the expanding universe of the first type, the expanding universe of the second type and the oscillating universe. From the purely mathematical point of view, therefore, λ is an arbitrary, indeterminate constant and all relativity conditions could be satisfied by putting λ equal to zero or, in other words, omitting it altogether.

To Sir Arthur Eddington λ is something much more fundamental. There is, in his view, a contest between two forces ; the ordinary gravitational attraction tending to make the individual galaxies cling together and the cosmical repulsion—a dispersive force tending to make a system expand uniformly—trying to scatter them. λ is a measure of this cosmical repulsion at unit distance. Sir Arthur regards the cosmical constant, equally with the constant of gravitation, as a constant of nature. Therefore he writes : "The conception of the expanding universe seems to crown the edifice of physical science like a lofty pinnacle. Or perhaps its strange fantastic character suggests that it would be more aptly compared to a gargoyle. But for my part I do not look on it either as a pinnacle or a gargoyle. I believe that it is one of the *main pillars* of the

edifice." There are many physicists who are not prepared to accept this view. But Sir Arthur, in his investigations on the constants of nature, has done much to make it plausible. A chapter is devoted to an account of the general argument upon which these investigations are based.

If, as Sir Arthur believes, this work is "on the main route of the future development of physics," it demands serious consideration. The investigations themselves are extremely abstruse and there can be few who would honestly claim to understand them. The summary of the general line of argument, given in Chapter XI, is therefore the more valuable. Sir Arthur starts with what he terms the seven primitive constants of physics: the charge and mass of an electron, the mass of a proton, Planck's constant, the velocity of light, the constant of gravitation and the cosmical constant. These can be replaced, by suitable combination, by seven others, of which three are respectively a length, a time and a mass, and four are dimensionless quantities. These four can be regarded in the truest sense as constants of nature. They are: (1) the mass-ratio of the proton and electron; (2) the fine-structure constant; (3) the ratio of the electrical force between the proton and electron to the gravitational attraction between them; (4) the ratio of the natural radius of curvature of space-time to the wavelength of a mean Schrödinger wave. The question arises whether these are what may be termed ultimate constants or whether some or all of them can be dispensed with.

Sir Arthur's answer to this question is that there is at most one arbitrary constant, *viz.* the large number that is the basis of both (3) and (4), which is identified as the square root of N, N denoting the total number of particles in the universe. We can only indicate very briefly and incompletely the outline of the argument. The interaction between two electrons is treated in classical physics as a difference in the behaviour of one particle due to the presence of the other. In quantum theory it is treated as a difference in the *probable* behaviour of one particle, arising from the presence of the other. The actual probability is expressed as the product of an *a priori* probability and a modifying factor. The change of actual probability due to the presence of the second electron can be incorporated either in the initial probability or in the modifying factor. The two procedures "are often mixed incongruously. When a few particles are considered, the interaction is described by the constant forces of repulsion or attraction and is then incorporated in the modifying factor. When many particles are considered, we used Fermi-Dirac statistics and the interaction is in-

corporated in the initial probability distribution. Sometimes—for greater security—it seems to get put in both ways ! " Thus a subject that is really one is arbitrarily divided into two. By developing this line of thought applied to a system of two particles and investigating the way in which the probabilities connected with the system are affected by the fact that it is impossible to distinguish one particle from the other (this conception forming the basis of the Fermi-Dirac statistics) an evaluation of the fine-structure constant is obtained.

The next stage, leading to the evaluation of the ratio of the masses of the electron and the proton, is more involved. The basis of the argument is that in passing from a macroscopic mass to a microscopic mass, as, for example, in the determination of the mass of the electron, there must be some connecting link between quantum theory and relativity theory. " In wave mechanics the mass of the electron is *defined* to be a certain characteristic of the simple wave function of the electron alone. When a so-called experimental determination of the mass has been made, it is assumed to give the value of this characteristic. . . . On the other hand, the experiment can only determine characteristics of the double wave function of an electron and a standard macroscopic mass. At some point therefore the theorist has inadvertently telescoped a double into a simple wave function, and we nowhere find any consideration of what is involved in this jump. . . . It remains to inquire under what circumstances the jump can be made without ill consequences. We find that there are just two values of the mass for which the simple wave function will give the same observable results as the double wave function would have done ; so that by introducing this jump the physicist (to hide his misconduct) is forced to assign to his ultimate particles one or other of these two masses. . . . Consequently he regards these masses as the only ones which can properly be separated off and treated as having an independent existence."

These two masses are identified as those of the proton and electron. Each is determined in terms of a unit m_0 and the ratio agrees closely with the experimentally determined constant (1). The unit, m_0, is interpreted *from a certain point of view* as the mass of the universe. It is regarded as the small irreducible mass (or energy) that by the Uncertainty Principle is associated with any particle in curved space. This is connected, through Heisenberg's principle, with an uncertainty of position, and explained as the uncertainty of position of the origin of reference of all the particles in the universe. This leads to a value for $R/\sqrt{N}$, where R is the

radius of space. Since relativity theory gives a value of R/N, both R and N can be separately determined.

The argument does not seem quite watertight though it must be admitted that the results are in reasonably good accordance with observational results. That such a unification should be found possible must be looked upon as one of the greatest advances in physics of recent years. Sir Arthur is gifted with extraordinary intuition and, difficult though the theory is to comprehend, it must not be passed over on that account.

Within the scope of a review that has extended even to the length of the present review, it is possible to touch upon only a few of the matters dealt with in this book. I can only hope that the readers of this review will have had their interest aroused sufficiently to read the book for themselves and that, when they have arrived at the end, they will turn again to the beginning and re-read it. For it is not possible to absorb all that is in this book in a single reading.

THE INCALCULABLE ATMOSPHERE. By R. A. WATSON WATT, B.Sc., F.Inst.P., M.I.E.E., National Physical Laboratory, Teddington. Being a Review of **Physical and Dynamical Meteorology**, by DAVID BRUNT, M.A., B.Sc. [Pp. xxii + 411, with 112 figures.] (Cambridge: at the University Press, 1934. 25s. net), and of **A Manual of the Principles of Meteorology**, by R. MOUNTFORD DEELEY, M.Inst.C.E., M.I.Mech.E. [Pp. xii + 285, with frontispiece and 134 figures, including 3 plates.] (London: Charles Griffin & Co., Ltd., 1935. 15s. net.)

IT is the major misfortune of Meteorology to mean all things to all men. To seven million out of the seven-and-a-quarter million wireless licence holders it means primarily saying whether we shall have rain to-morrow. Six of the seven millions would be surprised, if not indignant, to find no word "forecast" or "forecasting" in the index of either of these substantial volumes, each offered as a text-book of meteorology. The surprise, if not the indignation, would be legitimate. For the priesthood of meteorology no longer expound, from the temple steps above to the patient multitude below, the mystery and meaning of their high calling.

There are those in all priesthoods who, however mildly, resent the intrusions of the uninitiate and the apostate. Mr. Deeley, bearing high credentials from another realm, may expect some of that more generous tolerance that the world at large accords to the non-professional expositor, but he cannot escape the sharper scrutiny which the professional must turn upon the intruder. With all the daring of unchastened enthusiasm he calls his work *A Manual of the Principles of Meteorology*, and offers neither apology

nor qualification. The implied claim is a challenge which cannot be ignored by the most tolerant or apostate of reviewers. Professor Brunt has learnt caution in a stern school; his notably larger and fuller work undertakes only to treat of a special and limited subject called "Physical and Dynamical Meteorology." He tells us first what he means by Meteorology, and then what part of it he selects for study in this admirable text-book "for postgraduate students" and "those engaged in the profession of meteorology."

It was in the jubilee year of the Royal Meteorological Society that its President commended meteorology because it was so easy; it could be pursued by an intelligent gardener. For Professor Brunt "Meteorology, the science of things in the atmosphere, is concerned with the measurement and co-ordination of pressure, temperature, density and humidity of the air, and of the motion of the air relative to the earth. It seeks to explain the motions observed in terms of the changes of pressure, temperature and humidity, brought about directly or indirectly by the effects of incoming solar radiation." Professor Brunt's definition (never a word about rain!) would be accepted *in toto* by Mr. Deeley, yet the difference between these two books is profound, and is opposite in sign to that suggested by their titles.

These divergencies in view and in treatment are comprehensible. They rest on the triple aspects of Meteorology as hobby, as science and as art. They rest on the customary division of a science into observational, metric, classificatory and analytical branches. They rest on the circumstances, peculiar to meteorology, which merge recollections from the observational with extrapolations from the analytical into the art of forecasting. The dearth of accurate popular exposition and explanation would, indeed, matter less in any other branch of physics, pure or applied, than in this. For in no other does the man in the street, in the field, on the sea, in the air, feel the raw material of the science more intimately around him, the foundations of the art more deeply rooted in the traditions of his race and of his craft, the official preoccupation with the science and the art alike more widely advertised by newspaper and loud-speaker. And if neither of these books is designed for the man in the street, yet one of them increases the obscurity while the other provides a starkly honest picture of the formidable difficulties of the forecaster's art in its approach towards a science.

It would be the slightest of exaggerations to say that the task of physical and dynamical meteorology would be finished when the detailed heat balance-sheet of the atmosphere could be written down. For the intermediate accounts, between the intake of solar

radiation from space to the output of earth radiation to space, would bring into measurement the whole of the processes of weather. The merits of Professor Brunt's book include a full and systematically arranged treatment of the thermodynamics of the atmosphere and of the two most difficult sections of thermal transfer, by radiation and by turbulence. The prime demerit of Mr. Deeley's book is an arbitrary and premature assumption that the accounts will refuse to balance at all without the introduction of an unmeasured but demonstrably slender credit item, in the form of corpuscular bombardment from the sun. When accountancy descends from the quantitative to the merely qualitative it may be suggestive but it must be suspect.

Mr. Deeley's book is unsatisfactory not only because its scope falls far short of its title, but because it is discursive, iterative, elliptic and uneven. The sole justification for it, after pruning away the redundant and the well-known, is a potential sixty pages on "Atmospheric Pressures in High Latitudes" and on "Sunspots and sources of Cyclonic Energy." To the former the author has obviously devoted original thought and hard labour, but he mars his exposition at this vital point by an over-compression which would have been very welcome elsewhere. To the latter of his main subjects he brings a doubtful premise ("Meteorologists are agreed that many climatic and seasonal phenomena are not such as we should expect to experience as a result of the operation of known physical phenomena"), an inadequate study of recent literature, an absence of documentation by which fact might be discriminated from hypothesis, and an absence of quantitative test. The test, as Professor Brunt would agree, could not be conclusive, but on broad lines it could be attempted now. It was the duty of the author to see it attempted or to choose a more modest title. His book may indeed be recommended to postgraduate students and professional meteorologists, but it is unprofitable reading for the beginner in search of the principles of meteorology.

No contrast with other works is needed to give merit to Professor Brunt's book. It is a joy in itself, and it will remain unique and invaluable until the happy day when the work of the author and his fellow-pioneers in the building up of scientific meteorology have made its most valuable chapters obsolete. Beginning, as it should, with "The facts which call for explanation," it turns at once to the physics of the air. The air of the meteorologist is no simple oxygen-nitrogen mixture, but a more troublesome fluid in which water-vapour is the enemy of simplicity. This water-content removes the air from exact thermodynamic treatment and removes

it, too, from the tractable "black body" or "transparent" categories, through a "grey-body" class which lamentably failed to express its behaviour, to the "selectively absorbing" class in which it has been firmly and fruitfully established by Simpson's comparatively recent and epoch-making fresh attack on radiation problems.

The dynamical and thermodynamical behaviour of this intractable fluid, the problems of radiation and of turbulence, occupy a dozen lucid and stimulating chapters, in which the foreground is occupied by partially abstract mathematical physics, in which, however, no serious loss of contact with the real world is permitted. The later chapters bring the complexities of this real world into the foreground, and lead into discussions of the great outstanding problems of the cyclone and anticyclone, of air-masses and the polar front, and of the general circulation of the atmosphere.

The general reader will expect to find, in a treatise so erudite and so careful, some guidance on the present difficulties of the forecaster's art. He may admit that the intelligent gardener and his master are meteorologists. He may accept Sir Napier Shaw's official dictum that the Meteorological Office is "an organised central storehouse or memory of the experiences of weather for a long series of years." He may recognise the claim of physical and dynamical meteorology to a place among the nearly exact sciences, in its own right as an intellectual discipline. He may be sufficiently enlightened, or sufficiently cynical, to recall with approval the twelve-year suspension of the public forecasting activities of the Meteorological Office in the 'seventies. But in the end he can scarcely resist the "ordinary listener's" questions: "When will the forecasts be more uniformly successful, when will they be less guarded, when will they become valid for more than the present twenty-four hours—of which several have elapsed before the forecast reaches me?" He will not find, nor will he imprudently attempt an answer, but he will find many pointers towards a reasoned pessimism in Professor Brunt's book. "Theory has so far failed to explain the observed phenomena, as for example in dealing with the general circulation of the atmosphere and the travelling cyclone and anticyclone." (There will be noted here the superficial similarity to, the profound dissimilarity from, Mr. Deeley's premise.) ". . . the difficulty of applying physical reasoning to a medium of variable constitution . . . the paucity of observations in certain parts of the globe and in certain types of weather, in the free atmosphere over the oceans, in the polar regions . . . even over the continents direct observation of upper air conditions in cyclonic depressions is practically impossible." "Great as are the difficulties

of accurate observation in meteorology, the difficulties of theoretical discussion are even greater. It is not possible to isolate a portion of the atmosphere, and to discuss the physical processes which take place in that portion, on account of the translation of pressure systems and of the variation of wind with height, both of which factors make it impossible to specify with certainty what is taking place in a given mass of air. It is customary to speak of the atmosphere as a heat engine, working between a source of heat at the equator and a source of cold at the poles. The most formidable obstacle to progress is ignorance of the laws of transfer of heat within this so-called engine."

"We are left with four outstanding problems in connection with the distribution of temperature in the free air:

1. The approximate constancy of the mean lapse-rate at all heights in the troposphere and in all latitudes.
2. The sudden nature of the change at the tropopause.
3. The approximate constancy of temperature at all heights within the lower stratosphere.
4. The decrease of temperature from pole to equator within the stratosphere.

These problems are discussed . . . but it cannot be said that an adequate explanation has yet been given of any of them."

"Neither theory nor experiment is yet sufficiently advanced to enable us to say with any certainty what modifications should be made [in the laboratory curve for the absorption spectrum of water-vapour] to make it strictly applicable to atmospheric conditions." . . . "Turbulence is a far more effective agent [than radiation] in effecting the transfer of heat upward through the atmosphere. At very small heights above the surface of the ground, turbulence is unable to develop effectively, and the transfer of heat is there mainly by radiation. . . . Many efforts have been made to study [the resulting] large lapse-rates in detail, but the instrumental difficulties are serious. The theoretical difficulties are equally serious."

"Our lack of knowledge of the physical constants of soils, and, in particular, of the precise nature of the effect of a grass covering, makes it at present impossible to use the equation . . . to forecast night minimum temperatures." "The total radiation from a column of water-vapour cannot be represented by any simple mathematical expression." "A further difficulty lies in the fact that the radiation which traverses the atmosphere . . . does not travel upward or downward as a parallel beam, but is diffuse. This is in practice an almost insuperable difficulty." . . . "The physical

facts cannot be summarised in such a form as to lead to tractable mathematical expressions." "It cannot be said that any completely satisfactory theory of the conditions in the stratosphere has yet been evolved. . . . It remains for observation to show whether the temperature at heights of the order of 25 km. in the stratosphere really shows an increase from equator to pole." "Why the ozone should show this association with depressions is as yet a mystery, whose solution may reverse many of the ideas at present accepted concerning the dynamics of the atmosphere." "Errors will arise in the forecasts of temperature by the above method on account of neglected physical processes such as radiation, turbulent mixing, vertical motion, which may be accompanied by condensation of water-vapour or evaporation of water drops falling through the air. It is therefore not surprising that the coefficient of correlation between the predicted and observed temperatures should come out rather low." "No satisfactory detailed theory of land and sea breezes or of [mountain and valley] winds has been worked out." "Surface temperatures are notoriously unreliable for tracing fronts. Temperatures at say 2000 feet would afford a much more satisfactory criterion, but such temperatures are not usually available. In actual practice the forecaster has to rely very largely on indirect evidence." "The drawing of fronts on a synoptic chart cannot be regarded as a purely impersonal scientific operation . . . the drawing of fronts is full of pitfalls for the unwary. . . . The differences [among charts, showing fronts, issued for the same occasion by different European meteorological services] are astonishing, not merely in the details of the fronts but in their main outline, and the forms of isobars based on the same observations frequently show wide variations." "Observations of upper winds and temperatures have been far less useful to the forecaster than was anticipated some fifteen years ago."

"It is not possible at present to put forward any satisfactory theory of the general circulation, in part because the details of the circulation have only been observed very incompletely." . . . "An attempt has been made to summarise the known facts concerning depression, anticyclones, and the general circulation. The summary is of necessity incomplete, on account of the paucity of certain types of observations, particularly in the upper air . . . the difficulties have been largely due to lack of observations of the right type. . . . Further progress must depend largely on the introduction of new ideas . . . does not exhaust the problems, many of which are not yet sufficiently advanced to be capable of specific statement."

The unworthy aspirant may well be turned sorrowfully away by this sombre picture, but the neophyte who has truly heard the call will be stimulated to a new ardour. The general reader, the ordinary listener, his questions so unhopefully answered by implication, will pose new questions. The Marxist and the individualist will draw morals from those inspiring currents of fresh air which Brunt draws in from the capitalist-imperialist studies on manned flight, and the absence of a substantial profit-motive in weather-prophecy. All alike will be moved to call for a new survey of the heroic measures which are needed for an accelerated advance in meteorology. Whether the survey be made by professional meteorologists or by a court of representative users, two main findings are morally certain. The first is that the day of classificatory empiricism is done; that no mere accumulation of long experience by the greybeard "Clerk of the Weather," so dear to the jester and so little likely to be found in the meteorological service, will ever again suffice for the needs of a forecast service; that the urgent new meteorology of air navigation must be physical and dynamical meteorology. As flying speeds increase, the demand for merely rapid and accurate weather reporting may somewhat relieve the demand for short-range forecasting. But there can be few qualified judges who believe that moderate- to long-range forecasting will ever be successfully achieved unless its practitioners are highly qualified physicists with an intuitive and imaginative gift for seeing an invisible fluid of variable composition playing out its complex rôle on a three-dimensional stage of vast extent. The second finding is that the existing and almost traditional methods of observation and reporting are gravely inadequate, and in particular that the disappointments of the last fifteen years in relation to upper air data rest more on the inadequacy, in closeness of network and in vertical sounding height, and on the relative staleness in time, of the data obtained than in any weakening of the belief that meteorological truth is not to be found at the bottom of an atmospheric well.

THE SOLID STATE OF MATTER. By J. D. BERNAL, M.A., Crystallographic Laboratory, University of Cambridge. Being a Review of **Vol. II of Papers and Discussions of the International Conference on Physics, London, 1934.** [Pp. viii + 183, with 62 figures.] (London: The Physical Society, 1935. 10s.)

VERY wisely, the organisers of the International Conference on Physics did not attempt to cover the whole field of the solid state of matter. The work of recent years has shown that, in this hitherto

intractable field, there are extensive regions for experimental and theoretical research: indeed, the nature of the solid has now as much right to be considered a branch of physics as has molecular physics or thermodynamics.

The material in this volume falls into two quite distinct sections. The first deals with the deepest aspect of cohesion, the nature of the forces holding not only molecules but atoms in crystals together, as treated by the modern quantum mechanics methods; the second, of the types of behaviour of crystals in relation to mechanical forces which cannot be explained by any simple atomic theory.

In the first section there appears for the first time in English an account of Professor Hückel's theoretical investigation of the nature of the benzene ring. It is only by considering the ring as a whole, and not as separate atoms or links in it, that its properties can be attacked by quantum mechanical methods. The π electrons of the carbon atoms in their aromatic structures are not bound to individual atoms, but spread over the whole ring or complex of rings. By finding the possible states for electrons in such a system and noting down which of them are occupied, it is possible to show, first of all, why a certain type of molecule has what the chemists have denoted as an aromatic character, and secondly to give in terms of one unknown constant the extra molecular energy associated with this character for a very large variety of aromatic compounds. This in itself is a notable achievement. Up till now we may say it does, however, only put into precise mathematical terms the vague, but useful, intuitions of the chemist, based on a wealth of experimental material. But the quantum method does show possibilities of going beyond this. The case of tri-phenyl-methyl can only be explained in chemical terms rather weakly by conceptions such as steric hindrance. Here it is possible by rigid calculation to show that two molecules of tri-phenyl-methyl have in fact a lower energy than one of hexa-methyl-ethane.

A new experimental approach to the same problems is shown by Dr. Robertson's paper on the measurement, by X-ray Fourier analysis, of inter-atomic distances in molecules. The distance between atoms in a molecule is proved to be a very sensitive test of the kind of linkage involved, and may prove in time, when it is possible to give it quantitative meaning, one of the most powerful methods of molecular analysis.

Professor Hund considers the more general question from the quantum mechanical point of view of why it is that certain substances are metals, others insulating solids, and others again gases.

By means of an extremely simple model he is able to demonstrate that this depends to a large extent on how many electrons there are to fill the outermost layer of the atom. If the number is small, the electrons can change their places, and a metal results: if the number is too large, all places are occupied and a rare gas lattice results: but if the places are half filled, only the stable binding states are filled, while the unstable remain unfilled, and an insulating crystal like diamond results. We are here plainly approaching the fundamental ideas at the back of crystal chemistry. We shall soon be able to calculate instead of only finding intuitively what kind of substance any combination of atoms is likely to produce.

The second part of the Conference was devoted to the real crystal and no longer to the ideal crystal of the crystallographers. Here were gathered together for the first time representatives of nearly all the schools of thought in this difficult and controversial subject, and the result, although not bringing any new material to light, certainly served to give a much more comprehensive and clear picture of the present state of the subject than heretofore available. The two chief topics were, first, the existence or non-existence of a spontaneous secondary structure in crystals, that is, of some regular or quasi-regular structure a hundred to a thousand times the size of the lattice structure of the crystal. A large number of phenomena in the nature of surface markings, evidences for lamellæ, etc., go to support this view, of which the chief protagonists are Zwicky and Goetz. Other observers do not deny the existence of a structure, but consider it an accidental property connected with the mode in which the crystal was grown. This view was ably supported by Ewald and Renninger, who have shown that rock salt, usually considered the typical mosaic crystal, can in fact easily be prepared in an almost ideally perfect state. Whatever the real nature of these crystal imperfections, their importance for the mechanical properties of crystals is very great. The two protagonists in this field, Professor Smekal and Joffé, however much they may disagree with each other, both attribute the actual plastic breakdown of crystals to the existence of such flaws. A particularly valuable contribution to this discussion was given by Dr. Orowan, who has been able to show that cracks very much smaller than those demanded by the original Griffiths theory are sufficient to cause all the observed phenomena of breakdown.

The actual mechanism of plastic breakdown in crystals, particularly metals, occupied the second part of this section. Dr. Burgers and Professor Schmidt produced a very interesting new experimental material, but we are still very far from having reached

any fundamental explanation of plastic deformation. The theory of G. I. Taylor, combined with the experimental results of Stepanov, suggest that the mechanism of plastic deformation may be of a double nature, of which only the first stage follows the classical picture of slipping of a layer of atoms over each other, whereas the actual macroscopic change must be considered in some sense analogous to an electrical breakdown, in which the heat liberated is sufficient to break up the crystal structure over a considerable distance from the position of the slip plane.

On the whole, the discussion will have served its purpose if it has focussed attention on these fascinating, if still obscure problems.

REVIEWS

MATHEMATICS

The Differential Calculus. By THEODORE CHAUNDY, Student and Tutor of Christ Church, Oxford. [Pp. xiv + 460, with 7 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. 35*s*. net.)

NEW books on the Calculus are plentiful, and are generally much like each other; but this one is quite out of the common: both in plan and in execution its author has broken away from tradition and produced a work of remarkable interest and novelty.

Its theme is the Differential Calculus—not the Differential and Integral Calculus, as is more usual: and even the Differential Calculus is pruned of many developments which are normally given with it, such as applications to geometry and mechanics. Surely this must be the first text-book on the subject in which there is no mention of acceleration or of curvature!

Lecturers on the Calculus will find Mr. Chaundy's book a rich mine of unfamiliar results, new methods of proof, and fresh types of examples; and ideas taken from it will be infused into all future treatises, so that its publication will mark an epoch in the teaching of the subject.

At the same time, we must frankly say that it does not seem likely to come into use as a text-book; very few students can afford to pay 35*s*. for their manual of Differential Calculus, and its great length, its exclusion of applications to the other mathematical sciences, and the absence of solutions to the examples, will all tell against it. Moreover, it is not written with a careful avoidance of obscurities and a watch for possible difficulties. Thus near the beginning of the book, after introducing irrational numbers, it continues: "The system of number thus far extended constitutes the system of *real* number. It provides an arithmetical equivalent for every point of a straight line." The reader is left in doubt as to whether the assertion in the last sentence is presented as an obvious deduction from what has gone before, or a deduction which is not obvious but of which the proof is for some reason not given, or whether it is not a deduction at all, but a new axiom regarding the nature of geometrical continuity. The last is the true supposition: but how is the beginner to know?

Yet in spite of some defects of this kind, we welcome the book for its freshness and originality and scientific value.

E. T. WHITTAKER.

PHYSICS

Relativity Physics. By W. H. McCrea, M.A., B.Sc., Ph.D., F.R.S.E. Methuen's Monographs on Physical Subjects. [Pp. viii + 87, with 8 figures.] (London: Methuen & Co., Ltd., 1935. 2*s*. 6*d*. net.)

The purpose of these monographs is to give a *résumé* of their subjects from the point of view of modern developments and to provide a connecting link between the text-book and original research. They are written especially for the research student and for the senior undergraduate who is approaching the end of a degree course. In addition, they are intended to appeal to a wider field of readers not so directly connected with academic life.

An author of one of the monographs has therefore to face the problem of presenting the subject matter concisely, simply and in a way which is attractive to a reader who is not a specialist in the subject. He has also to indicate its modern application.

Judged from the point of view of these objects the volume under review is to be described as excellent. Its subject is the Special Principle of Relativity and its opening chapters require the simplest of algebraical manipulations. They are very consistently presented in the spirit of the subject; as an illustration we may note the derivation of the formula for mass by a method which might well replace some of the hybrid presentations with which the student of physics is familiar.

Later on the reader will require to be familiar with the electromagnetic theory of Maxwell, and a few sections will be better understood if he is familiar with the Hamiltonian methods of generalised mechanics. These sections may, however, be omitted by a reader whose interest does not lie in this direction.

The applications to Atomic Physics are very useful and very much to the point.

The volume is to be recommended particularly to students who still have examinations to consider and to all those who are attracted to a wider knowledge of the Theory of Relativity.

H. T. F.

Properties of Matter. By A. E. Goddard, M.Sc., F.C.S., and H. F. Boulind, M.A., B.Sc. [Part 1: Pp. viii + 101, with 57 figures; Part 2: Pp. viii + 119, with 63 figures. Parts 1 and 2 bound in one volume.] (London: Methuen & Co., Ltd., 1934. 4*s*.)

This surprisingly cheap book consists of two parts. Part 1 is designed to cover the ground required by Intermediate examinations, and Part 2 forms a supplement which raises the standard to that of the pass degree. Here, perhaps, the authors sometimes fall a little short of the goal; for instance, the elementary theory of bent beams is omitted as being "somewhat complicated," the dynamics of the helical spring do not appear even in their simplest form, and very little reference is made to the common methods of measuring surface tension. On the whole, however, the treatment is very satisfactory, and a wide range of subjects is brought to the reader's notice. These include Friction and Lubrication, Osmosis and Brownian Motion as well as the classical Dynamics, Surface Tension, Viscosity and so on. Throughout both parts attention is paid to the methods of setting out and treatment of experimental observations, and the straight-line graph appears in several

places. Examples are printed at the conclusion of each chapter, and answers are provided.

The book has a definite place among those dealing with properties of matter, and, no doubt, teachers of pass degree classes will recommend it to their students as forming an excellent summary of the subjects, rather than an amplification of lectures.

R. C. BROWN.

Principles of Mathematical Physics. By W. V. HOUSTON. International Series in Physics. [Pp. xii + 265, with 3 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1934. 21*s*. net.)

THIS book has been written more particularly as a text-book for students attending a course in an American University. The contents correspond to the studies of students in this country who are attending an honours degree course or are at the post-graduate stage and are engaged on research work in Physics.

The author meets with the difficulty, which is common to teachers everywhere, that it is necessary to teach mathematics to students before they can appreciate the principles of physics. Thus we find very much more mathematics than physics in the earlier part of the book.

Physical principles come a little more into the foreground in the chapter on Thermodynamics, but it is a chapter of only twenty pages and is thus of necessity very incomplete. This is followed by a short chapter on Statistical Mechanics, after which we come to four chapters on Electricity and Magnetism and on the Restricted Theory of Relativity.

More than half the book is devoted to the mathematics applicable to mechanics, the author being convinced of the importance of this branch of the subject as providing the conceptual basis for physics and as illustrating the methods employed in its theoretical aspect.

The subject of the book is wholly classical, if we include under this title the Theory of Relativity.

It is somewhat difficult to recommend the work to English students as a text-book by which we usually understand a treatise dealing thoroughly with its subject within a prescribed or accepted scope. The author makes no claim to be providing a text-book of this character. The volume belongs to the International Series in Physics with its familiar large and well-printed type. It could not, therefore, in its 260 pages be very detailed in the wide range of its subjects.

The purpose of the book is to direct a course of study and it is written for students who have the advantage of listening to a teacher and with opportunities to ask questions. Its use is to keep the student's attention on a precise course and its incompleteness should urge him to seek further information or to try to answer for himself the questions which the incompleteness sets him.

For the reader whose studies are not under direct control it should serve to point out what is known in these subjects and what he requires to supplement by reference to more detailed works.

Many examples are included in the text throughout the book with a view to exercising the reader in the principles they illustrate and of rousing in him the spirit of enquiry.

H. T. F.

International Conference on Physics. Papers and Discussions. Vol. I: Nuclear Physics. [Pp. viii + 257, with 1 plate and 83 figures.] (London: The Physical Society, 1935. Paper covers, 10*s.*; bound, 12*s.* 6*d.* net.)

The subject of nuclear physics is a favourite one for conferences, large and small, at the present time. It was the topic of the Solvay Conference in October 1933 and, almost exactly a year later, occupied at least half the attention of the International Conference on Physics which met in London, presided over jointly by Professor Millikan and Lord Rayleigh. The report of the Solvay Conference was published towards the close of last year; that of the London Conference followed a month or two later. Its first volume (Nuclear Physics) is now under review. Prefaced by the address of welcome of the president of the Royal Society and an opening survey by Lord Rutherford, its main bulk is made up of papers and discussions grouped under five heads. The material so gathered together forms an authoritative exposition of the state of development reached by these rapidly expanding branches of physics at the time in question.

First, in a section to itself, and without discussion, is a brief introduction to the new ideas upon which Born is at present attempting—rather speculatively, maybe—to base a more satisfactory quantum electrodynamics. This will certainly appeal to the theorist. Then follow, in succession, sections on natural β decay (3 papers and 5 contributions in discussion), artificial radioactivity (2 papers and 4 discussions), disintegration and synthesis of nuclei (5 papers and 7 discussions) and cosmic radiation (8 papers and 6 discussions). Here is a wealth of information for the specialist—some of it hitherto unpublished, and a great deal which will interest the general reader. Provided with so much, the latter will surely be more than satisfied; the former, however, may occasionally be inclined to grumble. Even then his criticism will concern omissions rather than anything which is included. It is rather a pity that the contributions of Fermi (on artificial radioactivity produced by neutron bombardment), of Compton and Bennett (on cosmic ray "bursts") and of Bethe and Peierls, are little more than abstracts of papers: together they occupy less space than the longest of the contributions in discussion. The task of editor to an international conference must indeed be a thankless one, and in this case, if Mr. Awbery has failed to extract full-length papers from some of his contributors, he has marshalled the rest with efficiency—even with tact (the presidential italics are there, as usual)! Finally, the Cambridge University Press has produced a very pleasant volume and, as was to be expected of them, done ample justice to a series of very remarkable photographs.

N. F.

Electrons (+ and —), Protons, Photons, Neutrons, and Cosmic Rays. By R. A. Millikan. [Pp. x + 492, with 98 figures.] (Cambridge: at the University Press; Chicago: at the University Press, 1935. 15*s.* net.)

In 1917 Prof. Millikan wrote what soon established itself with students both in England and America as an extremely popular account of much of the new physics—his book, *The Electron*. This was many times reprinted and a second edition was published in 1924. Now it reappears, with a great deal of new material under a more comprehensive title. In bulk the book is well-

nigh doubled. Ten chapters and eight appendixes represent *The Electron* almost as it was last reprinted; six chapters and two appendixes have been added. In this rearrangement the convenience of the publishers seems to have been largely studied; thus there is no numbering of equations in the later chapters, so that those of the appendixes retain their original reference numbers, and many changes in the text appear somewhat cramped for analogous reasons. (The atomic weights given in Appendix I are the same as those of the 1923 impression, although the International value has been seriously changed in more than 20 cases, and five atomic symbols remain unconventional.) But these are small criticisms when the new material should chiefly engage our attention.

After a short chapter on the wave aspect of matter and a longer one on the spinning electron, this new material is mainly concerned with topics germane to the general problem of "cosmic rays" and, less seriously, with certain cases of nuclear transformation involving neutrons and other particles. I must report at once that I found some of it disquieting. Whether it will appear equally so to the reader who is not actively engaged in the particular field of research with which the book deals, I do not know. The danger is that it may not. He will certainly admire the splendid illustrations and realise that the experiments described—most of them carried out by Prof. Millikan, or under his direction—have exercised the highest skill and ingenuity; it remains to be determined whether or not he will be satisfied with the mode of presentation. For myself, frequently, I was disappointed. "Although my purpose is to deal mostly with the researches of which I have had most direct and intimate knowledge . . . I shall hope to be able to give a correct and just review of the preceding work out of which these researches grew, as well as of parallel work carried out in other laboratories" —so the author writes (pp. 4, 5). This is one important point at issue: in treating of it frequent quotation is preferable to the expression of a personal opinion. In this place, however, even quotations must necessarily be limited to a few cases. For example: "Prior to the night of August 2, 1932, the fundamental building-stones of the physical world had been universally supposed to be simply protons and negative electrons" (p. 320)—this in spite of the fact that Heisenberg had been working at his theory of the neutron-proton constitution of nuclei throughout the summer of that year. Likewise, "Prof. Lauritsen and his collaborators have obtained similar results with the use of protons . . ." (p. 352)—with no mention here of Cockcroft, Gilbert and Walton who first obtained such results. Incidentally, it may be pointed out that of the five references cited in support of Lauritsen (p. 354) only two have any direct relevance to the point at issue (the first of these references is given its correct context on p. 381). One might almost conclude that the dates by which research at Pasadena is described are the dates of the experiments, those by which outside work is characterised being the dates of publication. Thus (p. 350), "From . . . 1896 up to January 15, 1934, the phenomenon of radioactivity had been found to be beyond the control of man. . . . On the aforementioned date, however, Mme. Irene Curie and her husband, M. F. Joliot, read a paper before the French Academy in which they showed . . ." Again, "Thus far they [*i.e.* positive electron tracks] had appeared only in cosmic-ray photographs taken at the Bridge Laboratory. In March, 1933, beautiful confirmatory evidence for the existence of positrons . . . was presented by Blackett and Occhialini . . ." (p. 335). I am not suggesting that Prof. Millikan should have been at any pains to

discover the prior dates of the European experiments, merely that he appears to attach to dates in general more importance than might normally be regarded as healthy.

The second point of general criticism has reference more particularly to those chapters dealing with the nature of the cosmic rays. It is that, although the results of other observers are fairly liberally quoted, the interpretation which these authors themselves attach to their experiments is never explicitly stated. There is no opportunity for the reader to attempt an individual judgment of rival views. It may be that few readers will bring to the book any very mature critical faculty which might qualify them to judge; all the more reason, then, that they should be spared equally uncritical writing: "from the known energies freed in the building of the common elements out of hydrogen, I had estimated as early as 1928, that the most penetrating of the observed cosmic rays had an energy of as much as 500 million electron-volts" (p. 321). All this is unfortunate, since the book might have served admirably as a strictly logical exposition of Prof. Millikan's own views, the claim of which to serious consideration no one would wish in the least to deny.

Perhaps at some future time Prof. Millikan might find opportunity to write such an account; then, if it were not determined in form by the needs of a popular text, serious students of physics would be certain to find it inspiring.

N. Feather.

Electromagnetism. By H. M. Macdonald, M.A., F.R.S. [Pp. xvi + 178.] (London: G. Bell & Sons, Ltd., 1934. 12*s*. 6*d*. net.)

The name of the author is a sufficient assurance that this book on Electromagnetism is both authoritative and accurate. It presents a consistent scheme for the description of electrical phenomena developed from the fundamental laws of electromagnetism. It takes the view expressed by Faraday that it is important to distinguish between those phenomena which are independent of the nature of the material medium and those dependent upon it. The laws assumed to be fundamental are those which are independent of matter. Assuming that Ampère's Law relating electric current and magnetic force, Faraday's relating rate of change of magnetic force and the electric force and Fresnel's that the waves are transverse, are of this nature, the electrical effects of empty space are treated without reference to the ideas of specific inductive capacity and permeability. These are regarded as peculiar to material media. In the treatment of such media it is assumed that they are represented by an electric and a magnetic current distribution due to the motion of electric and magnetic particles. The propagation of electrical effects is based on the assumption of the presence of oscillators in matter. When a disturbance falls on it without breaking up its steady motion, we have the case of propagation in transparent media, and this idea of particles with a steady motion that can be affected by an incident disturbance is used in the treatment of propagation in conducting media.

A chapter is devoted to the special problem of the conducting sphere, and the longest chapter of the book is given up to the treatment of diffraction.

A very useful chapter to those whose interest lies in the application of the theory to wireless phenomena is that on radiation and resonance.

The book ends with the treatment of moving media, where an electric

charge is replaced by a continuous distribution, but the solution of the equations so obtained are shown to be adaptable to isolated charges.

The characteristic of the book is the clear presentation of the subject and its careful statement of the assumptions underlying the application to the various branches.

The treatise is one of the author's last contributions to a subject in which he was a leader and master and it is a monument to his accuracy and lucidity of expression.

H. T. F.

Theory of Alternating Current Wave-Forms. By P. Kemp, M.Sc.Tech., M.I.E.E. [Pp. ix. + 218, with 73 figures and 32 tables.] (London: Chapman & Hall, Ltd., 1934. 15*s*. net.)

The series of monographs to which this book belongs is intended to perform the very useful function of collating and making readily accessible the most important information on specialised subjects in electrical engineering. In recent years the enormous extension of alternating current supplies and the more exacting conditions of operation, to which electrical engineers must ensure that the apparatus used conforms, has made a study of non-sinusoidal waves of far-reaching consequence.

The book under review, explaining as it does the essential principles governing harmonics in alternating-current power systems, is therefore a timely addition to this monograph series. Although the author does not say that his treatment of the subject is with special reference to power frequencies, it is at once apparent that the communications engineer is very scantily catered for. Thus the production of beats by the superposition of two harmonics of near frequency, referred to by the author as harmonic interference, is dealt with in a very superficial way, totally inadequate for the study of a radio-frequency heterodyne system. Nevertheless, there are sections of the book such as those dealing with wave-filters and harmonic analysis where a more general treatment is employed, applicable to communications systems.

In dealing with resonance and power factor, the author assumes that the reader is quite familiar with the general meaning of these terms, but the writer feels that it would be helpful if a precise definition were given, suitable for use in all cases whatever the wave-form of the current or voltage.

The author has developed his subject systematically and given a clear exposition of the essential ideas involved. Having properly grasped these ideas, the reader should have no difficulty in following the various applications afterwards discussed, although a considerable amount of labour must have been involved in working out the various formulæ and numerical examples given. The mathematics employed is quite simple and electrical engineers in general, but particularly those concerned with the distribution and utilisation of electric power, should find a study of this book invaluable.

H. M. Barlow.

The Principles of Electric Power Transmission by Alternating Currents. By H. Waddicor, B.Sc., A.M.I.E.E. Third edition. [Pp. xxi + 449, with 164 figures.] (London: Chapman & Hall, Ltd., 1935. 21*s*. net.)

In 1930 the writer had the privilege of reviewing the second edition of this book and he then expressed the opinion that as a treatise dealing in a concise

form with the theory, design and operation of alternating-current transmission systems it was unsurpassed. Of the third edition, just published, one is justified in going even further and asserting that the volume has no rivals in the field with which it is concerned.

The author's mastery of his subject is manifest by his ability not only to analyse the principles underlying the various problems involved but with no less success to expound and explain their practical significance. The treatment is as complete as anyone would normally desire and it is never confused by details which might be considered irrelevant. Throughout, the author obviously aims at giving a sound and concise statement of the essential facts shorn of unimportant frills, and he has been remarkably successful.

After a preliminary review of elementary economic and electrical principles the author shows how to deduce expressions for the capacitance and inductance of various arrangements of conductors. The influence of the primary constants on the distribution and phase of current and potential at different points along a transmission line is then investigated. A short line for which the capacitance can be neglected is considered first and then the argument is extended to the more general case. This is followed by an analysis of the special properties of the various conductors in practical use and a description of different types of supporting structures. Corona effects, the insulating arrangements for overhead lines, methods of voltage and power-factor control, are all discussed in turn. Two chapters, in this edition, are devoted to the characteristics of underground cables and these include a *résumé* of many recent developments. The economics of the choice of a conductor, its size, the voltage and frequency, etc., are briefly considered. Apparatus for the protection of lines against short-circuits and dangerous pressure rises due to induced charges, switching operations and resonance is described in detail, with an explanation of the conditions under which such disturbances arise. Finally an interesting investigation is made of the limiting load with which a given system may be expected to cope.

The field covered is necessarily very wide, and for certain specialised branches of the subject the reader is rightly referred to other sources of information. Although the book is primarily intended for the student it will be found of almost equal value to the designer and to the operating engineer.

H. M. Barlow.

CHEMISTRY

Crystal Chemistry. By Dr. O. Hassel. Translated from the German by R. C. Evans, B.A., Ph.D., B.Sc. [Pp. ix + 94, with frontispiece and 8 figures.] (London: William Heinemann, Ltd., 1935. 6*s*.)

Whatever it may have been in the past, crystal structure is now no longer a "special" study: in its broader aspects it is, or ought to be, perhaps the most general scientific study of our time. Almost anything from a diamond to a nerve, say, falls within its scope, and it is a singularly remote investigator who cannot learn anything from its far-flung lessons.—"Far-flung" is good, as Polonius might have said; but there is bound to be some difficulty in catching, especially when so many things are flung from every point of the scientific view, and chemists in particular seem hardly to have grasped all within their reach. But it is a hard life doing research nowadays, and we are all more or less living in glass houses.

In this little book, however, Dr. Hassel has performed a real service for

chemists, and Dr. Evans has ably seconded him by putting the work right under the noses of the English-speaking section. There are things—most of the important things in inorganic crystal chemistry in fact—here set down that chemists really should know if they are to keep abreast of events: things like the established sizes and shapes of atoms and radicals, the chief ways in which they fit together in the solid state, and the main types of structure that are found time and again. The whole book can be read in a few hours, and the story is told so simply that practically no special crystallographic knowledge at all is required. Dr. Evans has made it still easier by taking the opportunity to expand or "translate freely" most of the parts that might still seem a bit vague, and there are plenty of references to original papers for those who feel they would like to go further into the matter.

There is only one shortcoming, and that is not entirely the author's fault, since a number of outstanding advances have been made in quite recent years. We refer, of course, to the X-ray interpretation of organic crystals and biological material. This does not mean that there is no mention of such work; but little more than one-tenth of the text is devoted to it. On this account it would be rather more correct to call the book *Inorganic Crystal Chemistry*.

W. T. Astbury.

Chemical Kinetics and Chain Reactions. By N. Semenoff. The International Series of Monographs on Physics. [Pp. xii + 480, with 148 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. 35*s*. net.)

Developments in the theory of chemical kinetics during the present century have been largely concerned with the application of classical laws aided by a more intimate knowledge of the energetics of molecules which has been furnished by spectroscopic data. Thus the mechanism of uni- and bi-molecular reactions has been elucidated (Lindemann, Hinshelwood), the significance of activation energy has been made clearer (London, Polyani) and a number of complicated reactions have been analysed into stages which obey the normal laws (Bodenstein).

Nevertheless, it has become evident that the majority of chemical changes do not proceed regularly but exhibit marked deviations from the so-called normal laws. In particular such striking phenomena as ignition limits and explosive reactions found no satisfactory explanation until the advent of the theory of reaction chains. The concept of non-branching chains was introduced by Bodenstein in 1913 to account for the kinetics of the HCl reaction; the subsequent developments of the theory and its extension to branching chains are largely due to the author of the work under review.

The first part of the book develops systematically the mathematical theory of non-branching chain reactions and of branching chain reactions including the theory of explosions. It is followed by a critical discussion of the experimental data for nearly 50 reactions. This discussion is particularly valuable; it is written in a vigorous and stimulating style and makes accessible the results of a number of very recent investigations carried out in the U.S.S.R. A general summary follows and the remarkable conclusion is reached that the vast majority of chemical reactions proceed by a chain mechanism.

This volume will undoubtedly rank as one of the most important monographs of the series which is now being issued by the Oxford University Press.

S. Sugden.

The Adsorption of Gases by Solids. By S. J. GREGG, B.Sc., Ph.D. Methuen's Monographs on Chemical Subjects. [Pp. viii + 120, with 15 figures.] (London: Methuen & Co., Ltd., 1934. 2*s*. 6*d*. net.)

THIS book is a useful addition to the small monographs on physical and chemical subjects for which Messrs. Methuen have now established a reputation. In its own class this is a valuable little book and the reviewer was somewhat surprised to find how fully the field could be covered without undue condensation in such a small space. None the less, it must be admitted that the subject is probably too large a one to be successfully treated in a little over 100 pages.

The arrangement of the book is convenient and the author wisely and successfully makes a clear distinction between the properties of porous and non-porous adsorbents and, in general, treats these two classes of materials separately. The summary that the author gives of the enormous literature dealing with adsorption on charcoal and on silica gel is particularly sound and readable. Not unnaturally, the section on activated adsorption is less successful and this can be ascribed to the fact that at the time the book was written opinions differed widely as to the best interpretation of the experimental results. Nevertheless there is a good and critical account of the slow diffusion that often occurs parallel with, or in place of, true activated adsorption.

The reviewer also found the elementary mathematical discussion given on the forces causing adsorption extremely clearly and attractively written and likely to be of great value to the student. The account of the accommodation coefficient is also good but little more than two pages are devoted to the treatment of photoelectric, thermionic and other electrical effects, and it was surprising to find no numerical estimate of the life-time of an adsorbed molecule in any part of the book. The book is free from misprints and well presented. This is a good sample of a monograph on an interesting subject, which might be read with profit by anybody interested in adsorption.

O. H. W.-J.

The Kinetic Theory of Gases. By PROFESSOR MARTIN KNUDSEN. Methuen's Monographs on Physical Subjects. [Pp. viii + 64, with 20 figures.] (London: Methuen & Co., Ltd., 1934. 2*s*. 6*d*. net.)

PROF. KNUDSEN has put into permanent form the substance of three lectures which he delivered in the University of London, and has produced a most stimulating monograph on the foundations of the kinetic theory of gases.

The mathematics given in this book is of a very elementary kind and yet the author draws the most interesting deductions from the fundamental postulates of the kinetic theory. It is right and proper that Prof. Knudsen should dwell largely on his own researches in this field. He deals with the subjects of evaporation and condensation, with the molecular flow of gases through tubes when the free path is long compared with the diameter of the tube, with the cosine law of reflection and its experimental verification, with the theory of the absolute manometer and finally with the coefficient of accommodation. All these subjects have been illuminated by the experimental skill of the author and have thereby assumed greater importance in physics and chemistry.

There is concentrated wisdom in this little volume and no one will regret the modest sum of half a crown required for its purchase.

J. E. L.-J.

Physico-Chemical Practical Exercises. By WILLIAM NORMAN RAE, V.D., M.A., F.I.C., and JOSEPH REILLY, M.A., D.Sc., Sc.D., F.I.C. [Pp. xiv + 276, with 74 figures.] (London: Methuen & Co., Ltd., 1934. 7*s*. 6*d*. net.)

BEING familiar with more than one excellent text-book on Practical Physical Chemistry, the reviewer did not expect to conclude after reading this book that it had filled a gap. None the less, in spite of the fact that it contains little material that is not well treated elsewhere, he was left with no doubts as to its value. It would not be sound advice to suggest that students already using some other similar text-book should use this one in its place, but there is no doubt that masters teaching fairly advanced physical chemistry in a school, or demonstrators responsible for the conduct of elementary degree courses at a University, should all possess a copy.

Its special merits are two-fold. The value of the book is greatly increased by the inclusion of a large number of worked examples which make it particularly easy for the student to follow the arguments involved. Incidentally, to a student who has not got ready access to a laboratory this book would probably be more useful than any other for that reason. The other special merit it has is that the blend of classical experiments whose didactic value remains unchanged with experiments demonstrating newer principles seems to be unusually successful. Thus the authors have been careful to include newer or more accurate methods where such are available for doing the more old-fashioned experiments, and the inclusion of experiments on the parachor and on the determination of the Mean Free Path and the diameter of molecules is also to be welcomed. There must in any book of this sort be apparent omissions according to the taste of the reader, and it may be mentioned that the reviewer was most surprised to find that the only work on conductivity referred entirely to weak electrolytes. It would have seemed to him quite proper to have included simple measurements on the conductivity of, for example, potassium chloride.

The book is well produced and is to be recommended at the price.

O. H. W.-J.

Experimental Physical Chemistry. By FARRINGTON DANIELS, J. HOWARD MATHEWS and JOHN WARREN WILLIAMS. Second edition. International Chemical Series. [Pp. xx + 499, with 140 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1934. 21*s*. net.)

THE rapid development of modern physics has made the teaching of physical chemistry increasingly difficult. A knowledge of the classical work on the properties of gases, liquids, solids and solutions must be acquired as a foundation and there are a number of experimental text-books available for this purpose. Unfortunately when the student proceeds to research and to grapple with modern problems he finds a wide gap between the background given by his degree course and the treatment of the subject from the standpoint of mathematical physics. The only means of narrowing this gap is to begin as early as possible to familiarise him with the fundamentals of spectroscopy, photo-chemistry, and quantum theory so that the mind may be accustomed to thinking in these terms. The second edition of *Experimental Physical Chemistry* is suitable as the practical text-book for an honours chemistry course on the above lines.

The book is divided into three sections dealing respectively with "laboratory experiments," "apparatus" and "miscellaneous operations." The experiments described in Part 1 differ only in minor details from those performed in the majority of elementary physical chemistry courses. The instructions given are generally very brief but are adequate if the work is carried out under the supervision of a competent teacher. Parts 2 and 3 are intended to be a guide to the methods used in advanced work and research; the authors do not claim that their survey is comprehensive, but on reading the book one is surprised to find how much it has been possible to condense within its five hundred pages. For example, in the treatment of the physical properties of gases there is some discussion of modern high vacuum pumps, directions are given for the operation of barostats and there are small diagrams of ten of the more common forms of all-glass manometers. The wide scope of the work is shown by the presence of chapters on photo-chemistry, dielectric constants, capacity and high frequency bridges and vacuum tubes. References to recent and important original papers are always given, thus providing a stimulus to general reading and supplementing the account of those experiments which are inadequately described in the text. There are a few experiments where the description is misleading or the example chosen unfortunate, but as a whole the book can be confidently recommended.

R. H. P.

Clinical and Pathological Applications of Spectrum Analysis, with notes on Spectrography in Chemistry and Mineralogy, and Tables for Qualitative Analysis; being the Authorised Translation of Part II of **Die Chemische Emissionsspektralanalyse,** by DR. WALTHER GERLACH and DR. WERNER GERLACH. Translated by JOYCE HILGER TWYMAN. [Pp. 145, with 52 figures.] (London: Adam Hilger, Ltd., 1934. 14*s*. 6*d*. net.)

THIS monograph deals with the detection and estimation of certain elements, mainly metallic, in specimens of biological material. The main title is apparently intended to cover that portion of the book which describes the determination of metallic elements in various organs and tissues derived from clinical and pathological patients after treatment with gold, silver or bismuth, for example. The detection of poisonous elements, or metals at the edges of gunshot wounds and so on, is discussed. There are two valuable features of the book: (1) the description of the high-frequency spark whereby a minute piece of tissue or fabric may be examined spectrographically without preliminary treatment such as ashing or dissolution, and (2) the tables and rules for deciding the presence of traces of metals when other metals may yield spectrum lines in close proximity to those of the suspected element. It is shown, for instance, that the Pb line 3639·5 may be obscured by the Fe line 3640·4. In this case the Fe line 3638·3 should also be visible.

The hint is given that the observer's own experience (p. 110) should be of value. That the spectroscopic method, in experienced hands, is extremely delicate is shown by the detection and estimation of strontium in calcium, when a question of the calcium isotopes was being considered by Hönigschmid (p. 94). Again, minerals and glasses can be attacked by the procedure described on p. 107, using an interrupted arc, or an uncondensed spark to provide the emission spectrum.

The main purpose of the book is achieved; namely, the description of the

method of estimation of metals in minute portions of biological material and how a metal may be traced throughout the various parts of an organ with precision. There are few errors, only one of importance, the word "nitrate" for "nitrite" (p. 101). On p. 28 there is a curious apparent misuse of the word "maximum," the whole sentence being obscure.

On the whole, the translation is very fair and readable, but too literal adherence to the original has produced some strange words and phrases. Thus, "analysis samples" and "organ samples" presumably mean "samples for analysis" and "samples of an organ." "To last . . . between 1 and 5 minutes . . ." (p. 27) doubtless means "to occupy . . . between 1 and 5 minutes. . . ." Apart from these few peculiarities the text is readable.

As in this firm's publications generally, the type and reproduction of the spectrograms are good and clear.

J. J. F.

Die Mikrogasanalyse und ihre Anwendung. By Dr. Heinrich Schwarz. [Pp. xii + 286, with 52 figures and 31 tables.] (Vienna and Leipzig: Emil Haim & Co., 1935. RM. 22.80.)

This useful book suffers somewhat from the choice of an unsuitable title. A manual of micro-gas analysis is expected to deal especially with micro-methods, and, if not to describe, at least to mention all the more important ones—while this book comprises many well-known methods of ordinary gas analysis and only a selection of the micro-methods. The author, having been for many years in charge of the chemical laboratories of medical clinics, was naturally in the first place interested in the application of gas analysis apparatus to the study of medical problems: the determination of oxygen and carbon dioxide in blood, of urea in blood and urine, of the gas metabolism in cells, etc. Here he speaks with first-hand knowledge, and these chapters are full of valuable information. The descriptions of methods of purely chemical interest are somewhat less satisfactory, and one looks frequently in vain for any statements of the limits of sensitivity. On the other hand, tables are included about the surface of the body of children, human metabolism with respect to size and age, and similar ones which one would expect to find in a text-book on physiology rather than on analytical chemistry. As an introduction to methods of gas analysis for use in clinical laboratories the book is to be recommended to physiologists and doctors.

F. A. Paneth.

Unit Processes in Organic Synthesis. P. H. Groggins, B.S., Editor-in-Chief, assisted by M. R. Fenske, D.Sc., S. J. Lloyd, Ph.D., L. F. Marek, M.S., H. P. Newton, M.A., A. J. Norton, B.Sc., E. E. Reid, Ph.D., R. N. Shreve, A.B., W. A. Simpson, B.S., A. J. Stirton, M.A., and H. E. Woodward, Ph.D. Chemical Engineering Series. [Pp. xii + 689, with 137 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 30*s.* net.)

The Editor-in-Chief has evidently experienced some difficulty in finding a title for this work and in the end has not been very successful, since that chosen fails to indicate the great importance of the contents. One may fairly say that the book is one of the most valuable to the organic chemist that has been published in recent years and in order to define its scope the reviewer cannot

do better than quote the Preface. " In this book, an attempt is made to present in a systematic manner the principles and practice of the more important and well-defined reactions in organic synthesis. In commercial operations, it is found necessary to bring to the application of such reactions the pertinent information derived from inorganic and physical chemistry as well as the contributions of the chemical engineer. . . . The term *unit processes* is used here to represent the embodiment of all the factors in the technical application of an individual reaction in organic synthesis. . . . In the process of synthesising dimethylaniline from benzene, three distinct unit processes are involved, *viz.* (1) nitration, (2) amination by reduction, and (3) alkylation."

The subjects dealt with are nitration, amination by reduction, diazotisation, halogenation, sulphonation, amination by ammonolysis, oxidation, hydrogenation, alkylation, esterification, hydrolysis, the Friedel Crafts' reaction, and polymerisation. The terminology will indicate that the book comes from the United States.

The method of treatment is best indicated by taking, for example, nitration. A survey of nitro-compounds is given, something is said about orientation, a discussion follows of the various nitrating agents, their use and the mechanism of nitration. The plant used in nitration is described and finally some details are given for certain technical nitrations, namely of benzene, chlorobenzene, acetanilide, phenol, naphthalene and toluene. In this section all the available data on proportions of isomerides formed under varying conditions is given and a further section supplies the thermal data relating to the preparation and use of nitro-compounds, whilst another deals with the recovery of spent acid.

Other subjects are treated similarly and the book is packed with information not readily available elsewhere. Its use to the chemical engineer is obvious, but it is no less valuable to the research organic chemist of the most " academic " variety, as it supplies knowledge which is of the greatest importance in carrying out the common operations in the laboratory. The lecturer can also learn much from it; how many teach that synthetic malic acid is replacing tartaric and citric acids because it can now be easily prepared by the catalytic oxidation of benzene with air in the presence of vanadium pentoxide to maleic acid which can be hydrated by heating with water under pressure, or that the availability of high pressure plant and suitable catalysts have made practicable the preparation of aniline and phenol from chlorobenzene ?

Good though the book is, it is capable of improvement, especially on the more theoretical side. The formation of nitrites and nitro-compounds from alkyl iodides and nitrites is explained on outworn notions. Cain's structure for diazonium salts is needlessly mentioned and a ring structure is given for diazonium sulphonates, dry diazonium salts are described as somewhat (!) explosive. The function of potassium bromide with bromine in brominations does not seem to be fully appreciated, in some cases the addition of potassium bromide to bromine water can be used to reduce the free bromine concentration, for example, in the estimation of dinitrophenol in picric acid. These, however, are but minor criticisms of what is a really valuable book which should be read from cover to cover by all students of organic chemistry.

O. L. B.

The Nitrogen System of Compounds. By EDWARD CURTIS FRANKLIN. American Chemical Society Monograph. [Pp. 339, with 26 figures.] (New York: Reinhold Publishing Corporation, 1935. $7.50.)

SELDOM has the present writer been in the uncomfortable position of being unable to say one good word for a book. On the strength of the similarity between water and ammonia in some of their properties the author has evolved a whole new nomenclature for nitrogen compounds. For example, mono- and di-alkylamines are called ammono-alcohols and trialkylamines ammono-ethers, because they are the nitrogen analogues of the alcohols and ethers. Guanidine is a deammonation product of a hypothetical ortho-ammonocarbonic acid just as carbon dioxide is a dehydrated form of the hypothetical ortho-carbonic acid. Cyanamide, written as $HN{=\!=}C{=\!=}NH$, is an ammono-carbonic acid, hydrocyanic acid an ammonocarbonous acid, whilst dimethylnitrosamine and diazobenzene ($C_6H_5N : N.OH$) are, forsooth, esters of aquoammono-nitrous acid. This treatment is followed through the whole book and the writer feels disinclined to waste his own or his readers' time with a further description of a treatise which might have altered the course of chemistry if written when the subject was still much under the influence of the Type Theory but is not likely to attract much attention now.

There is a good deal of information summarised on the less common organic nitrogen compounds and their methods of formation which would be valuable if presented in the accepted nomenclature and interpreted on modern theories of organic chemistry.

O. L. B.

Metallurgy. By E. L. RHEAD, M.Sc.Tech., F.I.C., A.I.M.E. Sixth edition. [Pp. xiv + 382, with 182 figures.] (London, New York, Toronto: Longmans, Green & Co., Ltd., 1935. 10*s.* 6*d.* net.)

THE first edition of this very useful book on elementary metallurgy was published in 1895. Since that time, it must have served to instruct a large number of metallurgical students in the first principles of their branch of applied science.

The book was written with the definite object of providing an easy introduction to metallurgy. It is not a complete treatise, but a careful study of its context should give the student a good general knowledge of the subject and enable him to read the more advanced works with both profit and pleasure.

In the new edition, the sixth, a certain amount of deletion and condensation of matter has made it possible to enlarge many chapters and to add new sections on pyrometry, metallography and foundry work.

The volume first deals with the physical properties and the behaviour of metals, then with metallurgical terms and processes and next describes furnace types, refractory materials and fuels. In the description of the metallurgy of the metals, most space is given to that of iron and steel. Copper, lead, tin, zinc, mercury, gold and silver receive less detailed attention, but in view of the scope of the book, the treatment is adequate. In addition, mention is made of the other common metals.

The book closes with a chapter on alloys, pyrometry and metallography and with a list of the atomic weights of the principal elements. This latter appears to be out of date and should be corrected in later editions. Bearing in mind that the book has been written as an elementary manual, it can be recommended to those contemplating the study of metallurgy.

E. W. YEOMAN.

GEOLOGY

The Geology of Burma. By H. L. Chhibber, Ph.D., D.Sc., F.G.S., F.R.G.S. With contributions by R. Ramamirtham, M.A., and a Foreword by L. Dudley Stamp, D.Sc., B.A., F.G.S. [Pp. xxviii + 538, with 37 figures and 26 plates, including 3 geological maps.]

The Mineral Resources of Burma. By H. L. Chhibber. [Pp. xv + 320, with 13 figures and 10 plates.]
(London : Macmillan & Co., Ltd., 1934. 30*s*. net and 18*s*. net.)

The geology and mineral resources of Burma are of abounding interest and importance, not only in themselves, but also because Burma is the connecting link between India and the Himalayas on the one side, and Malaya and the East Indies on the other. The geology of India has been well served by several text-books of which that by Prof. D. N. Wadia is the most recent, while Malaya has been treated in Mr. J. B. Scrivenor's two well-known works (*S.P.*, XXIV, 154 ; XXVI, 528). All these books were published by Messrs. Macmillan, who have again rendered geologists a great service in publishing Dr. Chhibber's monumental volumes.

The Geology of Burma is very comprehensive indeed. It begins with studies of the physical features of the country including rivers and lakes, and goes on to deal with earthquakes, hot springs, mud volcanoes, denudation, limestone caves and the coastline, before settling down at p. 110 to the stratigraphy of the region. The representatives of Archæan, Ordovician, Silurian, Devonian, Permo-Carboniferous, Triassic, Jurassic and Cretaceous systems, are fully described, each in a separate chapter, before the widespread and economically important Tertiary rocks are dealt with in Chapters XXI–V (pp. 210–69). Then comes a chapter on Post-Tertiary deposits, also important from the economic point of view because of their content of tin, gold and gems.

The remainder of the book, with the exception of a final chapter on geotectonics, treats very exhaustively of the igneous rocks of Burma. Dr. Chhibber is here in his element as a petrologist, and he may be criticised for allotting a disproportionate amount of space to his beloved igneous rocks. The reviewer, however, is not disposed to quarrel with him on this account, as he thinks Dr. Chhibber has provided one of the best, and certainly one of the most detailed, regional petrographies which he remembers to have read. For any future discussion of the distribution of igneous rocks and of the problems of petrographic provinces this work will prove invaluable.

Useful appendices have been contributed by J. B. Scrivenor on the correlation of the geology of Burma and Malaya, and by P. Evans on the correlation of the geology of Burma and Assam. Three excellent folding geological maps of Burma as a whole, of the Northern Shan States and of the Mergui and Tavoy district, are provided, as well as a number of maps and sections in the text. The book is also well illustrated by plates illustrating scenery and rock structures.

In the preface to *The Mineral Resources of Burma* Dr. Chhibber tells us that he originally intended to incorporate this volume with *The Geology of Burma*, but later found it advisable to issue the work in two parts. This was regarded as the better course because Burma, from the point of view of mineral resources, is undoubtedly the most important province of the Indian Empire.

This work is also very thorough and comprehensive. After an introductory

chapter on the geological and geographical distribution of the mineral deposits, and statistics of production, there follow chapters on the numerous valuable gemstones, jadeite (which is treated exhaustively in 62 pages), amber, iron ores, coal and lignite, gold, lead silver and zinc, petroleum (this chapter contributed by Dr. L. Dudley Stamp), tin, tungsten, salt, and a long list of miscellaneous minerals. This bald summary alone suffices to show the value and the possibilities of Burmese mineral deposits. But Dr. Chhibber is not content to rest satisfied with these exhaustive accounts of mineral deposits. He also includes chapters on soils (written in conjunction with S. P. Aiyar), water supply, roadstones and building materials. In so far as it was necessary to elucidate the structure of the mining districts the stratigraphy of certain important areas is briefly sketched. Dr. Chhibber has also indicated the areas where further prospecting might be undertaken for certain minerals, and has contributed many useful notes on mining methods and on the present and prospective uses of the mineral deposits.

The book is well illustrated by a number of excellent plates and text-figures, including a folding map showing the locations of the more important mineral deposits.

These two volumes attest incredible industry and vast erudition on Dr. Chhibber's part, and are likely to remain the standard works on Burmese geology and mineral resources for many years to come. The production of the volumes is of the high standard so long associated with the house of Macmillan.

G. W. T.

Introduction aux Études Minières Coloniales. Publications du Bureau d'Études Géologiques et Minières Coloniales. [Pp. viii + 349, with 25 figures and 9 plates.] (Paris: Société d'Éditions Géographiques, Maritimes et Coloniales, 1934. Frs. 36.)

EARLIER publications from the source indicated above were reviewed in SCIENCE PROGRESS for January, 1935, p. 565, and the series is now continued with the present work which, like its predecessors, is the outcome of a course of lectures delivered during the sessions 1932–33 and 1933–34 at the Paris Museum of Natural History. The justification for a special introduction to studies in colonial mining is that special problems in alluvial prospecting, and difficulties arising from extensive surface rock alteration such as lateritisation, are encountered in the tropical colonies which are not met with nearer home. Moreover, there are special local problems of transport, travel and labour in colonial mining.

The book takes the form of a number of more or less connected essays from different hands. L. de Launay contributes a paper on "Les Grandes Types de Gisement," a succinct statement of the classification and origin of ore deposits. A. Lacroix follows with an excellent short memoir on lateritic rock alterations and their mining consequences. L. Thiebaut deals similarly with practical problems in the working of alluvial deposits, and C. Schlumberger writes on geophysical methods of prospecting. Then come four papers dealing with labour, transport and native problems incidental to colonial mining. A. Duparque then provides a valuable memoir on the microscopic study of coal illustrated by some beautiful plates, and J. Orcel similarly describes the methods of microscopic study of opaque metalliferous minerals. The two concluding papers deal with the commercial and marketing problems inci-

dental to the disposal of colonial mining products. The memoirs by Lacroix and Duparque may be singled out as of quite exceptional value to geologists in a volume the standard of which is uniformly high.

G. W. T.

BOTANY

Primitive Land Plants. By F. O. Bower, Sc.D., LL.D., F.R.S. [Pp. xiv + 658, with frontispiece and 449 figures.] (London: Macmillan & Co., Ltd., 1935. 30*s*. net.)

This work is in the main a restatement of the views of the author on the comparative morphology and anatomy of the Archegoniateæ, a restatement embodying in substance the views that appeared in *The Origin of a Land Flora* and in *Size and Form in Plants*.

Although the author states that his chief aim was not the tracing of phylogonies, a perusal of these pages inevitably recalls the enthusiasm for phylogenetic speculation of a quarter of a century ago, speculations which indeed have their uses although the chief centre of interest to-day is entirely different.

The various groups receive somewhat unequal treatment even having regard to their relative importance. As we should expect, the Filicales occupy the largest section, nearly a third of the entire work, and these chapters constitute a valuable summary of the views of our leading authority on this group in a more concise form than in the author's well-known work in three volumes. The account of the Bryophyta on the other hand is rather disappointing and here as elsewhere in the book the reader has the impression of dealing with laboratory specimens rather than with living organisms whose structure and mode of life is elucidated by laboratory technique.

The main theses are sufficiently familiar not to require recapitulation, but we may note that Prof. Bower treats the Devonian plants as synthetic types serving to bridge the gap between the Bryophyta and the Pteridophyta, and sees in the gametophyte and sporophyte of the Anthocerotales an approach to what he regards as the primitive condition of these phases in the life history.

E. J. S.

The Botanist in Ireland. By R. Lloyd Praeger, D.Sc. [Pp. xii + 587 sections, with 44 plates, 29 figures and 6 coloured maps.] (Dublin: Hodges, Figgis & Co., 1934. 12*s*. 6*d*.)

Students of the Irish flora will give a warm welcome to Dr. Praeger's latest book, for here in convenient form they will find embodied the results of a lifetime devoted to the study of Irish plants.

In the main, the present book follows the lines laid down in his earlier one—*A Tourist's Flora of the West of Ireland*, and much of the subject matter of that work has been incorporated in its pages. Apart, however, from its more ample scope—for the book now covers the whole of Ireland—it is essentially a new work and includes many features not found at all in the previous one.

The book is divided into two main parts. The first of these provides a general introduction to the flora as a whole, while the second, which is planned on a regional basis, describes the characteristic plants to be found in all the more interesting localities throughout Ireland.

This introductory section, which occupies in all some 70 pages, is a most valuable feature of the book and provides just that information which is required for an intelligent study of the flora. It opens with a concise but lucid account of the Geology, Topography and climate of Ireland, and shows how past events in its history account not only for its present topographical features, but have also determined the character of the soils which exert so marked an effect on the nature of the vegetation in different regions.

In the brief section devoted to the climate of Ireland the ameliorating effect of the warm seas and ocean winds which surround the country is shown with telling effect. Ireland lies in about the same latitude as Labrador, yet its mean annual temperature is above 50° F. and, what is probably more significant, the annual range is exceptionally small.

Thus the warmest areas in the hottest month have a mean of about 60° F., while in the coldest areas in the coldest month the mean is just under 40° F. In the west frost is of rare occurrence.

Everywhere precipitation is high, but both humidity and temperature increase steadily towards the west, and it is there that their influence on the character of the vegetation becomes so striking. The English botanist visiting Ireland for the first time is always astonished at the luxuriance of moisture-loving plants. *Osmunda regalis*, with great fronds 5 or 6 feet high, is a common sight in low-lying areas of the west, and Dr. Praeger tells us that on Achill Island *Hymenophyllum peltatum* forms an ingredient of the sward on the slopes of Slievemore, while in marshy ground hard by *Nymphæa alba* grows as an aerial plant.

But in spite of its warm and moist climate the West of Ireland suffers from a high degree of exposure and in winter is subject to a continuous succession of westerly gales. This has an astonishing effect in dwarfing the vegetation on unprotected slopes, yet wherever shelter is available the effect of the mild climate becomes at once apparent. The reviewer will not soon forget the luxuriance of the vegetation growing in the deep clefts and fissures of the limestone in the Burren country of County Clare, yet the exposed slopes of the same rocks are so devoid of vegetation as to present an appearance of almost forbidding barrenness.

A considerable part of the introduction is taken up with a discussion of the character of the Irish flora. An attempt is made—admittedly a difficult one—to analyse it into its various geographical components and a comparison is made with the flora of the adjacent islands of Britain and to a lesser extent of the European mainland.

This section will be read eagerly by students of plant distribution, for it raises issues which have been fruitful sources of discussion and have led to considerable differences of opinion. There will be general agreement with Dr. Praeger's recognition that the Irish flora is in the main the result of a series of post-glacial invasions from the continent of Europe, via the neighbouring islands of Britain ; but many botanists will find it less easy to concur with his conclusion regarding those plants which are absent from Ireland but present in England or Scotland. He regards this " reduction in the Irish flora as due largely to its insularity " and concludes that " the Irish Sea has proved a serious barrier to immigration from the east."

Some little attention is also given to the highly interesting group of plants comprising the so-called " Lusitanian " element in the Irish flora and also to the smaller, but equally interesting section which has its headquarters in North America. Much discussion has centred around these plants, but

Dr. Praeger's views concerning them are well known and it would be inappropriate to discuss them here; the more so as he has dealt with the whole question at some length in a recent paper.

Some forty pages of the Introduction are occupied with notes on the rarer and more interesting plants of Ireland and are followed by a list of unverified records and of missing plants. The botanist visiting Ireland will find himself turning again and again to this section, for it contains a mass of interesting information not readily available elsewhere.

In a brief review it is impossible to do justice to the topographical part of the book. The plan followed has been to take each district in turn and, after briefly describing its physical features and geological structure, to indicate the character of the local flora.

Although this treatment necessarily involves a measure of repetition it is unquestionably the one best fitted to meet the needs of the visiting botanist as he travels from place to place in Ireland. Wherever he finds himself he has but to turn to the appropriate section of the book to see at a glance the plants he is likely to meet with as he explores the neighbourhood.

As regards format the book could scarcely be bettered; of convenient size, well printed and well bound, it is equally suitable for use in the field or for reference indoors, while its wealth of illustrations and its folding maps add much to its interest and value.

A census list at the end of the book gives the distribution of Irish plants throughout the forty divisions recognised in the same author's *Irish Topographical Botany* and its supplements, while an excellent index, completes a book which reflects the greatest credit on author and publisher alike, and which will earn for Dr. Praeger the gratitude of every botanist who visits Ireland.

C. L. H.

Untersuchungen über die Verbreitung, Biologie und Variation der Ceratien im südlichen Stillen Ozean. By E. Steemann Nielsen. Dana-Report No. 4. [Pp. 67, with 73 figures and 11 maps in the text.] (Copenhagen: C. A. Reitzels Forlag; London: Oxford University Press, 1934. 8*s*. net.)

Our knowledge of the distribution of the oceanic Peridinieæ, and especially of the genus *Ceratium*, is steadily increasing. While Peters in 1932 dealt with the *Ceratia* of the South Atlantic, Nielsen has investigated the same genus in the South Pacific. He records in all 60 species which in great part are common to both oceans. He arrives at the conclusion that the factors of greatest importance in determining the horizontal distribution of the diverse species are temperature, currents, and the nature of the water itself, whether oceanic or neritic. He disputes Peters' conclusion as to the importance of salt (nitrate and phosphate) content and of the amount of nannoplankton. According to Nielsen the paucity of *Ceratia* in neritic waters is a result of the accumulation of metabolic products. The evidence for this view is, however, at present rather scanty; moreover, Schiller has recently adduced some further data in favour of Peters' conclusions. About one-third of the Pacific species are shade-forms, not found above a depth of 50 metres in water poor in plankton. These forms are all characterised by flattening of the cell or by other devices for increasing the assimilating surface.

F. E. Fritsch.

Diseases of the Banana and of the Manila Hemp Plant. By C. W. WARDLAW, Ph.D., D.Sc., F.R.S.E. [Pp. xii + 615, with coloured frontispiece and 295 figures.] (London: Macmillan & Co., Ltd., 1935. 30*s*. net.)

IN no crop plant are the ravages of disease more evident than in the banana. Panama disease has been responsible for the abandonment of huge tracts of country in Central America; it is at present threatening the banana industry of Jamaica, and is a possible menace wherever bananas are grown. This single disease has cost the growers losses to be counted in hundreds of thousands of pounds sterling and no remedy is known. Other diseases take a heavy toll of this crop. A book dealing with the diseases of the banana was urgently required and Dr. Wardlaw, breaking entirely new ground, has produced a volume which no doubt will be of great service to the industry.

The book gives a comprehensive and up-to-date survey of the present knowledge of the subject. Apart from storage troubles, 62 individual diseases of the banana and 11 of the related manila hemp are treated—a testimony to the thoroughness with which the literature has been searched. Pertinent extracts from original papers are freely quoted and references are given to a bibliography of 559 titles at the end of the book. The illustrations are carefully selected and well reproduced; a few drawings of fungi are new. A feature of the work is the list of bacteria and fungi recorded on the banana given in an appendix. Descriptive accounts of banana cultivation and transport and storage of the fruit greatly help the reader to understand the problems of the banana industry. The volume is written primarily for the plant pathologist, but it is not so severely technical as to be unintelligible to the layman. The style is vigorous and the text is pleasingly free from misprints.

In a book of this sort complete agreement with the author is not to be expected, but to my mind, it is a pity Dr. Wardlaw has emphasised his unorthodox views on the pathogenesis of the Panama disease. There is no doubt whatever, that *Fusarium oxysporum cubense*, the cause of the disease, is a very virulent fungus, and until Dr. Wardlaw has shown that by improved methods of cultivation it is possible to control this disastrous disease, the importance he attaches to environmental conditions will probably not be accepted by plant pathologists generally. Furthermore, in view of the recent work on the infection of water-melon by *F. niveum* and of cotton by *F. vasinfectum* var. *ægyptiacum*, a re-examination of the method of infection adopted by the Panama disease organism appears to be necessary. But it is with reluctance these criticisms are made. The volume is a very valuable publication and it is to be hoped will stimulate further work on the diseases of this interesting crop.

E. J. BUTLER.

ZOOLOGY

Introduction to the Reports from the Carlsberg Foundation's Oceanographical Expedition round the World, 1928–30. Dana-Report No. 1. [Pp. 130, with frontispiece, 2 figures and 7 plates.] (Copenhagen: C. A. Reitzel; London: Oxford University Press, 1934. 16*s*. net.)

IN June 1928 a small ship, the *Dana*, originally built as an English trawler, set out from Copenhagen on a voyage round the world. She was admirably

equipped for oceanographical work, and her main object was to investigate the pelagic organisms of the open sea, with special reference to the distribution of the larvæ of the various species of eels. The leader of the expedition was the famous oceanographer, Prof. Johannes Schmidt, who had carried out with brilliant success many previous cruises in the North Atlantic and Mediterranean with the same objects in view. This world cruise was the crowning achievement of Schmidt's life ; unfortunately he did not live long enough to see the publication of this, the first of the series of reports embodying the results of the *Dana's* investigations. The present volume appropriately commemorates Schmidt's remarkable work in an In Memoriam notice written by his colleague Prof. Martin Knudsen. For the rest, the volume contains a short account of the voyage by Schmidt's assistants, Dr. Jesperson and Dr. Taaning, a full list of the Stations worked (over 4,000 in number), and details of the numerous Echo-soundings made. It will be indispensable to the professional oceanographer and marine biologist in their study of the later reports of the Expedition, now in course of publication.

E. S. Russell.

Phytoplankton and the Herring. Part 1, 1921–1932. By R. E. Savage, A.R.C.Sc., D.I.C., and A. C. Hardy, M.A. Fishery Investigations, Ser. II, Vol. XIV, No. 2, 1934. [Pp. 73, with 42 figures and 31 tables.] (London : H.M. Stationery Office, 1935. 3*s*. 6*d*. net.)

The fortunes of the East Anglian herring fishery depend mainly on (1) variations in the actual numerical strength of the stock of fish, (2) environmental changes which may cause either a dispersal of the shoals in some cases, or a concentration of the shoals in others, (3) lunar influence, (4) interference with fishing operations by storms, strikes and bad trade.

This account of phytoplankton and the herring is an important addition to our knowledge of fluctuations in the fishery, especially as the authors have dealt, year by year from 1921 to 1932, with the changing characteristics of each season so far as phytoplankton "patches" and the movements of the shoals are concerned. The diatom *Rhizosolenia styliformis* and the colonial flagellate *Phæocystis* are the main components of the phytoplankton barriers which have been found to interfere with the usual migrations of the herrings, and the condition of the water known as " stinking " or " weedy " is definitely associated with very poor catches of fish.

The season of 1927 may be cited as an excellent example of " local interference " by phytoplankton. The early fishing was good on the usual Smith's Knoll ground but suddenly the fishery in that area failed, and the herrings reappeared some 60–70 miles to the north-east. Subsequently, the presence of a broad belt of *Phæocystis* extending for 100 miles across the northern entrance to the southern bight of the North Sea was discovered, and it appears obvious that the herrings, in order to avoid water which they disliked, had to make a considerable detour before they could continue their southerly migration.

In their description of the changing aspects of the fishery between 1921 and 1932, charts have been prepared by the authors to show the results of the many cruises of the research vessel so far as the phytoplankton patches are concerned, but they have always been careful not to press their case for " interference " to the exclusion of other factors which could—and probably did—influence the fishery. They have given full weight to the importance of

lunar influence and brood strength, but it is clear, nevertheless, that the evidence they have produced is more than sufficient to establish the claim that phytoplankton in concentrations has a definite bearing on the fluctuations of the East Anglian fishery.

Part II of this paper is to continue the evidence in the southern part of the North Sea, and one feels that in its complete form *Phytoplankton and the Herring* will fill one more gap in the knowledge of the herring and its movements.

W. C. H.

Early Forerunners of Man. By W. E. Le Gros Clark, D.Sc., F.R.C.S. [Pp. xvi + 295, with 89 figures.] (London: Baillière, Tindall & Cox, 1934. 15*s.* net.)

This volume is certainly one of the most important contributions to anthropology which has appeared in the last few years. The author has made use of the recent detailed studies of the morphology of the Primates and has written within a brief compass what may well be considered a comparative anatomy of that branch of the animal kingdom. Such studies are apt to be rather dull except to specialists, but owing to the general philosophical attitude which Prof. Le Gros Clark has adopted he has avoided the pitfall of producing a mere detailed catalogue of systematic anatomy. He considers that the proper way to approach the problem of Man's ancestry is to start far back in the geological record and to study the general trend of evolution of the various branches, in other words, as he himself says, to look at the matter in prospect rather than in retrospect. He emphasises the necessity of taking all the evidence, not merely a single line such as, for instance, the central nervous or the reproductive system. His evidence leads him to the conclusion that there has been much parallel evolution in the Primates and he stands a confessed believer in Orthogenesis. He cannot see that evolution proceeds by a mere chance working but rather as the result of the development of certain potentialities in the germ plasm, not perhaps quite moving in predestinate grooves like the tram in the Balliol limerick, but still circumscribed in possible lines of evolution. Of the ultimate mechanism he refuses even to theorise but concludes "if the mysteries of the living and evolving germ plasm are even deeper and more enigmatical than we have been inclined to believe, it were better to recognise the fact." In contrast to much that has recently been written about the ancestry of Man this is not a popular book, it presumes considerable knowledge of anatomy and is especially intended for the use of those whose studies have been limited to human anatomy. The work is most appropriately dedicated to Sir Grafton Elliot Smith whose own work in the subject combined with the enthusiasm which he has kindled in his students and colleagues is too well known to need elaboration in these columns.

L. H. D. B.

Elementary Microtechnique. By H. Alan Peacock, M.Sc. [Pp. vii + 200, with 15 figures.] (London: Edward Arnold & Co., 1935. 5*s.* 6*d.* net.)

Quite a remarkable amount of information is contained in the 200 pages of this handy little book. A short introductory account of the structure of animal and plant cells is provided in order to indicate the reason for micro-

scopic work and the need for certain methods. It is intended for the use of teachers in schools and of sixth-form and first-year University students. This aim it will fulfil well and indeed in those Universities where special aspects of Cytology and Histology are not stressed it will probably be sufficient for the ordinary pass student as far as practical microtechnique is concerned. It will at least furnish the backbone of the work and may be supplemented by notes upon special points. On one point we are not in agreement with the method suggested and that is in regard to honing a razor for microtome use. It is true that a skilled worker could do this with a Turkey stone and olive oil and it might serve to sharpen a razor for shaving. In ordinary hands an oil-stone is better fitted for sharpening plane irons and chisels than a microtome razor.

The ordinary text-books on this subject are much larger—and, of course, expensive—and frequently contain so much material that is of interest to the specialist that the ordinary, standard routine methods are hidden. The present volume, however, contains the formulæ for fixing and staining that are of general use and has the advantage of giving information as to where the material for ordinary purposes may be obtained. We wish the book all success, for it fills a much-felt need and can be thoroughly commended.

C. H. O'D.

AGRICULTURE AND FORESTRY

Publications of the Institute of British Geographers. No. 1: Transactions, and No. 2: The Pastoral Industries of New Zealand. By R. O. Buchanan. [Pp. xvi + 99, with 9 graphs and 12 maps.] (London: George Philip & Son, Ltd., 1935. 7*s*. 6*d*. net.)

A comprehensive treatise on farming conditions and methods in New Zealand. The climatic and relief conditions of a country eminently suited to the growth of European animals and pastures are discussed in detail and compared with those of the British Isles. An analysis of the distribution of sheep and cattle shows that this is not correlated with climatic conditions such as rainfall, but is dependent more upon the relief of the country.

The author weighs the advantages of climate, fertility, etc., against the disadvantages of isolated geographic position and small population, and discusses how the former has been largely overcome by improvement in refrigerating and chilling methods, and with the increasing efficiency of transport; and shows how the economic disadvantages of a small population (*i.e.* high wages) is counterbalanced by improvements in agricultural machinery, and in the diminished labour required under milder climatic conditions.

He shows how the tremendous output of pastoral produce has caused the development of large factories and co-operative institutions for control and ease of marketing, and how that competition from other countries has made the farmer and the dairyman pay special attention to maintaining the highest possible grade products.

In brief, this publication gives a remarkably clear and concise outline of farming conditions generally, and should be of interest not only to the geographic and economic expert, but to all those interested in agriculture. Besides giving many useful figures and facts about conditions as a whole, it gives a very fair idea of problems encountered, and the ways and means

of overcoming these. There is a full appendix attached, which includes geological and climatic maps, tables and diagrams dealing with numbers and distribution of stock, grazing areas and exports.

E. M. M.

Forest Mensuration. By DONALD BRUCE, B.A., M.F., and FRANCIS X. SCHUMACHER, B.S. American Forestry Series. [Pp. xvi + 360, with 102 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 21*s*. net.)

THIS text-book is one of the new American Forestry Series and it has to recommend it the high reputation of its authors in this particular corner of Forest Research. It is, however, an unusual book. The arrangement of material, familiar to readers of the older text-books, has been dispensed with and the student is no longer asked to consider first the measurement of individual trees both felled and standing, then the measurement of whole woods and finally the problems arising out of the determination of increment. Instead, he is offered, in a brief section at the beginning, a summary of the methods of direct measurement in the forest of diameter, height, volume and age, and at the end, in a longer but not wholly satisfactory section, a discussion on the prediction of growth and yield.

The rest of the book, including Parts 2 and 3, is much fuller and more thoroughly worked out than the first and the fourth parts which have already been referred to, and it is easy to see where the enthusiasm of the authors is centred. Part 2, entitled "Direct Estimate by Sampling," gives an interesting and well-written account of elementary statistical methods and is illustrated by useful examples drawn from common problems in forest mensuration, while Part 3, "Indirect Estimate based on one or more Independent Variables," deals with graphical methods, the method of least squares in its application to forest mensuration, the transformation of curves into straight lines and the "alinement chart." These parts will be of great service to the forest officer who has to deal in his office with masses of data, and will show him how to reduce these masses into order, and there is much information here that he cannot very well obtain elsewhere. For the man who has to take the measurements in the forest this book has relatively little to offer. The authors no doubt hold the view that in that case experience and not the text-book is the best of teachers.

J. M.

MEDICINE

Respiration. By J. S. HALDANE, C.H., M.D., F.R.S., and J. G. PRIESTLEY, M.C., D.M., M.A. New edition. [Pp. xii + 493, with 124 figures, including 17 plates.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. 30*s*. net.)

THIRTEEN years have elapsed since Haldane first published *Respiration*, and it is fitting that in a new edition Priestley's name should be associated with him, for it was in 1905 that these two authors gave an account of their researches on the regulation of the lung ventilation, the foundation, it may fairly be claimed, of our modern knowledge of respiration. In these thirteen years our knowledge has expanded and this has entailed an extensive revision of the whole book. The reader taking up the book for the first time, and

glancing perhaps at the chapter headings and the long bibliography at the end of the volume, may for a moment fear that he has to face fact piled on fact in pitiless sequence. But the book is far different from this. Throughout it there runs the idea, more fully developed by Haldane in his philosophical writings, that maintained co-ordination is characteristic of life. This guiding principle welds the book into a coherent whole. The regulation of the breathing and circulation to suit the varying needs of the body, the transport of gases by the blood, the delicate adjustment of the hydrogen ion concentration within the body, the influence of changes in the composition of the atmosphere we breathe, are not treated as isolated and independent questions: they are taken in association with one another, so that as we read we can see more and more clearly their true significance in the ordered complexity which we call life.

This book is almost entirely based on experiments and observations made on man, and again and again we are shown how usefully the information gained by the physiologist can be applied to the elucidation of the effects caused by disease. If it gives us an insight into the progress that has been made in modern times in the study of human physiology it also emphasises the close interrelations that must always be maintained between physiology and medicine. It shows us too how the growth of physiological knowledge has enabled us to gauge more certainly the dangers which some have to face in their daily occupations and the measures by which these risks may be minimised—such risks as those of foul air and harmful dust encountered by the miner, of the abnormally high air pressure to which the diver and caisson worker are exposed and of the reduced barometric pressure associated with high altitudes.

C. G. D.

Epidemics and Crowd-Diseases. An Introduction to the Study of Epidemiology. By Major Greenwood, D.Sc., F.R.C.P., F.R.S. [Pp. 409, with 8 graphs and 74 tables.] (London: Williams & Norgate, Ltd., 1935. 21*s*. net.)

Epidemiology is a science which concerns itself chiefly with epidemic phenomena which cannot be explained merely by the summation of individual effects which are more or less understood. It is an old science dating to Hippocrates and throughout its history it has been somewhat given to speculation and argumentation. The typical epidemiologist is a scholarly man with a severely logical, almost scholastic outlook, and nowadays he is usually a trained statistician. He thus usually triumphs in argument with his traditional antagonist, the experimenter, though the latter often gets his revenge in seeing the successful application of his work to communal problems. Prof. Greenwood is an epidemiologist of the traditional sort. He is severely logical, extremely well-informed and is one of our most distinguished living statisticians. At the same time no living epidemiologist has had closer contact with the work of the experimenter, nor has done more to teach experimenters, particularly bacteriologists, to exercise logical and statistical discipline in their arguments. His book brings out very clearly the numerous and fascinating problems presented by epidemics and shows how useless it is at present to attempt to explain them in terms of the individual. Many points of general interest are discussed such as the alleged increase in the incidence of cancer, the decline in virulence of small-pox,

scarlet-fever and tuberculosis. The historical chapters give an admirable account of the development of epidemiological method and include sympathetic accounts of some of the pioneers of the subject. The author expresses the hope that the book will appeal to "all educated men and women interested in the communal aspects of health and disease" and certainly it is not too technical for such readers. Its main popularity, however, is likely to be with health officers, bacteriologists, epidemiologists, statisticians and the general medical public. It is a happy combination of light and amusing reading with solid instruction.

C. C. O.

Illustrations of Regional Anatomy. By E. B. Jamieson, M.D., Senior Demonstrator and Lecturer, Anatomy Department, University, Edinburgh. (Edinburgh: E. & S. Livingstone, 1934. 30*s*. the set of 5 volumes.)

This publication consists of a set of five volumes of illustrations without text, bound in loose-leaf form. They are intended, says their producer, to be used for reference during lectures, perhaps to be pasted in notebooks and perhaps to be coloured by the student as an exercise and as a method of memorising. In so far as any of these procedures may be desirable, the volumes admirably fit the intention. The drawings are accurate, the labelling clear and copious, and the selection and in general the production such as one would expect of a teacher of Dr. Jamieson's ripe experience. Nor would we offer any criticism of the somewhat original methods of learning which the use of these illustrations suggests.

This publication may also be very useful to the teacher who from time to time wonders how best to illustrate his subject on the blackboard; for we unhesitatingly think that the pictures and the selection of viewpoints are ideal.

Even if we doubt whether the use of them will appeal to the majority of students, we have not the slightest doubt that a set of these volumes is a valuable and beautiful possession. The limbs are not included but, no doubt, two volumes will duly be forthcoming to complete the set.

Our congratulations to Dr. Jamieson on a distinctive production of permanent value.

E. P. S.

PHILOSOPHY AND THE HISTORY OF SCIENCE

The Unity of Science. By Rudolf Carnap, Professor of Philosophy in the German University of Prague. Translated, with an Introduction, by M. Black. Psyche Miniatures, No. 63. [Pp. 101.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1934. 2*s*. 6*d*. net.)

The title of Prof. Carnap's essay is somewhat misleading. His theme is rather "the unity of the *language* of science." No doubt the title-subject is partly involved in the thesis. But as the treatment is essentially logistic and linguistic, the actual unity of science is presupposed rather than made out. To the present writer it seems obvious that the problem of the unity of science is much more likely to be dealt with fruitfully on the lines followed, say, in Dr. Wightman's *Science and Monism* (see following review) than on the lines of essentially logistic and linguistic considerations. Prof. Carnap

does not profess to have proved his thesis. The "miniature" essay hardly gave him sufficient scope for that. He has published a larger treatise on the subject, but the present reviewer has not yet seen it, and is consequently unable to judge. For the present it may be sufficient to place before the reader Prof. Carnap's own statement of his main aim, and only to add the remark that the essay under review certainly makes interesting reading. "The thesis of *the unity of science* (writes the author) has nothing to say against the practical separation of various regions for the purposes of division of labour. It is directed only against the usual view that in spite of the many relations between the various regions they themselves are fundamentally distinct in subject matter and methods of investigation. In our view these differences of the various regions rest only upon the uses of various definitions, *i.e.* of various linguistic forms, of various abbreviations, while the statements and words, the facts and objects, of the various branches of Science are fundamentally the same kind. For all branches are part of the unified Science of Physics" (p. 101).

A. WOLF.

Science and Monism. By W. P. D. WIGHTMAN, M.Sc., Ph.D. With a Foreword by SIR PERCY NUNN. History of Science Library. [Pp. 416.] (London: George Allen & Unwin, Ltd., 1934. 15*s*. net.)

IN his Foreword to Dr. Wightman's book Sir Percy Nunn describes it as "an able and thorough study of one of those great ideas that have inflamed the imagination and guided the inquiries both of philosophers and of men of Science since the history of thought began." Not all readers of *Science and Monism* may agree with all the views expressed in its pages, but few can fail to appreciate its deep interest and breadth of competence. What is particularly attractive about Dr. Wightman's studies in the monistic tendencies of science is the happy blending of scientific and philosophic knowledge. And it may be regarded as striking evidence of the value of the pioneer work of the University of London, in introducing higher Degrees in the History, Methods and Principles of Science, that it has stimulated the preparation of books like *Science and Monism*. It augurs well for a closer and more intelligent co-operation between science and philosophy than has been usual in the recent past.

The book is divided into four parts. Part I gives a detailed history of the monistic concept in ancient, mediaeval and modern times to the age of Haeckel. As is but natural, a considerable amount of space is devoted to a study of Spinoza's pantheistic philosophy. Part II deals with monistic tendencies in science till the end of the nineteenth century. It examines critically such concepts or hypotheses as the unity of matter, the unity of natural forces, and the unity of life; and concludes with a discussion of Haeckel's naturalistic monism. Part III surveys the data and concepts of natural science, under the headings of "our knowledge of the external world," "the laws of nature," "the concepts of science," "the notion of causality and the validity of induction." Lastly, Part IV describes the monistic tendencies in the twentieth century, and ends with a statement of the author's tentative conclusions on the whole complex of problems.

Dr. Wightman rightly stresses the importance of studying the history of scientific and philosophic concepts. Much current confusion is due to the neglect of such study. Among other things it is partly responsible for the

mutual misunderstandings between philosophers and men of science. The book under review might well serve as a valuable help towards a better mutual understanding. The present reviewer is so much in general agreement with the views expressed in *Science and Monism* that he feels no desire to criticise any of them. And he warmly commends it to the attention of students of science and of philosophy, and especially to students of science, who are sure to find Dr. Wightman's book more helpful as an introduction to the philosophy of science than any of the special introductions written by philosophical specialists who are not in sufficiently close touch with the mental habits of the students of science.

A. WOLF.

Aristotle, Galileo, and the Tower of Pisa. By LANE COOPER, Professor of the English Language and Literature in Cornell University. [Pp. 102.] (Ithaca, New York: Cornell University Press; London: Humphrey Milford, Oxford University Press, 1935. 7*s*. net.)

THE purpose of this book is to examine the evidence for the belief, current among a wide variety of writers, "that Galileo, having ascended the leaning tower at Pisa, by a single dramatic experiment refuted an assertion of Aristotle that had not been challenged since the days of ancient Greece, nor then." The author especially enjoys himself with those writers who have supplemented with a picture the story of the dropping of the two balls of different sizes and their alleged simultaneous arrival on the ground. Indeed, he makes the whole affair legendary: for Galileo, he says, nowhere mentions the leaning tower or such experiments in his extant writings, the first mention of the experiment being made by Viviani, Galileo's biographer, in 1654, that is, more than sixty years after the experiment is supposed to have been carried out (in 1590). Moreover, Stevin wrote in 1605 that he and Grotius had performed such an experiment *formerly*, and therefore at least half a century before there is any mention of Galileo carrying it out: and further still, the author contends, in criticising the usual versions of the story, Aristotle "in his writings on physics never once uses the Greek word for 'fall' in relation to speed." Full references and complete original passages are quoted, including the passage in Galileo's *Dialogues concerning Two New Sciences* (1638) on which the legend has been built and which attributes to Aristotle a statement that Aristotle never made. An exact and scholarly criticism of an important detail in the literature of the history of science.

D. MCKIE.

Die Quaestiones naturales des Adelardus von Bath. Herausgegeben und untersucht von Dr. med. et phil. Martin Müller, Privatdozent der Geschichte der Medizin an der Universität München. Beiträge zur Geschichte der Philosophie und Theologie des Mittelalters, Band XXXI, Heft 2. [Pp. viii + 92.] (Münster: Aschendorff, 1934. RM.4.40.)

ADELARD (or Aethelard) of Bath was the first Englishman to figure in the history of science. Very little is known about his life and activities. He seems to have travelled a great deal, and claimed to have come into personal contact with many learned men, in the near East as well as in the West. His principal work, *Natural Questions*, was written by him some time between 1111 and 1116, and in Latin, of course. It was first printed about 1475,

and copies of the text have been fairly difficult to procure. Moreover, the printed text was not edited with sufficient care. Dr. Müller has therefore rendered a service to students of the history of science in supplying a carefully edited edition based on a collation of the principal manuscripts. It is true that English-reading students are fortunate enough to have an easily accessible and useful English translation of the *Natural Questions* in the late Sir Hermann Gollancz's edition of Berachya's *Dodi Venechdi* (Oxford, 1920, pp. 85–161). But it is an obvious advantage to have the original text at hand. Dr. Müller's edition also contains (pp. 72–91) a helpful summary of the available information about Adelard and his work. The *Natural Questions* range over a great variety of subjects—botanical, meteorological, psychological and metaphysical problems are discussed in any sort of order. And the answers are for the most part given in terms of the "four elements" and similar Greek concepts. Adelard professed to have acquired his views from Arabian or Saracen thinkers. Some of his remarks throw some doubt on the sincerity of this claim. But in so far as it was sincere, it was probably true only in the sense that in his day the East was better acquainted with the doctrines of the Greek philosophers than was the West. The most interesting feature of Adelard's *Natural Questions* is its general tone rather than its detailed contents. It is, for its day, remarkably free from purely theological explanations of natural phenomena, and does not take shelter behind any kind of "authority" as such. Without denying the plausibility of ultimately referring all natural events to the Divine Will, Adelard attempted consistently, if not very successfully, to explain natural phenomena by reference to their natural causes. In this respect he showed a scientific attitude of mind that was comparatively rare among his Christian contemporaries.

A. Wolf.

Die Lehre von der Bewegung bei Nicolaus Oresme. Von Ernst Borchert. Beiträge zur Geschichte der Philosophie und Theologie des Mittelalters, Band XXXI, Heft 3. [Pp. xvi + 112.] (Münster: Aschendorff, 1934. RM.5.60.)

Nicolaus Oresme lived in the fourteenth century. He was a native of Normandy, studied at the University of Paris, became Director of the Collège de Navarre, enjoyed the patronage of Charles V, and, after being Dean of the Cathedral at Rouen, was made Bishop of Lisieux in 1377. He was a noted translator and commentator of the works of Aristotle, and his first contributions to the study of motion were made in his commentary on Aristotle's *De Coelo et Mundo*. Though essentially an Aristotelian, Oresme disagreed with Aristotle on various points, notably in holding that the Earth moved. Some twenty-five years ago Duhem, the well-known historian of astronomy, directed special attention to Oresme as the forerunner and inspirer of Copernicus. Duhem went so far as to credit Oresme with clearer ideas about motion than Copernicus subsequently had. Dr. Borchert has now given us a detailed account of Oresme's conception of motion, in relation to the ideas of his predecessors in this field of study. The net result is that it is quite impossible to maintain Duhem's view that Oresme anticipated or inspired Copernicus. It is true that Oresme, and even more so his contemporary Buradin, advocated the view of a moving Earth; but his grounds were essentially theological, not strictly methematical like those of Copernicus. One of Oresme's main reasons for believing in the Earth's rotation

consisted in his conviction that the sphere of the fixed stars, being so much nearer God ("The Unmoved"), must be at rest. Copernicus, on the other hand, based his views mainly on the scientific ground that Ptolemy's calculations, based on the geocentric theory, were shown to be inaccurate by actual observations, whereas calculations based on the heliocentric hypothesis were both simpler and more in accordance with observations.

One or two points of some historical interest may be noted here. (1) Oresme posited an infinite space beyond the sphere of the fixed stars. This supra-cosmic space he identified with the unmoved God. Long afterwards Isaac Barrow, Newton and others still identified God and Space. (2) Oresme insisted that although the movement of a body cannot be perceived or measured except by reference to another body in relation to which its position was changing, yet motion itself is something intrinsic in the moving body, a quality, "accident," or "impetus" within the moving body. This "impetus" might vary quantitatively within a body, just as its temperature might vary, quite independently of an observer's ability to note and measure it.

A. WOLF.

Three Philosophers (Lavoisier, Priestley and Cavendish). By W. R. AYKROYD. [Pp. xi + 227, with 8 plates.] (London: William Heinemann (Medical Books), Ltd., 1935. 10*s*. 6*d*. net.)

THIS book is the outcome of Mr. Aykroyd's enthusiastic admiration for Lavoisier, but not so much for Lavoisier, the Newton of chemistry, as for Lavoisier, "the virtual founder of the science of nutrition." Priestley and Cavendish are brought into the picture as contemporaries whose work "dovetails" with that of Lavoisier, though what they had to do with the so-called science of nutrition is not at all apparent. The biographical matter throughout has been industriously collected and is very well written: but the book cannot be regarded as a serious study of Lavoisier's labours. Indeed, the *Prefatory Note* describes the founder of modern chemistry as having "contributed largely to physics and chemistry"!

Priestley was not elected to the Royal Society as a result of the publication of his *History of Electricity* (p. 40): Boyle's "spagyrists" appear on p. 46 as "stagyrists": the statements on pp. 47–8 about the levity of phlogiston and about Mayow are incorrect, and the date of Mayow's *Tractatus Quinque* is wrongly given: the idea that it needed a Laplace to construct the "formidable" equation in the famous *Mémoire sur la chaleur* is laughable (p. 105), although the description of this trivial piece of elementary algebra as "formidable" may help us to understand the author's remark (p. 97) on the use of statistical methods that "graphs and figures are all very well, but what can be more accurate than the clinical impressions of a lifetime?": the middle compartment of Lavoisier's ice-calorimeter (p. 106) did not contain a weighed amount of ice: the statement on p. 214 about Priestley and carbon monoxide is incorrect: the first edition of Kerr's translation of Lavoisier's *Traité* appeared in 1790, not 1793, and in one volume, not in two (p. 219): and the reviewer is unable to make anything of the statement on p. 94 that "in modern times a scientific discoverer is apt to be rewarded by a professorship which compels him to devote most of his time to teaching second year medical students."

"1872" on p. 130 should read "1782."

D. McKIE.

.ANEOUS

Dynamics of Population. By FRANK LORIMER and FREDERICK OSBORN. [Pp. xiii + 460, with 54 figures.] (New York and London: Macmillan & Co., Ltd., 1934. 15*s*. net.)

POPULATION changes are becoming of more and more interest, now that populations are tending to some stability in regard to total numbers. During the nineteenth century large increases in population accompanied advances in the technique of production and were accompanied by rises in the standard of living. The lower Birth and Death rates of the past 50 to 60 years are now focussing attention on the prospect of stationary or declining populations in many large countries in the near future. The quality of these populations has received attention in the past, but the present nearness of this state of affairs means that the problem is no longer one of academic interest to grandparents, but concerns directly all those persons who are now of reproductive age.

The most important single fact which emerges from this and other similar studies of populations is that where apparently stable conditions exist in regard to total numbers, in reality there are large and vital changes in structure of population taking place continually. In a few years the proportions of the total in one group, be it an age group, or a social group, or an occupation group, are altered in a remarkable degree. Populations are not stable; the authors have chosen their title well.

The study under review refers to the United States. The authors have examined in great detail the trends in population growth especially in relation to rural and urban communities, to social groups and to racial origin. The problem of the absorption of immigrant groups into the population of the United States is obviously most important.

The authors point to the fact that the "better" types of citizens are not supplying their proper quota towards population replacement. "Many of the present varying rates of reproduction in American groups are bad from the economic, the cultural or the eugenic point of view." On the other hand there is evidence of a capacity for more isolated and less-developed groups to acquire rapidly the attitude of the majority to current modes of behaviour, and thus the extreme differences in fertility are expected to disappear. There is apparent a need for a greater study of the question whether the new generations will be of as good "material" in all senses of the term as past generations which have contributed to the greatness of the United States.

E. C. RHODES.

The Problem of Noise. By F. C. BARTLETT, M.A., F.R.S. The Cambridge Miscellany, XV. [Pp. x + 87.] (Cambridge: at the University Press, 1934. 3*s*. 6*d*. net.)

THIS little book of eighty-odd pages is an admirable appreciation of the present-day situation in regard to noise as an environmental factor of significance to mental health, comfort and efficiency. Prof. Bartlett has made an important contribution to the study of the problem by orientating the varied opinions, researches and special difficulties associated with this distressing phenomenon of modern civilisation.

He notes the gaps in our knowledge of the mechanism of sensory adaptation, and while he pleads for further research he indicates the lines

on which a great deal can undoubtedly be done in individual cases. He rightly states that the practical problem is one not for the politician but for the architect, builder and engineer who should be called upon by public opinion to keep abreast of scientific research and utilise at once such knowledge as is already available, both for the lessening of noise production and for reducing its effect. In referring to certain investigations he draws attention to the work of Weston and Adams for the Industrial Health Research Board, who showed that in the face of very noisy conditions in industry the use of ear defenders leads to a diminution in fatigue as indicated by increased output. He points out that in the construction of buildings considerable success in noise reduction or prevention is possible, and urges that the need for this to be done is particularly great in hospitals.

The irritating and disturbing qualities of noise are connected with loudness, pitch, discontinuity, direction or lack of directional quality and the general background on which the noise in question is superposed.

No one who has read this book will disagree with the author when he states that the case for serious and organised attempts to reduce noise is overwhelming and the general impression he leaves is that if such serious attempts are made success will attend them.

G. P. C.

Manual Skill, its Organisation and Development. By J. W. Cox, D.Sc. The Cambridge Psychological Library. [Pp. xx + 247, with 53 figures.] (Cambridge: at the University Press, 1934. 16*s*. net.)

THIS work constitutes an attempt to rationalise the approach to the problems of vocational guidance, selection, and training. The author has carried out a very detailed investigation on original lines and deals in particular with an analysis of the psychological factors determining ability to perform "assembly work" which is so commonly met with in modern industry.

After a review of previous work on the subject in which he points out the gaps in our knowledge of the psychological processes underlying manual skill he emphasises the need for a systematic analysis of the operations involved in motor activity and proceeds to describe his method of investigation. He subdivides assembly work into two classes, namely work involving manual skill only, such as the straightforward routine manipulation of objects, and work in which the individual is called upon to apply his knowledge and use discrimination in the act of assembling the parts. The actual operation selected for the purpose of his experimental investigation was that of assembling the component parts and wiring an electric lamp holder. Tests for initial ability, effect of practice and the effect of training as distinct from practice were devised and carried out on a group of about 70 adults and 200 boys and girls from elementary schools. The analysis of the results of the tests showed that, in comparison with the effect on proficiency of the repetition involved in practice, a short period of training in the best methods of performing the operations not only facilitated and accelerated the acquirement of manual skill for the specific operations performed in the test, but enabled the subjects to approach other work with greater efficiency.

This book is a notable contribution to the science of vocational psychology and should prove of interest and of value not only to scientific investigators in the same field of research but to industrialists who interest themselves in the placing and training of new employees.

G. P. C.

BOOKS RECEIVED

(Publishers are requested to notify prices)

An Introduction to Projective Geometry. By L. N. G. Filon, C.B.E., M.A., D.Sc., F.R.S., Fellow of and Professor of Applied Mathematics at University College, London. Fourth edition. London: Edward Arnold & Co., 1935. (Pp. xviii + 407, with 79 figures.) 16*s*. net.

An Introduction to the Theory of Functions of a Complex Variable. By E. T. Copson, M.A.(Oxon.), D.Sc.(Edin.), Professor of Mathematics at University College, Dundee, in the University of St. Andrews, Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. (Pp. viii + 448, with 8 figures.) 25*s*. net.

A First Course in Differential Equations. By Norman Miller, Professor of Mathematics in Queen's University, Kingston, Canada. London: Oxford University Press, 1935. (Pp. viii + 148.) 7*s*. 6*d*. net.

The Collected Works of George Abram Miller. Vol. I. Urbana, Illinois: University of Illinois Press, 1935. (Pp. xii + 475.) $7.50.

Mathematics and the Question of Cosmic Mind, with Other Essays. By Cassius Jackson Keyser, Adrain Professor Emeritus of Mathematics, Columbia University. The Scripta Mathematica Library No. 2. Yeshiva College, New York: Scripta Mathematica, 1935. (Pp. vi + 121, with 1 plate.) 75 cents.

Le Pansoma et la Géométrie de l'Énergie. By Dr. A. C. Léemann. Geneva: Georg & Co., S.A., 1935. (Pp. viii + 257, with 18 figures.) Frs. suisses 15.

A Table of Eisenstein-reduced Positive Ternary Quadratic Forms of Determinant ≤ 200. By Burton W. Jones. Bulletin of the National Research Council, No. 97. Washington, D.C.: The National Research Council of The National Academy of Sciences, 1935. (Pp. 51.) $1.00.

Worlds Without End. By H. Spencer Jones, M.A., Sc.D., F.R.S., Astronomer Royal. London: The English Universities Press, Ltd., 1935. (Pp. xvi + 262, with 32 plates.) 5*s*. net.

An Introduction to Astronomy. By Robert H. Baker, Ph.D., Professor of Astronomy in the University of Illinois. London: Macmillan & Co., Ltd., 1935. (Pp. vi + 312, with frontispiece and 89 figures.) 12*s*. 6*d*. net.

The Solar System and its Origin. By Henry Norris Russell. New York and London: Macmillan & Co., Ltd., 1935. (Pp. x + 144, with 13 plates and 1 figure.) 8*s*. 6*d*. net.

The Dimensions and Structure of the Galaxy. Being the Halley Lecture delivered on 5 June, 1935. By J. S. Plaskett, C.B.E., F.R.S. Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. (Pp. 30, with 2 plates and 3 figures.) 2*s*. net.

The Design of Experiments. By R. A. Fisher, Sc.D., F.R.S., Galton Professor, University of London. Edinburgh and London: Oliver & Boyd, Ltd., 1935. (Pp. xii + 252, with 5 figures and 39 tables.) 12*s*. 6*d*. net.

Probability and Random Errors. By W. N. Bond, M.A., D.Sc., F.Inst.P., Lecturer in Physics in the University of Reading. London: Edward Arnold & Co., 1935. (Pp. viii + 141, with 16 figures.) 10*s*. 6*d*. net.

A Comprehensive Treatise on Practical Mechanics. An Introduction to Mechanical Science and its Practical Applications. By J. M. Lacey, M.Inst.C.E. London: The Technical Press, Ltd., 1935. (Pp. viii + 320, with 102 figures.) 18*s*. net.

Mechanical Properties of Matter. By S. G. Starling, B.Sc., A.R.C.Sc., F.Inst.P. London: Macmillan & Co., Ltd., 1935. (Pp. viii + 336, with 222 figures.) 6*s*.

Strength of Materials. By Edward R. Maurer and Morton O. Withey, Professors of Mechanics in the University of Wisconsin. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xii + 382, with 333 figures and 17 tables.) 17*s*. 6*d*. net.

A Textbook of Physics. Vol. V: Physics of the Atom. By E. Grimsehl. Edited by R. Tomaschek, D.Phil., Director of the Physical Institute, Technical College, Dresden. Authorised Translation from the Seventh German Edition by L. A. Woodward, B.A.(Oxon.), Ph.D.(Leipzig). London and Glasgow: Blackie & Son, Ltd., 1935. (Pp. xiv + 474, with 310 figures.) 17*s*. 6*d*. net.

Industrial Electronics. By F. H. Gulliksen, Member A.I.E.E., and E. H. Vedder, Member A.I.E.E. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 245, with frontispiece and 245 figures.) 17*s*. 6*d*. net.

Mercury Arc Rectifier Practice. By Frederick Charles Orchard, A.M.I.E.E. London: Chapman & Hall, Ltd., 1935. (Pp. xii + 224, with 106 figures, including 23 plates.) 15*s*. net.

Electrical Measurements in Principle and Practice. By H. Cobden Turner, M.I.E.E., M.I.Mech.E., and E. H. W. Banner, M.Sc., A.M.I.E.E., F.Inst.P. London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 354, with 219 figures and 1 folding plate.) 15*s*. net.

Impregnated Paper Insulation. The Inherent Electrical Properties. By J. B. Whitehead, Ph.D., Professor of Electrical Engineering, The Johns Hopkins University. National Research Council Committee on Electrical Insulation, Monograph No. IV. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 221, with 110 figures and 17 tables.) 20*s*. net.

Why the Weather? By Charles Franklin Brooks, Ph.D., Professor of Meteorology and Director of Blue Hill Observatory, Harvard Univer-

sity. With the collaboration of Eleanor Stabler Brooks and John Nelson. Revised and enlarged. London: Chapman & Hall, Ltd., 1935. (Pp. xviii + 295, with 52 figures, including 32 plates.) 10*s*. 6*d*. net.

The Brunner Focal Depth-Time-Distance Chart. A graphic chart for the determination of the focal depth, the time of occurrence, and the epicentral distance from the seismograms of a single station. By G. J. Brunner, S.J., and J. B. Macelwane, S.J., Department of Geophysics of Saint Louis University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 10*s*. net.

General Chemistry. An Elementary Survey, emphasising industrial applications of fundamental principles. By Horace G. Deming, Professor of Chemistry, University of Nebraska. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 778, with 170 figures.) 17*s*. 6*d*. net.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. Vol. XIV: Fe (Part III), Co. By J. W. Mellor, D.Sc., F.R.S. London, New York, Toronto: Longmans, Green & Co., 1935. (Pp. viii + 892, with 277 figures.) 63*s*. net.

Handbuch der Anorganischen Chemie. Abegg, Auerbach, Koppel. 4. Band, 3. Abtlg., 3. Teil, Lfg. 2. Edited by Dr. I. Koppel, Berlin. Leipzig: S. Hirzel, 1935. (Pp. xxvi + 848, with 194 figures.) RM. 30, paper covers.

The National Physical Laboratory Collected Researches. Vol. XXV: Metallurgy. London: H.M. Stationery Office, 1935. (Pp. v + 432, with numerous plates and figures.) 25*s*. net.

Fuel. Solid, Liquid and Gaseous. By J. S. S. Brame, C.B.E., F.I.C., F.C.S., and J. G. King, Ph.D., A.R.T.C., F.I.C. Fourth edition. London: Edward Arnold & Co., 1935. (Pp. xvi + 422, with 2 plates, 83 figures and 63 tables.) 25*s*. net.

The Structure of Metallic Coatings, Films and Surfaces. A General Discussion. Reprinted from the "Transactions of the Faraday Society." London and Edinburgh: Gurney & Jackson, for the Society. (Pp. 1043–1290, with 77 plates and numerous figures.) 21*s*. net.

The Quantum Theory of Valency. By W. G. Penney, A.R.C.S., M.A., Ph.D., 1851 Exhibitioner, Trinity College, Cambridge. Methuen's Monographs on Chemical Subjects. London: Methuen & Co., Ltd., 1935. (Pp. viii + 95, with 14 figures.) 2*s*. 6*d*. net.

Introduction to Quantum Mechanics. With Applications to Chemistry. By Linus Pauling, Ph.D., Sc.D., Professor of Chemistry, California Institute of Technology, and E. Bright Wilson, Jr., Ph.D., Society of Fellows, Harvard University. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xiv + 468, with 65 figures and 19 tables.) 30*s*. net.

Inorganic Colloid Chemistry. By Harry Boyer Weiser, Professor of Chemistry at The Rice Institute. Vol. II: The Hydrous Oxides and Hydroxides. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. viii + 429, with 70 figures.) 23*s*. 6*d*. net.

The Theory of Emulsions and Their Technical Treatment. By William Clayton, D.Sc., F.I.C., Chief Chemist and Bacteriologist, Messrs. Crosse & Blackwell, Ltd. (London). Third edition. London: J. & A. Churchill, Ltd., 1935. (Pp. x + 458, with 91 figures.) 25*s*. net.

Principles and Applications of Electro-chemistry. Vol. I: Principles. By H. Jermain Creighton, Professor of Chemistry, Swarthmore College. Third edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xviii + 502, with 84 figures and 124 tables.) 20*s*. net.

Physical Principles and Applications of Magnetochemistry. By S. S. Bhatnagar, M.Sc.(Panjab), D.Sc.(Lond.), F.Inst.P., University Professor of Physical Chemistry, and Director, University Chemical Laboratories, Lahore, and K. N. Mathur, M.Sc., D.Sc.(Allahabad), A.Inst.P., A.R.P.S., Lecturer in Physics, the University, Lucknow. London: Macmillan & Co., Ltd., 1935. (Pp. xiv + 375, with 58 figures and 1 folding plate.) 21*s*. net.

Principles of Experimental and Theoretical Electrochemistry. By Malcolm Dole, Ph.D., Assistant Professor of Chemistry, Northwestern University. International Chemical Series. New York and London: McGraw Hill Publishing Co., Ltd., 1935. (Pp. xiv + 549, with 158 figures and 90 tables.) 30*s*. net.

Physical Aspects of Organic Chemistry. By William A. Waters, M.A., Ph.D.(Cantab.), Lecturer in Chemistry in the University of Durham. With an Introduction by Professor T. Martin Lowry, C.B.E., D.Sc., F.R.S. Twentieth-Century Chemistry Series. London: George Routledge & Sons, Ltd., 1935. (Pp. xvi + 501, with 15 figures.) 25*s*. net.

The Systematic Identification of Organic Compounds. A Laboratory Manual. By Ralph L. Shriner and Reynold C. Fuson, Professors of Chemistry in the University of Illinois. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. x + 195, with 9 figures and 31 tables.) 11*s*. net.

Notes on Organic Chemistry. By F. Francis, D.Sc., Ph.D., F.I.C., Alfred Capper Pass Professor of Chemistry, University of Bristol. London: Edward Arnold & Co., 1935. (Pp. viii + 525.) 12*s*. 6*d*. net.

The Structure and Composition of Foods. Vol. II: Vegetables, Legumes, Fruits. By Andrew L. Winton, Ph.D., Sometime State and Federal Chemist, and Kate Barber Winton, Ph.D., Sometime State and Federal Microscopist. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 904, with 303 figures.) 75*s*. net.

Fundamentals of Biochemistry in relation to Human Physiology. By T. R. Parsons, B.Sc.(Lond.), M.A.(Cantab.), Sidney Sussex College, Cambridge. Fifth edition. Cambridge: W. Heffer & Sons, Ltd., 1935. (Pp. xii + 453, with 26 figures, including 1 coloured plate.) 10*s*. 6*d*. net.

The Chemistry of Rubber. By H. Freundlich, University College, London. Methuen's Monographs on Chemical Subjects. London: Methuen & Co., Ltd., 1935. (Pp. xii + 72, with 13 figures, including 3 plates.) 2*s*. 6*d*. net.

Forensic Chemistry and Scientific Criminal Investigation. By A. Lucas, O.B.E., F.I.C., formerly Director, Chemical Department, Egypt. Third edition. London: Edward Arnold & Co., 1935. (Pp. 376.) 18*s*. net.

Applied Chemistry. A Practical Handbook for Students of Household Science and Public Health. Vol. I: Water, Detergents, Textiles, Fuels, etc. By C. Kenneth Tinkler, D.Sc., F.I.C., Professor of Chemistry in the University of London, Head of the Chemistry Department, King's College of Household and Social Science, and Helen Masters, B.Sc. and King's College Diploma in Household Science, Head of the Domestic Science Department, Battersea Polytechnic. Third edition. London: The Technical Press, Ltd., 1935. (Pp. xii + 296, with 2 plates and 34 figures.) 15*s*. net.

Industrial and Manufacturing Chemistry. Part II, Inorganic. Vol. I. Edited by Geoffrey Martin, D.Sc., Ph.D., F.I.C. Fifth edition. London: The Technical Press, Ltd., 1935. (Pp. xx + 496, with 277 figures.) 28*s*. net.

The Teaching of Chemistry in the Universities of Aberdeen. By Alexander Findlay, Professor of Chemistry, University of Aberdeen. Aberdeen University Studies No. 112. Aberdeen: The University Press, 1935. (Pp. viii + 92, with 18 plates.) 5*s*. net.

Fluorescence Analysis in Ultra-Violet Light. By J. A. Radley, M.Sc., A.I.C., and Julius Grant, Ph.D., M.Sc., F.I.C. Being Vol. VII of a Series of Monographs on Applied Chemistry, under the Editorship of E. Howard Tripp, Ph.D. Second edition. London: Chapman & Hall, Ltd., 1935. (Pp. x + 326, with 28 plates, 17 figures and 31 tables.) 21*s*. net.

Analytical Chemistry. Vol. II: Quantitative Analysis. Based on the text of F. P. Treadwell, late Professor of Analytical Chemistry at the Polytechnic Institute of Zürich. Translated, enlarged and revised by William T. Hall, Associate Professor of Analytical Chemistry, Massachusetts Institute of Technology. Eighth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 858, with 153 figures and 24 tables.) 30*s*. net.

The Essentials of Qualitative Analysis. By A. Holderness, M.Sc., and John Lambert, M.Sc., Senior Chemistry Masters at Archbishop Holgate's Grammar School, York, and Woodhouse Grammar School, Sheffield, respectively. London: William Heinemann, Ltd., 1935. (Pp. viii + 72, with 6 figures.) 1*s*. 6*d*.

Tables for Qualitative Analysis. By F. Sherwood Taylor, Ph.D., M.A., B.Sc., Assistant Lecturer, Queen Mary College, University of London. London: William Heinemann, Ltd., 1935. (Cards I–XIV.) 1*s*. 3*d*.

Exercises in General Chemistry and Qualitative Analysis. By Horace G. Deming, Professor of Chemistry, University of Nebraska, and Saul B. Arenson, Associate Professor of Inorganic Chemistry, University of Cincinnati. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xvi + 326, with 25 figures.) 9*s*. net.

An Introduction to Physical Geology. With Special Reference to North America. By William J. Miller, Professor of Geology, University of California at Los Angeles, California. Third edition. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xii + 465, with frontispiece and 397 figures.) 15*s*. net.

Bulletin of the National Research Council, No. 98. Report of the Committee on Sedimentation 1932–1934. Prepared under the auspices of the Division of Geology and Geography, National Research Council. Washington, D.C.: The National Research Council of The National Academy of Sciences, 1935. (Pp. 246.) $1.00.

Historical Geology of the Antillean-Caribbean Region or The Lands Bordering the Gulf of Mexico and the Caribbean Sea. By Charles Schuchert, D.Sc., LL.D., Professor Emeritus of Paleontology in Yale University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xxvi + 811, with 123 figures, including 18 plates.) *50s.* net.

Tectonic Essays. Mainly Alpine. By E. B. Bailey, M.C., Lég. d'Hon., Cr. de Gu., M.A., F.R.S.L. & E., Professor of Geology, Glasgow University. Oxford: at the Clarendon Press; London: Humphrey Milford, 1935. (Pp. xii + 200, with 5 plates and 49 figures.) *12s. 6d.* net.

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SCIENCE PROGRESS

THE ULTIMATE STRENGTH OF METALS

By E. N. DA C. ANDRADE, D.Sc., Ph.D., F.R.S.
Quain Professor of Physics in the University of London, University College

Metals as we ordinarily know them are assemblages of very small crystals, and it is, apparently, to this fact that they owe their strength. It would be interesting, but it would lead us far from our subject, to consider if a fine polycrystalline structure always makes for strength—if, for instance, a granite is always stronger than a large single crystal of any of its constituents. Be this as it may, it is a fact of experience that single crystals of metals, which have been carefully handled, are in all cases very soft, and that, in general, the strength of a metal is greater the finer its crystalline structure. The view has been put forward that between the crystals of a metal exists a layer of metal in the amorphous state—"the intercrystalline cement"—to which the strength is attributed. There is little doubt now that a metal can exist in an amorphous state, as suggested long ago by Beilby. He based his belief on certain phenomena connected with the flow of the surface layers of metals during polishing, and the nature of the films so formed. The method of electron diffraction has definitely proved the amorphous nature of this polished layer.

This concerns conditions at the surface only. As regards the conditions inside a metal, it is clear that at the junction of crystals, arranged with their axes in random directions, there must exist a sheet of atoms which cannot strictly belong to either lattice, and it is probable that the conflict of directive forces, to speak in general terms, will extend for the distance of an atom or two, so that the layer appreciably departing from the stable lattice configuration may be not one, but a few atoms, thick. At the same time we must remember that the interatomic force dies off rapidly with distance, so that the disturbing boundary effect cannot extend very deep into the individual crystals. Therefore, on account of its extreme thinness, it seems unlikely that the amorphous layer

can be responsible for the strength, in the sense that the steel girder structure of a building is responsible for the strength, that is, because of the intrinsic toughness of the connected shell—this altogether apart from the fact that there is no real proof that metals in the amorphous state are particularly strong. We shall have to look elsewhere for the secret of polycrystalline strength.

The single crystal is, then, the brick of which metals are made without appreciable mortar, and it is with the properties of the single metallic crystal that we shall be mainly concerned in this article. Postponing for a moment the consideration of how such crystals can be prepared, and of their more detailed mechanical properties, we are met straight off with two fundamental facts which are so far not explained in a really satisfactory manner, in spite of the great amount of attention they have received. They are, firstly, that single crystals are very soft, and secondly, that any distortion, such as bending, tension, hammering or the like, makes them very much stronger. My lamented friend K. W. Hausser prepared single crystals of copper in the form of rods 6 mm. in diameter, which gave the dull sound of lead if dropped on the table, and which bent in the hands like lead. After a little such manipulation, however, such a rod became so hard that it could no longer be bent. A single crystal bar 16 mm. in diameter was easily bent a little, with difficulty bent into a semicircle, while an athlete could not bend it back into its original straightness. Such large specimens make sensational examples of the hardening effect of strain on single crystals, but the effect has been quantitatively established with ordinary single crystal wires. At atmospheric temperature the hardening effects are, with metals of similar crystal structure, the more pronounced the higher the melting point of the metal, although the rule has its exceptions. With nickel the effect is particularly marked (Fig. 1).

The weakness of the most perfect single crystals that we can make is the more remarkable in that theory indicates that regular crystals should be very strong. The only case that has been worked out theoretically in detail is that of a simple ionic crystal, such as sodium chloride, consisting of equal numbers of positively and negatively charged atoms (ions). For rock salt, theory indicates a strength, for pull along the axis of four-fold symmetry, of 20,000 kg.wt./cm.^{2}, which is about 1,000 times that observed experimentally under ordinary conditions. The case of crystals where all the atoms are electrically similar—the so-called non-polar type of structure—is much more difficult to treat mathematically, whether we are dealing with non-metallic crystals, such

as diamond, or, still worse, with metals, where the conduction electrons, of whose behaviour so little is still known, complicate matters. A very rough estimate of the kind of figure to be expected for the strength can, however, be made from general energy considerations, as pointed out by Polanyi [1]. When a rod, for instance, is extended to the breaking-point, two new surfaces are formed, and, supposing we assume that the surface energy of these new surfaces is equal to the elastic energy stored in the crystal at the

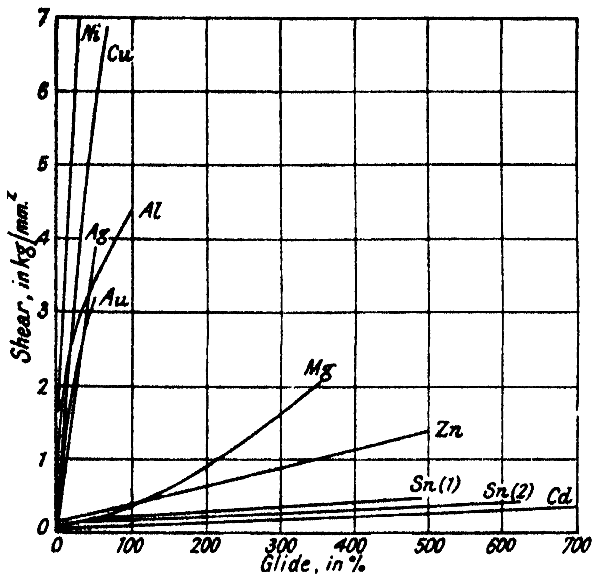


FIG. 1.—Hardening of single crystals with glide, for various metals (after E. Schmid).

moment of breaking (*i.e.* the work done stretching it to this point), we can write

$$\tfrac{1}{2}\mathrm{F}d = 2\gamma$$

where F is the breaking stress; d the extension, at breaking point, of a length equal to the intermolecular distance a; and γ the surface tension.

Further, if Hooke's law holds up to fracture and E is Young's modulus, then

$$\mathrm{F} = \frac{d}{a}\mathrm{E}$$

so that $$\mathrm{F}^2 \quad 4\gamma\mathrm{E}$$

Approximate estimates of the surface tension of solids can be made, by extrapolating from the liquid state. We are confirmed

in our opinion of the reliability of such estimates by the fact that in the case of simple ionic crystals the surface tension can be calculated, and agrees roughly with the values obtained from fused salts. The value of γ being known, the above formula gives us a value for F.

An estimate can also be made of the ultimate shear strength by considering that when one half of a perfect crystal is sheared over the other, in the direction z, the shear must vary periodically, approximately harmonically, as the rows of atoms are moved past one another, reaching zero at intervals equal to the interatomic spacing. Thus

$$\sigma = \sigma_0 \sin \frac{2\pi z}{a},$$

where σ_0 is the maximum shear

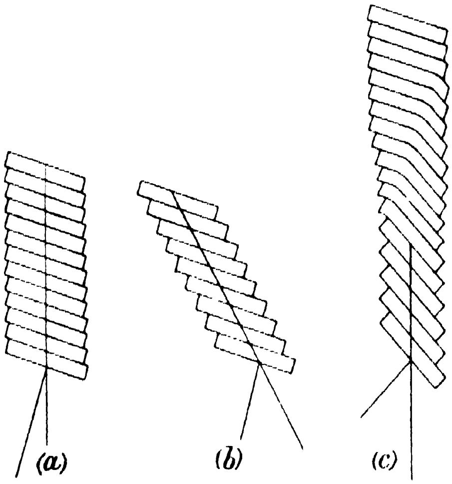
and $$\sigma = G\frac{z}{a}$$

where G is the shear modulus so that approximately, since $\frac{z}{a}$ is small,

$$\sigma_0 = \frac{G}{2\pi}$$

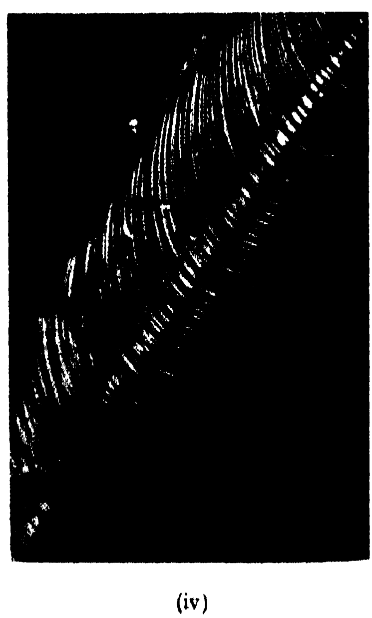
which gives us a numerical value. In the case of both tensile strength and shear strength the values obtained from these simple calculations (and, in the case of rock salt and such crystals, the more precise values obtained by much more elaborate methods) are about 1000 times as great as those measured experimentally. This discrepancy is the first of our great problems.

The first stage in experimental investigation of the two problems—weakness of the unstrained crystal, and hardening by strain—is clearly the preparation of unstrained crystals which shall give consistent results. Single crystals of metals—lead, tin and solid mercury—were prepared in 1914 [2], by simply heating wires of the metal above the melting point, and letting them cool slowly. The oxide film holds the wire together. Since then many other methods have been evolved—that of recrystallisation after plastic deformation, used by Sir Harold Carpenter and Miss Elam to prepare single crystals of aluminium [3]; that of crystallising molten metal contained in a tube from a point, used in a variety of forms by many workers [4]; that of withdrawing from the molten metal, contained in a crucible, a fine rod, on the point of which a crystal forms, used by Czochralski [5]. Some of these methods, at any rate, do not give very consistent results. Dr. Roscoe and I [6] have found that,



(i)


(iii)


(iv)

with soft metals, at any rate, the best results can be obtained by a method, which consists in drawing a small furnace, maintained at some degrees above the melting point, along an evacuated glass tube containing the metal wire. The diameter of the tube is about twice that of the wire, which is therefore not strained, while the curvature helps to maintain the molten metal in wire form. By suitably adjusting the temperature gradient and the velocity of the travel of the furnace along the tube, single crystal wires of any length can be obtained, which give more uniform results than have been obtained with crystals made by other methods. The reason seems to be that crystals prepared in this way are free from strain, which is clearly not the case with, *e.g.* crystals prepared with the metal tightly enclosed in a tube.

In general, when a single crystal wire is stressed beyond a certain limit, slip, or glide as it is generally called, takes place along a set of parallel planes, which make their appearance as elliptical traces on the surface of the wire, as was first shown in 1914 [2]. The conditions determining glide have been worked out by Polanyi, Schmid, G. I. Taylor and others [7]. These conditions are simplest in crystals of hexagonal structure, for in this case glide takes place preferentially on a set of parallel planes, the basic planes of the hexagon, which are unique in the crystal. On the other hand, with cubic crystals, for instance, there are four equivalent sets of (111) planes, which leads to complication, for, according to the direction of pull, glide may take place on two or more sets either simultaneously or successively. In the case of body-centred cubic crystals there are further complications. Taking hexagonal crystals then, not only does glide take place in one particular set of planes, but in one particular direction in those planes. This direction is one of the three digonal axes, the one, in fact, which is nearest to the direction of the tensile force. This slipping on a unique set of planes leads to peculiar flattening of the originally cylindrical wire: in the case of cadmium, where extensions of 500 per cent. are possible, the wire may become a flat ribbon.

In extension experiments carried out with wires, the two ends are held fast in clamps, while a load is applied in a fixed direction, which is that of the original wire. The glide planes in and near the clamped part of the wire remain, of course, fixed during the experiment. If glide took place throughout the whole length of the wire on a series of parallel planes, the axis of the wire would take up a new direction, as shown in Plate I (i), where (*a*) shows the wire before glide and (*b*) how it would be after glide, if simple glide had taken place on the same series of planes. This is, of

course, impossible when the pull is maintained in a fixed direction. We therefore have a region in which the planes are deformed, so as to keep the axis in a fixed direction, as shown in (c). Plate I (ii) is an actual photograph of a cadmium wire, showing the region of adaptation. In the main part of the wire therefore, the angle χ (see below) steadily decreases with increasing extension.

If a large number of crystals be prepared, it will be found, in general, that the least force required to cause glide to begin varies from crystal to crystal. This is because the angle between glide plane and axis of wire, and also between glide direction and axis of wire, varies from crystal to crystal. The simple law, due to Schmid [8], is that it is the component of the force resolved in the direction of glide, divided by the area of the surface of glide, that is the determining factor, or if χ is the angle which the glide plane makes initially with the axis of tension (axis of the wire), λ the angle which the glide direction makes with the axis (which may be the same as, and in any case is not very different from, χ), then the stress F (force per unit area normal to the axis) required to initiate glide is given by $S = F \cos \lambda \sin \chi$, where S is a constant known as the critical shear stress. The normal stress $N = F \sin^2 \chi$ is without influence on the glide, but determines brittle rupture, as originally discovered by Sohncke with rock salt as far back as 1869. N and F may vary in a different way with the temperature: Thus Roscoe and I have found that crystals of bismuth which are brittle at room temperature glide freely at temperatures near the melting point, because whereas at room temperature, over a range of values of χ, N reaches its critical value before S does, at high temperatures, with the same range of χ, S reaches its critical value before N does as F is increased.

The critical stress law has been roughly proved with the hexagonal metals by Boas and Schmid and others, but there were wide random divergencies even with crystals of one batch, while with cadmium, for instance, crystals prepared by Czochralski's method varied in behaviour according to the rate at which they were drawn [9]. Roscoe and I have found that crystals prepared by our method behave very consistently, and we have confirmed Schmid's law with considerable accuracy.

The critical shear stress having been reached in a plastic crystal, the crystal glides by a small given amount, and gradually comes to rest. To make it glide farther, the stress must be increased. The elastic recovery on unloading is very small, so that we may say we have a plastic deformation, in which hardening takes place with

increasing glide, glide being measured as the relative displacement of two parallel planes unit distance apart.

The observed effect is, however, complicated by the fact that, if a large stress be applied, the wire will flow plastically, but at a rate which steadily decreases. Again, owing to the rotation of the planes which accompanies extension there may be the so-called geometrical hardening, due to the fact that the resolved shear stress varies. If the angle χ is greater than 45° there will be a geometrical softening. Allowance can be made for this, however, by taking the shear stress resolved in the direction of glide. The type of curve we obtain may be illustrated by Fig. 2, which shows

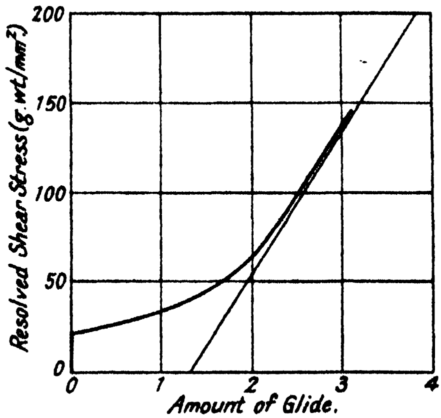


FIG. 2.—Hardening of a single crystal of cadmium with glide on one set of planes.

plastic flow of a single crystal of cadmium, gliding on one set of planes.

The elucidation of the connection between hardening and glide is made difficult by the fact that the hardening is not a function of the amount of glide, but also of the rate of glide. However, after considerable glide has taken place the rate seems to have a decreasingly small influence, which simplifies things, but a consideration of the details the laws of hardening would take us beyond our space.

Let us now return to the question of strength, as represented by the critical shear stress. The theoretical figure should be round about 5×10^5 gm.wt./mm.². The figures given by Schmid are of the order of 100 gm.wt./mm.², *e.g.* copper 100, silver 60, magnesium 83, zinc 94, cadmium 58 gm.wt./mm.² With very pure unstrained crystals, however, lower values are obtained, *e.g.* we have found 17 gm.wt./mm.² for very pure cadmium, and about

9 gm.wt./mm.^{2} for very pure mercury. Mr. Hutchings, Mr. Greenland and I have worked with single crystal wires of solid mercury (generally at about $-60°$ C.) because this metal can be obtained absolutely gas free and very pure—we estimate our impurities at about 1 in 10^8. This, we think, disposes of the suggestion, often made, that impurities or dissolved gas play a prominent part in the anomalous properties of metals, by causing irregularities in the lattice. Plate I, (iii) and (iv), represent single crystal wires of mercury, (iii) in the early stage of glide, and (iv) after considerable glide has taken place. The latter picture shows the nature of slip bands very well [9a].

The perfect lattice should, firstly, be very strong, and secondly, as Polanyi has pointed out, if the yield took place in the way to be expected from the ideal lattice, the applied forces and crystallographic forces should be in equilibrium at the moment where plastic deformation begins, and the elastic modulus should be zero, whereas Hooke's law holds roughly up to commencement of permanent deformation. On this view, then, the crystal yields prematurely, long before this stage is reached. Some hypothesis of a type which we may call "theory of the weakest link" is necessary. We have to consider separately the strength of a brittle crystal, and that of a plastic crystal, for the problem is clearly different in the two cases.

Some very important observations were made some years ago by Griffiths [10] in his work on the tensile strength of threads of quartz and glass. Griffiths found that freshly drawn threads were very much stronger than old threads, but that they gradually lost their strength, and came down to the normal low value. He assumed that the weakening took place by the formation of small surface cracks, which normally exist, but which are absent in a freshly drawn thread. Inglis [11] has pointed out that a crevasse in an elastic solid very much weakens that solid by virtue of the concentration of stress about the region of greatest curvature. For an elliptic hole in a plate, the tension τ_c in this region is given by

$$\tau_c = 2\tau\sqrt{c/\rho}$$

where τ is the tension a long way from the crevasse (or which would exist in the absence of the crevasse), of length $2c$, ρ being the radius of curvature. Clearly in the case of an atomic structure a lower limit for ρ is given by the interatomic distance a, so that if we suppose rupture to take place when the stress in the neighbourhood of the end of a submicroscopic crack reaches the theoret-

ical limit, then the actual measured breaking strength S is given by

$$F = \tfrac{1}{2}F_t\sqrt{\frac{a}{c}}$$

F_t being the theoretical strength. The exact value of the constant, here written $\frac{1}{2}$, is not important: other ways of deducing the formula, *e.g.* from energetic considerations, give slightly different results for it.

The weakening effect therefore depends on the depth of the crack, which can be worked out on the assumptions embodied in the above formula. For quartz glass and ordinary glass Griffiths found the value of 1 to 2 μ, which is a reasonable depth for these faults. For rock salt and metals, however, the values required for the depth of the crevasse or crack come out impossibly large, *i.e.* several millimetres. For zinc, for instance, we find $2a = 1{\cdot}1$ cm., which can scarcely be the case in a wire a millimetre in diameter. This is a difficulty which must now be considered.

We must remember that the theory really applies to brittle rupture only. If the material behaves plastically the whole basis of the theory is removed, since it presupposes a substance which behaves elastically. This can be shown with strips of paper and of celluloid. If a transverse crevasse be cut in such strips, and their strength be compared with strips of equal carrying breadth $b = b_c - 2c$ where b_c is the breadth of the pierced strip, the paper will be found to be much weakened, but the strength of celluloid is much the same. The paper tears from the sharply curved ends of the crevasse, but examination shows that in the celluloid tearing does not take place because the celluloid yields plastically in the region of greatest curvature, and relieves the strain.

Before we proceed to look into the quantitative behaviour of a crystal which glides—that is the plastic behaviour of a crystal—let us see if there is any direct experimental evidence for the existence of Griffiths cracks. Let me first quote an experiment of Orowan's concerning mica [12]. If a strip of this substance is loaded by means of clamps which extend across the whole width, so that the edges are stressed, the maximum strength is found to be about 3000 kg.wt./cm.^{2}. If, however, it is loaded by small clamps set near the middle of the sheet, so that the edges are stres-free, then the strength goes up to 32,000 kg.wt./cm.^{2}. The cleavage faces of mica are unusually perfect, but the cut edges are clearly full of small cracks: the experiment shows that if we avoid stress in the regions of trivial cracks the strength increases more than tenfold. When, in the crystal stressed with small clamps, cracks

do appear in the face, they run into the principle cleavage plane, and the crystal is saved, which seems to be against the theory of *Lockerstellen*, mentioned later, as a cause of weakness.

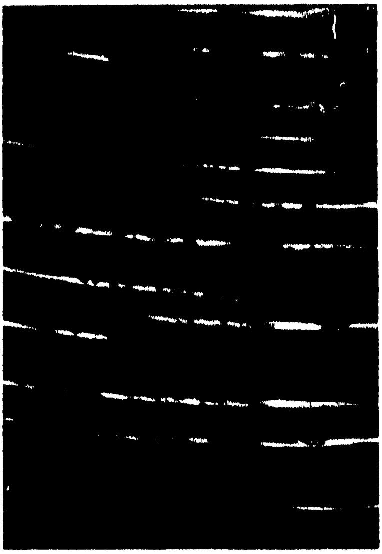
A direct proof of the existence of invisible surface cracks on apparently perfect surface of glass, quartz glass and other substances is furnished by some experiments carried out by Dr. Martindale and myself [13]. In these experiments a very thin film of metal, silver or gold, is deposited on the surface, and the substance is then maintained at a temperature in the region of 300° C. The thin metal begins to form minute crystals, which appear in well-defined lines on the surface, although the most searching methods of illumination fail to reveal any scratch, even at the highest magnification, as shown in Plate II, (ii), which represents silver crystallites on the surface of quartz glass, with very strong grazing illumination, at a magnification of $\times$ 1000. If the metal is then dissolved away, the surface carefully cleaned by the most rigorous methods, and a fresh metal film deposited and heated, the minute crystals form once more on the same lines. If, however, the surface be lightly polished, then the lines do not form, neither do they form on the very perfect cleavage faces of mica. The only explanation seems to be that the minute crystals are forming on submicroscopic surface cracks. This hypothesis is strongly confirmed by using surfaces of the two types of diamond described by Sir Robert Robertson, J. J. Fox and A. E. Martin [14]: one type is believed to possess a perfect structure, and the other a structure containing planes of misfit, or flaws. On the surface of the one type (Plate II, (iii)) the crystallites appear in random positions,[1] on the other (Plate II, (iv)) they form in lines.

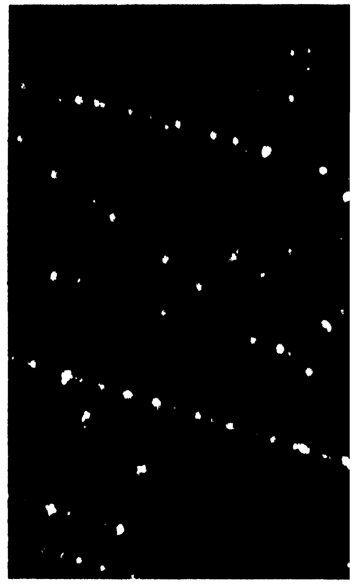
Slightly more indirect evidence of surface cracks is furnished by some experiments of Roscoe's [15]. He found that a very thin coat of oxide, a few tens of molecules thick only, increases the critical shear stress of cadmium wire to as much as double the normal value. The effect cannot be due to the strength of the layer of oxide, since this is far too thin, and further, the strength does not increase with the thickness of the layer, once a certain small thickness has been reached. It seems natural, therefore, to attribute it to the filling in of the sharp corners, if not of the whole, of the submicroscopic cracks by the oxide.

Griffiths' theory, then, can be adopted to give reasonable results for brittle rupture, but fails for plastic yield, which, for instance, takes place with single crystals of metal in general, and with rock

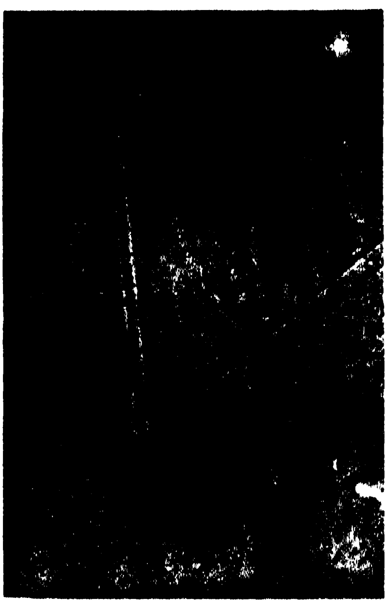
[1] The clear-cut triangles are surface pits, which are often found on natural crystal faces, and have nothing to do with the point under discussion.

PLATE





(i)



(iii)

salt. It fails because the crystal will not stand up until it is valid: it is like a boxer on whom his opponent's theory fails because he moves his face back, away from the glove.

We have to consider, then, the mechanism of glide and hardening. It has already been pointed out that a perfect crystal cannot glide, still more glide under small forces, and of recent years many types of imperfection have been put forward as representing the structure of real crystals. A theory that has attracted attention is that put forward by Zwicky and his collaborators [16], which states that the perfect crystal lattice is inherently unstable, or, more particularly, that at more or less regular intervals the spacing of the planes deviates from the normal value, by reason of thermodynamic necessity. To help us to understand Zwicky's ideas we may recall that the spacing in a three-dimensional lattice is different in the body of a crystal from what it is at a crystal boundary, as proved mathematically by Lennard-Jones [17]. Zwicky calls this postulated arrangement, in which the crystal is divided up into little blocks by a secondary lattice, or structure of differently spaced planes, occurring at intervals of some thousands of atoms, a mosaic structure. He has spent much ingenuity in defending his ideas, but it seems certain nowadays that the theory is untenable. In the first place Orowan has shown that the mathematical reasoning by which it was sought to establish the instability is invalid, and in the second place, while many crystals show surface marks of various kinds spaced at distances of the order of 10^{-4} cm., which is about the size demanded by Zwicky for his mosaic blocks, Renninger has been able to demonstrate that there are rock salt crystals which are perfect, which disposes of the theory of essential instability.

Although a mosaic structure, in Zwicky's sense, cannot be sustained, there is no doubt that perfect crystals are exceptional, and that all crystals which are ordinarily the subject of experiment contain flaws of some kind. The matter has not been made any easier by the multiplicity of terms that have been used in discussing these faults, sometimes without any very clear definition. We have the *Lockerstruktur* (or loose-structure) of Smekal [18], the lineage structure of M. J. Bürger [19], and the terms block structure and secondary structure which have been fairly widely used. As long ago as 1914 C. G. Darwin [20] pointed out that the intensity of X-ray reflection could only be explained by a slight variation of the orientation of the crystal surface from place to place, so that the crystal face is something like a parquet floor in which the different blocks have not been set quite flat. Bürger has shown

that when a crystal grows there is a slight branching here and there, so that small differences of orientation take place. There seems no reason why such structural imperfections as these should weaken the crystal—in fact, from analogy with polycrystalline mass, they might strengthen it.

"Lockerstruktur" is rather a vague term and I have not been able to find any very precise definition as to what is implied by it. The "looseness" may, apparently, be anything from a Griffiths crack to the change of lattice between "blocks" in the hypothetical block structure. If the looseness is an actual gap, then it is only effective if it is a long thin gap like the Griffiths crack: a spherical or cylindrical cavity produces a comparatively small increase of stress in its neighbourhood. Another looseness is that contemplated by Smekal in his theory that a real crystal consists of blocks of ideal crystal loosely held together. Not only is there no definite experimental evidence for this structure, but, as Orowan has pointed out, it would imply, since the surface of the wire, say, consists of the surface of blocks, that the surface energy should bear to the theoretical surface energy the same proportion as the experimental strength bears to the theoretical strength. While the methods of estimating the surface energy of solids are approximate only, yet they could not fail to reveal an effect of this magnitude.

The most promising conception of the mechanism of glide seems to be that based on quite a different kind of irregularity. As Orowan has pointed out, while with rupture the surface energy increases, which needs a supply of energy, with glide there is, apart from hardening, only a slight change of energy. This is consistent with an irregularity, *i.e.* a deviation from the ideal structure, being propagated along a glide plane leaving everything behind it much as it was, except for an advance of one part of the crystal relatively to the other by an atomic spacing. The same general type of irregularity, capable of fulfilling this condition, has been put forward independently by Polanyi [21], G. I. Taylor [22] and Orowan [23]. It is suggested that, somehow or other, within a certain space an atom is missing from the normal spacing, so that there are, for instance, nine atoms against ten. We might call this a "vernier spot," or a "place of misfit." The position is differently represented by different workers; Polanyi and Orowan picture the state of affairs much as shown in Fig. 3, while G. I. Taylor, who has given the most precise presentation of the mechanism of glide, prefers to have one atom space completely blank, and to represent the blank space as becoming less and less in successive rows until we have normal spacing once again, with a

corresponding slip of one atom at the edge (Fig. 4). He calls such a place a dislocation, and distinguishes two sorts, according as the "healing" takes place in one direction or the other. These he calls positive and negative, but it must be understood that this

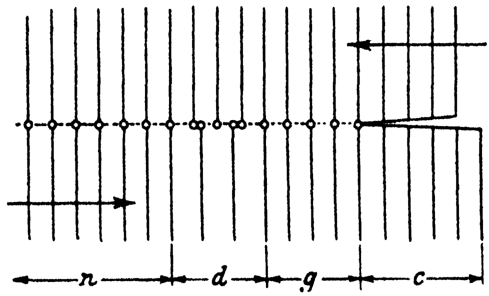


FIG. 3.—Dislocation propagated from a minute crack, as pictured by Orowan.

merely distinguishes opposed kinds: if a single dislocation is pointed out no one can say if it is positive or negative, but once one has been named, the others are defined for that particular crystal. Each vernier spot, or dislocation, is clearly surrounded

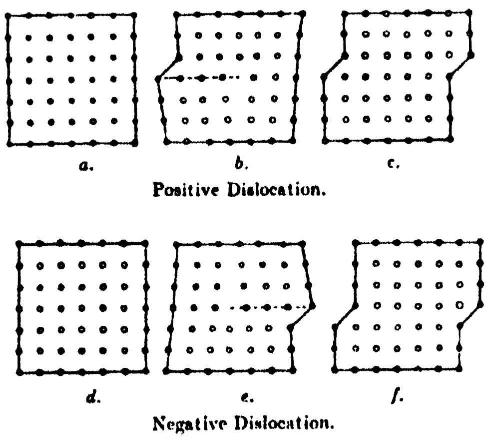


FIG. 4.—G. I. Taylor's picture of the propagation of a dislocation.

by a field of stress. It can be shown that when a stress is applied, the dislocation runs along the plane, and over the distance through which it has travelled the whole crystal above the plane traversed is stepped by an atomic spacing relative to that below the plane, as represented in Fig. 4. How the dislocations originally arise is not known, but we may regard them as frozen temperature fluctuations, or frozen Brownian movements.

Taylor's theory is constructed to explain shear hardening. The

elastic forces round like dislocations repel, while unlike dislocations attract. Consider dislocations in neighbouring planes. Under an external force the unlike dislocations, which would tend to stop opposite one another, can be drawn past one another, and if the separation of the neighbouring planes exceeds a certain distance, the force being maintained, the dislocations escape from one another. The force required is given by $\frac{G\lambda}{2\pi h}$, where h is the distance between the paths of the two dislocations, normal to their direction of run, and λ is the atomic distance, G the rigidity modulus. The nearer the paths of the dislocations, the greater the force required to produce yield. Taylor assumes a more or less regular distribution of dislocations, and considers a displacement of the positive lattice relative to the negative one. He further assumes that the distance which a given dislocation can run is limited by a system of flaws, at average distance L from one another. From these assumptions he arrives at a parabolic hardening law, $\tau = k_1\sqrt{N}$, where N is the number of dislocations, which is proportional to h^{-2}, and k_1 is a constant. Assuming further that the shear strain is proportional to the number of dislocations we get $\tau = k_2\sqrt{s}$, where s is the glide. This assumes implicitly that extra dislocations can appear under extra stress, since N acts as a parameter. It is possible to get an estimate of the order of magnitude of the separation of the flaws which hold up the run of the dislocations. It comes out to be $1 - 5\mu$, which is of the order of certain spacing marks observed on actual crystals.

The theory is extremely elegant, but admittedly makes some rather artificial assumptions, chosen for ease of mathematical handling. It scarcely attempts to give an account of certain of the most striking phenomena of actual glide, in particular of the formation of definite slip bands. The nature of the flaws which have to be postulated to limit the passage of the dislocations is not very specific, and it is difficult to form a picture of them which shall comply with observation by letting the dislocations pass more easily the higher the temperature. Again, in the particular form put forward by Taylor, it gives a parabolic law of hardening, which holds for certain metals only.

Orowan's view of glide [23] is somewhat different, and mathematically less precise than Taylor's. He attributes an essential part to surface cracks, and supposes that, in a loaded crystal, the effect of a sharp edge of a crack, combined with thermal fluctuations, is to produce momentarily a very high local stress, of the order of the theoretical stress necessary to produce yield in a

perfect lattice, although the mean actual stress may be a very small fraction of this. Local glide then takes place, producing a dislocation in the way shown in Fig. 3. This dislocation is then propagated much in the way described by Taylor. Whereas in brittle rupture the crack itself grows, in plastic glide a dislocation runs from its edge, leaving the crack much as it was, but producing a displacement of all the crystal above it in the diagram by one atomic spacing to the left. The high local stress may attack closely neighbouring planes, or more than one dislocation may travel, at intervals, along the same plane.

It seems easy to apply Orowan's ideas to the glide which produces the actual slip bands observed. I cannot help thinking that these slip bands have a fundamental importance. As Roscoe and I have shown with lead, their spacing follows a probability law, as is perhaps only to be expected, but the mean separation is to a great extent a characteristic of the metal, being independent of the rate of extension, of the diameter of the wire, of the amount of extension and of the temperature within certain limits. The separation of the slip bands is about 4×10^{-4} cm., which incidentally is of the order calculated by Taylor for the free runs of his dislocations. I would like, therefore, to be able to modify Taylor's theory so as to allow the dislocations to run right across the metal, from side to side. In this connection the behaviour of slip bands in lead, illustrated in Plate II (i), is suggestive, as the reluctance of close bands to pass one another is strongly reminiscent of the theory. This leaves, however, the difficulty of hardening. It seems possible to found a theory of hardening on the basis of an important fact which has been established by W. G. Burgers [24], that shearing of an aluminium crystal distorts the planes of the lattice, in such a way as can be explained if the planes of slip are not ideal lattice planes, but have local irregularities where portions of the crystal are, as it were, broken off and rotated under the shear. It is natural to suppose that such fragments will exert a blocking action. A similar distortion on the glide planes has been established with polarised light in the case of rock salt, and the actual passage of the distortion has been watched. The picture of a mathematical dislocation running along a perfect plane seems too ideal! If the natural unflawed crystal is hard, but contains planes of easy flow, across which dislocations can run, then glide on these planes will be made more difficult by crystalline fragments torn off and rotated in the way just described. The dislocation may start at surface cracks, as suggested by Orowan, so that what we require, in this case, is a distribution of more or less

regularly arranged surface cracks, spaced at a few times 10^{-4} cm. apart, all of about equal size with some metals, but varying very much in size with others, since in some metals the stress-strain relation follows a linear law, while with others the law is parabolic. A hardening by "block rotation" will explain very easily how it is that glide on one set of crystal planes hardens the crystal for glide on another set.

The matter obviously cannot be adequately discussed here, but it may be mentioned that with aluminium fresh slip bands appear as the force increases, while with certain other metals, lead and cadmium for instance, after an initial slip all the glide takes place on the bands already formed. A linear law of hardening *on each slip band* will then give a parabolic law for aluminium and a linear law for the other metals quoted, which agrees with experiment.

Finally, we may consider what must be, from the technical point of view, our ultimate object, how to explain the properties of ordinary crystalline metals in terms of the properties of single crystal. Here little more than a first beginning has been made. An interesting fact is that for very fine wires or threads of certain substances—glass, quartz, selenium, resin—the ultimate tensile strength is much greater than it is for the substance in bulk. These substances are amorphous: the effect has been established for fine crystal threads in the case of the inevitable rock salt, but not for metals, except in the case of antimony threads. Orowan interprets this to mean that the depth of the Griffiths cracks must diminish with the linear dimensions of the specimen, and points out that the effect commences when the specimen is of the order of the depth of the crack estimated for large specimens, *e.g.* at 5μ with quartz, where the depth of crack is 1–2μ. If the origin of the cracks is some kind of strain in the material the diminution of depth with size can be understood.

Let us now turn to ordinary metals. We know (*a*) that the polycrystal is much stronger than the single crystal; (*b*) that the finer the grain the stronger the polycrystal. We have a general explanation of this if we consider that the crystalline cracks are held up at the crystalline boundary, as, in fact, we have seen they are with mica. Orowan [25] concludes, from general considerations, that the strength should be inversely proportioned to the square root of the average linear dimensions of the grains. As the calculation assumes no plastic flow, we can only expect it to be valid for brittle rupture. There are some experiments of Masing and Polanyi [26] on zinc at liquid air temperature which agree as well with Orowan's formula as can be expected in view of the

approximate nature of the measurements. As regards the comparative toughness of the polycrystalline metal, when it comes to plastic flow it is clear that the random arrangement of adjacent crystallites, with their glide planes in different directions, leads to a jamming which renders glide difficult. Nevertheless, a certain amount of glide can take place, although, no doubt, there may have to be a bending of the crystal planes in the course of the adaptation. I showed, many years ago, that if we apply constant stress, we can produce a flow which gradually diminishes to a constant value, and put forward the idea that in the early stage of the flow we have a rotation of the crystallites, coupled with a certain amount of glide, and in the last stages a pure glide. This view has been strengthened by some experiments which I carried out with Dr. Chalmers on the electrical resistivity of metal wires at various stages of the plastic flow [27]. We found that the resistivity of the cubic metals, copper and aluminium, for which the crystallites have the same resistance in all directions, was unaffected by the flow. With metals crystallising with a unique axis of symmetry there was no change of resistance during the final stage of steady flow, but, on the other hand, the resistivity changed by as much as 2 per cent. during the intermediate steps, decreasing with cadmium (hexagonal), for which the glide planes are normal to unique axis of symmetry, and increasing with tin (tetragonal), for which the glide planes are parallel to the unique axis. In both cases the resistance, in single crystals, is a maximum in the direction of the unique axis. The results can therefore be explained by a general rotation of the planes in the crystallites so as to tend to lie in the direction of the applied force. The changes of resistance are of the right magnitude for this explanation. More recently Dr. Gibbs and Ramlal [28] have established a rotation of the crystallites by direct X-ray methods, of the magnitude to be expected from the electrical resistance. This is apparently the first attempt to calculate quantitatively the properties of the polycrystal from the single crystal.

We have now considered a few of the fundamental problems of metallic strength and of yield under stress. It is possible that I may have done nothing but produce the impression that the matter is a very complex one. That, I am afraid, is a correct impression, but I think that the work that is going on, in particular that of Orowan, is slowly leading us to a better understanding. I wish that I could have set down a few clear conceptions with the aid of which all the properties of metal single crystals—let alone polycrystalline metals—could be simply expressed. The trouble

is that we have not yet reached those conceptions, and it is no good pretending that we have. But I, for one, am not without hope that the time is not far distant when we shall have a tolerable idea of what happens, from first to last, when we perform the simple experiment, which most of us have carried out from boyhood's days, of bending a piece of wire backwards and forwards until it breaks.

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THE CONTROL OF DIFFERENTIATION[1]

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THE form of organisms and the differentiation of their parts are seldom directly explicable on the basis of cause and effect. The following, however, may be instanced as belonging to this category:

The spherical form of many minute organisms is the reaction of fluid, or semi-fluid, material to its own surface tension. Here we think of cocci, Palmelline cells—forms assumed when physiological contractions are relaxed. Ova and spores when unconstrained by containing structures are further cellular examples.

The spherical form of vacuoles when undistorted by local intracellular restrictions. Chambers' and Höflers' beautiful microdissection experiments on the tonoplast of *Allium cepa* illustrate diagrammatically the reaction of a uniform coherent substance to the diffusive forces of contained solutes.

In this connection may be recalled Lloyds' observations on the development of contractile vacuoles in conjugating Spirogyra cells. Here contraction of the protoplast leaves it unsupported by the cell-wall, and the osmotic pressure of the solutes distends the individual vacuoles till they burst and allow the contained solutions to escape into the surrounding water. Then the elastic recoil of the protoplasm heals the rupture, and the solutes, once more enclosed, beginning to push back the semi-permeable protoplasm, generate vacuoles afresh and cause explosive ruptures in repetitional sequence. In this process we have a rare example of the form and functional changes of a part produced and maintained as the direct reaction of known configurations to demonstrable physical forces. It may be assumed that this is the underlying mechanism of all contractile, or pulsating, vacuoles.

In the great majority of cases, however, we can associate certain applications of energy only more or less indirectly with observed developments of form and differentiation, or with certain

[1] A paper read before the Section of Morphology of the Sixth International Congress, Amsterdam, 1935.

liberations of energy in organisms. The intermediate steps, connecting the causes and effects, are slightly—usually very slightly—understood. The study of such phenomena embraces that of the control of differentiation.

Advance in this subject has been slow—not more slow than the complexity of the substances and processes involved should have led us to expect. But it must be admitted that much work has still to be done to reveal fully this complexity, and before the great unifying principles begin to become apparent, which will give us the explanation which we seek. Meanwhile we gratefully admit our colleagues have accomplished much in laying the foundation of a new knowledge. To quote Spemann's words, "The first step has been achieved. We still stand in the presence of riddles, but not without hope of solving them. And riddles with the hope of solution—What more can a man of science desire?"

The quest is charged with allurement. Each headland and island of acquired knowledge, described and charted by the navigators of the unknown, inspires those who follow in their wake with confidence and hope. Naturally deductions and speculations by the leaders in discovery carry most weight. The results and conclusions of Professor Bower have done much to lay bare the principles governing the form of organisms and the laws controlling their development. How increasing size necessitates alteration in the form and in the anatomy of plants has been his theme. He has elucidated it with a remarkable wealth of illustration, drawn chiefly from the Pteridophytes. Among many other interesting results he has shown that the sequence protostele, siphonostele, dialystele, eustele, has been passed through in several distinct lines of descent. Each of these features must, at least once in the phylogeny, have been represented by factors, or genes, in the germplasm, which at some time became changed so that the factor, or factors, determining protostely then became factors of siphonostely. Evidently the change from protostely to siphonostely involving only the omission of the lignification of certain cells, requires a change in probably only a single factor of the germplasm. Similarly the change from siphonostely to dialystely may be ascribed to a second simple germplasmic mutation. Assuming, as I believe we are entitled to assume, from the work of Muller and others, that these changes of the germplasm are perfectly random changes, probably often entirely due to the fortuitous action of short-wave cosmic radiation on the atomic structure of the germplasm, it is easy to see that the probability of the occurrence of a single change in the germplasm, commanding the production of a

viable or advantageous character, is much greater than the coincidental production of two or more such changes. It is due to this principle that although mutations are fortuitous, nature usually presents us with an orderly series of slightly differing forms, connecting the most diverging extremes ; and to it is traceable the appearance of homoplasy which Bower has pointed out in so many of the Pteridophyte phyla. Because, starting with the same germplasm, there is a greater probability that a given divergence will be attained by the accumulation of unitary fortuitous steps, than by the coincidental occurrence of an equal number of viable unitary changes. By the same principle the late arrival of the dendrostele (as we may designate the type of central cylinder characteristic of exogenous trees), with its congeries of interdependent characteristics, becomes intelligible. Furthermore, natural selection itself favours the occurrence of instances of parallel (homoplastic) modifications in more or less closely related lines of descent. For the possession of common modifications adapting the possessors to certain conditions, rules out many other modifications and so limits the possible channels of variation.

Bower's studies have brought before us many illuminating examples illustrating the application of the principle connecting size and form to the vascular system of the Pteridophytes. This is sometimes expressed as the relationship of surface to volume—a form of statement which applies to the size and form of those organs and parts whose surfaces are permeable, and function in the interchange of material, and possibly in the transmission of energy. Sometimes, however, this form of statement is meaningless—for example when it is applied to those vascular systems of the Pteridophytes which are embedded in impervious vascular sheaths. Here the medullation and fragmentation of the vascular system must be conditioned, not by the relation of the surface of the conducting cords to their volume, but by the maximum distance by which the lifeless conducting elements may be separated from living cells. It was the coincidental production of the several independent modifications, securing this relationship in the primary and secondary arrangement of tissues, which delayed the advent of the dendrostele.

In these and similar discussions it is, strictly speaking, erroneous to invoke Natural Selection as the cause of phylogenetic differentiation. The cause is radiation, possibly assisted by other unknown factors, producing changes in the germplasm.

It is equally intriguing to speculate on the mechanisms of ontogenetic differentiation. Contrary to what we might at first

anticipate, it has been shown, and D'Arcy Thompson has emphasised the fact, that the cell-divisions occurring in the embryo do not control the growth of its parts. Actual observation shows that growth precedes cell-division, and the direction of growth controls cell-division. The forces, available for extending the constituent cells of the growing regions, are osmotic pressure and imbibition, while the form of the growing part seems to be determined by the distribution of growth substances, whose nature and action have been so wonderfully elucidated by the work of the Dutch botanists.

The controlling action of cell-growth on the orientation of cell-division may probably be traced to the more rigid consistency which characterises the protoplasm of the mitotic spindle as compared with that of the rest of the cell-protoplasm. Such a semi-rigid elongate body will tend, owing to the movements of the cytoplasm, to take up a longitudinal direction in an elongating cell. Hence the following cell-division will be transverse to the elongation. It may well be that the early stages of enlargement determine a flow of mitosis-evoking hormone, such as was predicated by Haberlandt and others. The action of such a substance initiating mitosis is indicated by my own observation of mitosis occurring in the cœnocytic stage of the embryo-sac. Here the nuclei uniformly distributed in the film of cytoplasm lining the sac are often seen in the successive stages of mitosis—the earlier stages being further from the chalaza on both sides of the sac—a distribution which would follow the diffusion outwards from the chalaza of a mitotic hormone. Incidentally it may be noticed that these mitotic figures being unconstrained by the proximity of cell-walls are fortuitously oriented in the film lining the sac; but the axes of all, being constrained by the thickness of the film, lie in a tangential plane.

The differentiation of the first organs of the Pteridophyte embryo seems determined by the first division of the zygote. Microdissectional work and experimental investigation combining artificial nutrition with vital staining are very desirable to decide whether the local predetermination is traceable to the distribution of materials in the cytoplasm, or to differential mitosis occurring in the zygote. Analogy with the animal ovum seems to favour the former and the records of vegetative reproduction from mature tissues are against the latter. It seems necessary to admit, at least as a general rule, that mitosis secures that each cell receives a complete germplasm—which, given the necessary opportunities, can give rise to a complete individual. It is however certain that, in any given cell, *all* these possibilities are never realised, and hence

there must be in its nucleus numbers of genes, associated with morphological and physiological properties, which never find expression. At the same time other genes, in the same nucleus, make their presence felt in moulding the structure and metabolism of the cell. If speculation is permissible, we may suppose that active genes—together with their co-enzymes or accessory substances—are displayed upon the nuclear filament, while the latent genes or their activating substances are imprisoned in the nucleolus. The usual solution of the nucleolus, the distribution of its material on the chromosomes and the whole process of mitosis secure the allotment of complete sets of genes with their activators to the daughter cells. However this may be, the enormous surface exposed by one nuclear constituent (chromatin) contrasted with the minimal one of the other (nucleolar material) is provocative of speculation.

Among the possible agents controlling early differentiation we must admit the directed forces of gravity and light, together with localised supplies of hormones and of nutrients, administered by the surrounding tissues of the parent. Morphogenetic stimuli, too, may early become effective. But as soon as this early differentiation is under way, we must further expect the young tissues themselves to become new environmental agents, and to act as controllers of differentiation. The procambium once differentiated will probably precipitate procambium from the undifferentiated meristem, and the protophlœm and protoxylem will induce the formation of these tissues from the procambium. Similar functions in differentiation may be assigned to other differentiated tissues in contact with the growing points of the plant. So far as I know, the probability of the differentiated tissues in contact with the undifferentiated cells of the meristems, acting as "Organisers" in Spemann's sense, has hitherto not been considered; and yet this view offers an attractive working hypothesis explaining the differentiation of tissues at the growing apices and their continuity throughout the plant.

MINERAL TRANSFORMATIONS, AND THEIR EQUATIONS

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In the customary lists of "modes of alteration" appended to formal descriptions of mineral species prominence is given to mineral changes which are mainly of the retrograde (breakdown or destructive) kind, such as the commonplace changes biotite ⟶ chlorite, olivine ⟶ serpentine, orthoclase ⟶ sericite or kaolin. But a large volume of fact is on record concerning the reverse kind of change—that of aggradation, illustrated for example by the progressive metamorphism of a clay-slate, which may be aggraded to successive schist-facies "indexed" for metamorphic "grade" by chlorite, biotite, garnet, staurolite and kyanite: each index-mineral evolves partly at the expense of antecedent species, final terms having a very complex genealogy.

The study of evolutionary processes, in both the igneous and the metamorphic fields, is the distinctive feature of modern petrology, which seeks to interpret the mechanism of mineral-change, to determine the degree of genetic relationship (if any) between various rock-types in which mineral-transformations have played an important rôle, and to establish the origins of magma. Its ultimate problems rest in the complex mineral changes which occur on the vague border-line between the igneous and the metamorphic fields—notably those induced in crustal rocks by emanations and exudates from magma.

Most of us would frankly include among our early difficulties that of appreciating, at sight, the terms of an equation of change—the precise nature of reactions engaging somewhat complex "molecules." The reality of "*atomic*," as distinct from "molecular," reactivity is an outstanding fact established by X-ray studies of crystal-structure: atoms, not molecules, are the movable units in crystal-architecture, and the varietalism displayed by pyroxenes, amphiboles, micas, etc., expresses the effects of atomic interplay. The extent to which the results of X-ray work illumine and expand

Bowen's reaction-principle may be usefully examined here—as a preliminary step to this discussion of mineral-changes in general and of equations formulated to explain them.

Atomic Activity—in Relation to Bowen's Reaction-Series

1. *The "Continuous-Reaction" (Plagioclase Series).*[1]—The actual mechanism of the reaction is the replacement of atomic Ca by atomic Na, and of atomic Al by atomic Si, *i.e.* (NaSi) $\rightarrow$ (CaAl), while the initial pattern, or "class," of crystal-structure persists unchanged. It may be expressed thus:

$$\begin{array}{ll} & \text{Albite} \longleftarrow \text{Anorthite (the continuous change).} \\ \text{Replacement:} & \text{(NaSi)} \longrightarrow \text{(CaAl).} \end{array}$$

Both the initial and the final terms conform to the three-dimensional space-lattice $n(SiO_2)$; for convenience, let $n = 4$.

In the case of anorthite, $4(SiO_2)$ is expanded to (Si_4O_8), then to $(Si_2Al_2)O_8$, to express the fact that one-half of the Si-atoms are already replaced by Al-atoms. The derivative group $(Si_2Al_2)O_8$ now offers two free oxygen-valencies, whereby Ca″ is attached, and the anorthite-formula is therefore $Ca(Al_2Si_2)O_8$ which is more informative and more accurate than the empirical "molecular" formula $CaO.Al_2O_3.2SiO_2$.

For albite, in which only one-fourth of the Si-atoms are replaced by Al-atoms, the constitutional formula is $Na(AlSi_3)O_8$.

The fact that tetravalent silicon, Si^{IV}, replaces trivalent aluminium, Al^{III}, and Na^{I} replaces Ca^{II}, illustrates the control exercised by relative atomic volumes (at. weight/Sp. Gr.), or ion-radii—measured in Å-units. For example, the atomic volume of sodium (23·7) renders it more compatible with calcium (26·0) than with potassium (45·5). The atomic volumes of Al (10·6) and Si (11·4) are similarly compatible. Briefly expressed, the case becomes

	Albite (NaSi)	$\longleftarrow$ $\longrightarrow$	Anorthite (CaAl)
At. vols:	23·7 + 11·4		26·0 + 10·6
Total:	35·1		36·6
Ion.-rad.:	1·00 + 0·40		1·05 + 0·55
Total:	1·40		1·60

These considerations have an obvious bearing on a miscellany of relationships between the various felspars.

2. *The "Discontinuous-Reaction" Series.*[1]—The real "discontinuity" consists in the abrupt change in crystal-structure which, from high-temperature to low-temperature (in the general case), is elaborated step by step from "simple" to "complex":

[1] N. L. Bowen, *The Evolution of the Igneous Rocks*, 1928, p. 60.

At first :	simple :—the unit-tetrahedron, (SiO_4) class.	*e.g.*	Olivine.
Then :	The single chain, with (SiO_3)-groups.	„	Pyroxenes.
Next :	The double band, with (Si_4O_{11})-groups.	„	Amphiboles.
Finally :	The extended sheet, with (Si_4O_{10})-groups.	„	Micas.

To the sheet-class belong sericite, chlorite, serpentine, kaolin, etc., which are typically the degradation-products of all but the most refractory minerals. Destructive (retrograde) changes converge, via such sheet-minerals, towards "clay" and, in the ultimate, laterite or bauxite.

The terms "degradation" and "aggradation" are used in the sense suggested by the relative status of the pyrogenic minerals in Bowen's reaction-series and of the index-minerals to metamorphic grades.

The "discontinuous" reaction-series affords scope for elaborate interplay between both (*a*) atoms individually : $Mg \rightleftharpoons (Fe \rightleftharpoons Mn)$, $(Cr \rightleftharpoons Fe''') \rightleftharpoons Al$, $(K \rightleftharpoons Na) \rightleftharpoons Ca$, $(Mg \rightleftharpoons Al \rightleftharpoons Li)$, etc., and (*b*) atom-associations : $(NaAl) \rightarrow (CaMg)$ concerned in the change actinolite $\longrightarrow$ glaucophane ; $(CaAl) \rightarrow (MgSi)$ concerned in the progressive aggradation of hornblende in certain epidiorites, *vide* p. 623. To a large extent, atomic dimension fixes the limit to which some particular replacement can proceed without involving an abrupt change in the class of crystal-structure. The effects achieved, and their bearing on varietalism in mineral species, may be illustrated by an example taken from the pyroxenes—for which the "class"-group is $n(SiO_3)$, and $n = 2$:

The group $2(SiO_3)$, or (Si_2O_6), could be modified by atomic reactivity to $(Si_mAl_nFe'''_p)O_6$, in which case $m + n + p = 2$, and the number of free oxygen-valencies $= 12 - (4m + 3n + 3p)$ attaching bases according to the general scheme $(Ca_aMg_bFe''_cMn_dAl_eFe'''_fTi_g \ . \ . \ .)$. It follows that m, n, p, a, b . . . are usually fractional.

An interesting comparative study is afforded by three genetically related augites in the rock-suite of Scawt Hill, where assimilation of lime by (A) olivine-dolerite magma led to the formation of (B) pyroxene-rich dolerites and pyroxenites, passing into (C) titan-augite-rocks, with nephelite, melilite, etc.[1]

The pyroxene of (A)[2] is a feebly aluminous diopside-rich member of the enstatite-diopside series—varietally in contrast with the pyroxenes of (B)[3] and (C),[4] which are themselves in contrast; thus :

(B) $(Ca_{.936}Mg_{.809}Fe''_{.178}Ti_{.035}Al_{.004}Fe'''_{.035})(Si_{1.9}Al_{.10})O_6$.
(C) $(Na_{.030}Ca_{1.063}Mg_{.491}Fe''_{.278}Ti_{.124}Fe'''_{.037})(Si_{1.698}Al_{.256}Fe'''_{.046})O_6$.

The contrast between (B) and (C) is due to atomic interplay according to the scheme $(CaFe''TiFe''') \rightarrow Mg$ and $(AlFe''') \rightarrow Si$,

[1] C. E. Tilley and H. F. Harwood, "The Dolerite-Chalk Contact of Scawt Hill, Co. Antrim," *Min. Mag.*, 1931, **22**, 439–68.
[2] *Loc. cit.*, p. 449. [3] *Loc. cit.*, p. 451. [4] *Loc. cit.*, p. 454.

with additive Na-atoms, *i.e.* an expansion of the more generalised reaction (CaAl) → (MgSi).

It will be obvious that the interpretative value of X-ray work in petrogenetic studies must depend upon adequately detailed analyses—not merely of rocks in bulk and their conspicuous por-

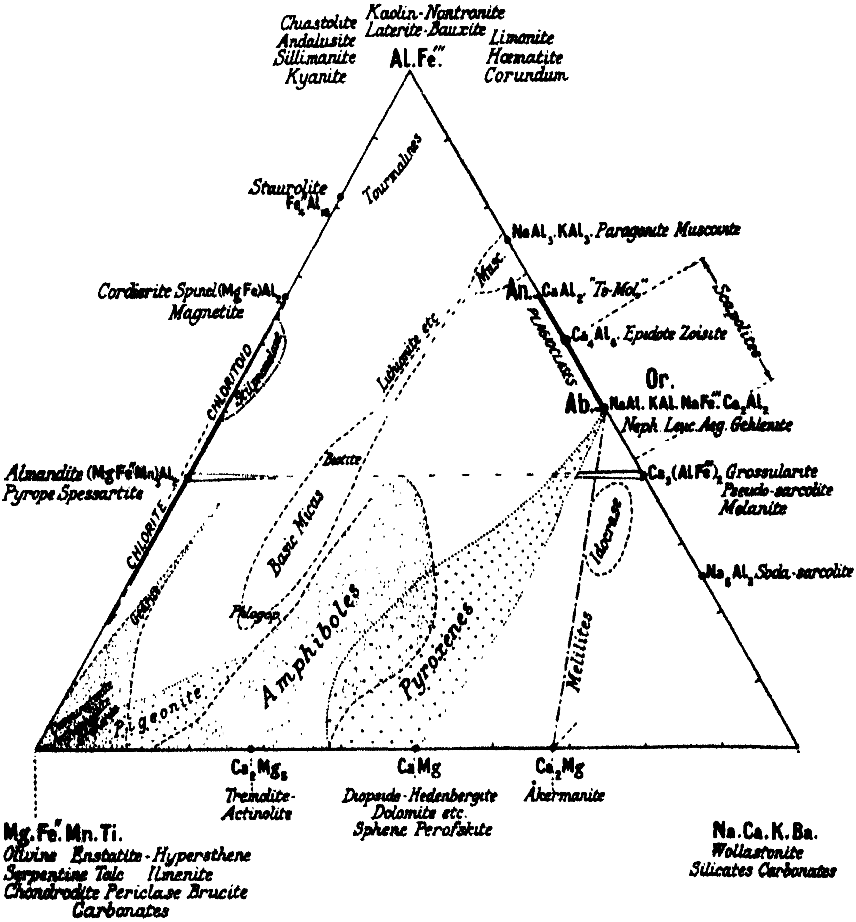

FIG. 1.—Showing the composition-fields for the chief rock-forming minerals. Some hundreds of carefully selected analyses are plotted on an *atomic* basis (in conformity with the results of X-ray work), silicon, phosphorus, hydrogen, etc., being "in reserve."

phyritic crystals but also of the more minute groundmass minerals, which require for their isolation both labour and technique. Lack of analytical data for granular pyroxenes such as occur for example in the groundmass of many mica-lamprophyres must cast some doubt upon the reality of the break in the pyroxene-field shown in Fig. 1.

Equations of Change

The terms of an equation of change can often be inferred from a study of the change in its various "arrest" stages, which show relics of minerals belonging to the initial reaction-system ($R_1 + R_2 + \ldots$, and summated in ΣR) intimately associated with reaction-products (summated in ΣP). The balanced equation $\Sigma R = \Sigma P$ requires that both ΣR and ΣP shall be the exact and complete census.

But balanced equations which reach the standard familiar in Chemistry are practicable—at present—for only the simpler kinds of change and parts of more complex changes. The precise composition of terms recognised may not be known; other terms may escape recognition, while also doubt may exist concerning both the source of particular reactive substances and the fate of other substances mobilised by the change, *e.g.* K-atoms mobilised when potash-felspar in pegmatite is albitised. The limitations imposed in the general case may be appreciated by examining even the commonplace change biotite $\longrightarrow$ chlorite, for which a first expansion would be: biotite, by virtue of gains (ΣG, which includes water) and losses (ΣL, including alkali), yields products (ΣP) which include chlorite; whence

$$\text{Biotite} + \Sigma G - \Sigma L = \text{Chlorite} + \ldots$$

By treating "gains" as reactive terms promoting the change and including "losses" among the products of the change, we obtain the reversible equation:

$$\text{Biotite} + \Sigma G = \text{Chlorite} + \Sigma L + \ldots$$

Pursuing the possibilities for further expansion: no precise formula can be assigned to either the biotite or the chlorite; only by a remarkable chance would the composition of any two occurrences of either be identical. Moreover, the adjustment of a biotite-composition to that of a chlorite usually disturbs initial ratios such as Al : Fe‴, Mg : Fe″, Si : Al, Si : Ti, etc., which change towards new equilibria involving subsidiary reactions liable to be telescoped in the bleak general equation. An expansion of ΣP for a particular occurrence of altered biotite in a Dartmoor granite for example is as follows:

$$\Sigma P = \text{Chlorite}\begin{cases}(\longrightarrow \text{kaolin} \longrightarrow \text{muscovite})\\(\longrightarrow \text{tourmaline} + \text{anatase}) + \text{rutile-needles} (\longrightarrow \text{rutile grains})\end{cases}$$
$$+ \text{sphene} + \text{magnetite} (\longrightarrow \text{limonite}).$$

A further selection of mineral-transformations will now be examined by the aid of Fig. 1, which may assist the reader to appreciate equations formulated by researchers who have actually

investigated the transformations in detail. It is perhaps unnecessary to state that Fig. 1 offers neither ready-made explanations for any change demanding detailed research nor security for offhand explanations which may be consistent with it.

Equations expressing the effects of dry fusion present comparatively simple cases :

1.—In Fig. 1 several lines could be drawn connecting "magnetite" with various "augite"-points and also traversing the hornblende-field. "Hornblende" thus integrates "augite" and "magnetite"—silica, with a passive rôle, being in reserve. It is therefore not unexpected that fused hornblende, on cooling, does yield products according to the equation :

$$\text{Hornblende} = \text{Augite} + \text{Magnetite} + \Sigma P,$$

in which ΣP includes some glass and the volatile constituents of the hornblende.

Similarly, under appropriate conditions, actinolite would be expected to yield diopside and orthopyroxene. Incongruent meltings provide additional examples.

2.—The KAl/Olivine-line crosses the tip of the phlogopite-field ; hence the products of fused phlogopite could conceivably include leucite and olivine.

Fouqué and Michel-Levy fused together microcline and biotite, and recognised in the cooled product leucite, olivine, and magnetite. For the equation of change, Holmes [1] suggests the equation :

$$\text{Biotite} + 3KAlSi_3O_8 = 11KAlSi_2O_6 + 5Mg_2SiO_4 + 2Fe_3O_4 + 2H_2O.$$

The biotite-composition given by Holmes may be integrated thus : $H_4K_8(Fe_5Mg_{10})(Al_8Fe''')Si_{18}O_{76}$, whence its locus in Fig. 1 is determinable directly ; it makes a close approach to the biotite-field.

The biotite-locus could be determined also by manipulating the terms on the right-hand side of the equation, thus :

11 Leucite + 5 Olivine contribute (11K + 11Al + 10Mg)-atoms assembled at a point P. Compounding the 32 atoms at P with the 6 atoms contributed by $2Fe_3O_4$ and located at the "magnetite"-point, we obtain a point R, which therefore assembles the whole 38 atoms. Hence R must also integrate 3 Orthoclase + Biotite. The total orthoclase contributes 6 atoms ; therefore the biotite must account for the balance, 32 ; whence the biotite-locus is fixed.

The biotite-formula usually given in this connection is

[1] A. Holmes, *Petrographic Methods and Calculations*, 1921, p. 409.

$H_2K_2Mg_6Al_2Si_6O_{22}$, which locates a point in the amphibole-field and is inadmissible on other grounds.

3.—Another familiar but inadmissible text-book equation is that put forward to express the reversible reaction investigated by Doelter :

$$\text{Anorthite} + \text{Olivine} = \text{Augite}$$
$$CaAl_2Si_2O_8 + Mg_2SiO_4 = CaMgSi_2O_6.MgAl_2SiO_6.$$

A glance at Fig. 1—or even at the particularised equation itself—is sufficient to detect the unlikelihood of this formulation, which would need revision if the " augite " is to make a reasonably close approach to some natural augite-occurrence—richer in Ca-atoms and poorer in (AlFe''')-atoms.

No analyses of natural pyroxenes fall within the field about the locus of " actinolite " : the break in the continuity of the enstatite-diopside series is the immiscibility-gap explored by Kennedy.[1] It seems probable that, as Kennedy suggests, any pyroxene-composition falling within this gap would imply an intergrowth of two independent pyroxenes.

The position of " actinolite " within this gap recalls that of " epidote " in the middle of the plagioclase-range. Should the suspected discontinuity in the plagioclase-series (within the Sp. Gr.-range 2·667–2·698) be confirmed, it may reduce several genetic difficulties raised by epidote, zoisite, etc.

4.—The following progressive changes are familiar :

i.—Augite ⟶ Hornblende ⟶ Biotite ⟶ Chlorite.
ii.—Augite ⟶ Chlorite.
iii.— Hornblende ⟶ Chlorite.

Change of the first (calcic) pair towards the second (non-calcic) pair in (i) commonly evolves epidote and some coloured variety of sphene. There is as yet no certainty concerning the source of either the Ca-atoms trapped in the epidote or the (AlFe''')-atoms required for transformation in the general sense augite ⟶ biotite. The Ca-atoms may be derived, in part, from antecedent augite or hornblende ; but both the Ca-atoms and the (AlFe''')-atoms could be the products of some change affecting the lime-rich felspars, *e.g.* :

i.—Anorthite + ΣR = Epidote + Al-atoms + Σp.
or ii.—The continuous reaction (NaSi) → (CaAl) in the plagioclase-series.

5.—An instructive study in the progressive aggradation of hornblende is provided by Wiseman's work on the epidiorites of the

[1] W. Q. Kennedy, *Min. Mag.*, 1935, **24**, 206.

South-West Highlands of Scotland.[1] Initial stages in this metamorphic change could be formulated thus:

i.—Anorthite + ΣR = Al-atoms + Epidote + Sphene + Σp.
ii.—Augite ($\pm$ Olivine) + Al-atoms $\longrightarrow$ Low-grade hornblende + Prochlorite.

The relationship of this low-grade hornblende (chlorite-zone) to a higher-grade hornblende in the garnet-zone, and also to actinolite, is explored below:

A. $(OH)_2\ Ca_2\ (Mg,Fe'')_5\ (Si_8O_{22})$ . . Actinolite
B. $(OH)_2(Na,Ca,K)_{2.09}(Mg,Fe'',Ti,Mn,Fe''')_{5.08}(Si,Al)_8O_{22}$. Low-grade Hornblende
C. $(OH)_2(Na,Ca,K)_{2.2}(Mg,Fe'',Ti,Mn,Fe''',Al)_{4.9}(Si,Al)_8O_{22}$. High-grade ,,

The really significant details of the change are revealed by the further expansion of the constitutions B and C:

B. $(OH)_{2.02}(Na_{.27}Ca_{1.73}K_{.09})\ (Mg_{2.85}Fe''_{1.67}Ti_{.05}Mn_{.03}Fe'''_{.53})\ (Si_{7.61}Al_{.41})O_{22}$
C. $(OH)_{2.02}(Na_{.39}Ca_{1.67}K_{.15})\ (Mg_{1.58}Fe''_{2.22}Ti_{.14}Mn_{.03}Fe'''_{.33}Al_{.56})\ (Si_{7.16}Al_{.84})O_{22}$

whence the scheme of atomic reactivity is as follows:

Tremolite $\longrightarrow$ Actinolite $\longrightarrow$ Low-grade Hornblende $\longrightarrow$ High-grade Hornblende.
($Fe'' \rightarrow Mg$) initiated $\rightarrow$
($Ti,Mn,Fe''' \rightarrow Mg$) initiated $\rightarrow$
($NaK \rightarrow Ca$) ,, Na additive.
($Al \rightarrow Si$) ,,

6.—The following reaction is familiar:

Anorthite-rich plagioclase + ΣR = Epidote (zoisite) + Albite + Σp

Under certain aggrading conditions it is reversed. But changes which are observable in some "mixed" rock-facies in the Malverns include:

i.—Epidote (zoisite) $\longrightarrow$ Oligoclase-andesine (enclosing hæmatite).

The anorthite-poor plagioclase evolved in this case could be explained by the reaction (NaSi) $\rightarrow$ (CaAl) operative on both epidote and quartz—the latter being, in this environment, a vanishing term.

ii.—Sillimanite + ΣR = Sericite ($\longrightarrow$ Felspar).
iii.—Garnet + ΣR = Chlorite + ΣP.
Chlorite + ΣR = A kaolin-species + ΣP (including Fe,Mg,Mn lost).
Kaolin + ΣR = Muscovite.
Muscovite + ΣR = Felspar.

In many cases, the changes in (iii) are telescoped in the apparently direct change garnet $\longrightarrow$ muscovite, or garnet $\longrightarrow$ felspar.

7.—Complex mineral-transformations involving silication, desili-

[1] J. D. H. Wiseman, *Quar. Jour. Geol. Soc.*, 1934, **90**, 354–417.

cation, and fractional resorption are well exemplified by those investigated by Tilley in the Scawt Hill rock-suite [1] and cited above (p. 618). Step by step, the changes can be followed in Fig. 1.

(*a*) *Facies-change: Olivine-dolerite* (*A*) ⟶ *Pyroxenite* (*B*).

This is characterised by a reduction of olivine and, to a less extent initially, of plagioclase ($Ab_{35}An_{65}$)—compensated, in part, by progressive increase in pyroxene, which is the new variety B (p. 618). The steps in the change may be formulated thus:

i.—Olivine + Silica = Enstatite.
$Mg_2SiO_4 + SiO_2 = 2MgSiO_3$.

ii.—Lime + Silica = Wollastonite.
$2CaO + 2SiO_2 = 2CaSiO_3$.

iii.—2Enstatite + 2Wollastonite = 2Diopside.
$2MgSiO_3 + 2CaSiO_3 = 2CaMg(SiO_3)_2$.

Summating these three equations:

iv.—$Mg_2SiO_4 + 3SiO_2 + 2CaO = 2CaMg(SiO_3)_2$.

The additional silica, $3SiO_2$, required in equation (iv) is derived from albite, which is *desilicated* by lime to nephelite:

v.—1·5 Albite ($NaAlSi_3O_8$) = 1·5 Nephelite ($NaAlSiO_4$) + $3SiO_2$.

Summating equations (iv) and (v)—while doubling the quantities to avoid fractional indices, we obtain the equation given by Tilley (*loc. cit.*, p. 461):

vi.—$3NaAlSi_3O_8 + 4CaO + 2Mg_2SiO_4 \longrightarrow 4CaMg(SiO_3)_2 + 3NaAlSiO_4$.

The albite desilicated is provided by some portion (solid or liquid) of the plagioclase proper to the olivine-dolerite. The anorthite thus released is also desilicated, by lime, to the Wahl-silicate, or "Tschermak-molecule":

vii.—$CaAl_2Si_2O_8 + CaO = CaAl_2SiO_6 + CaSiO_3$.

The diopside of (vi), and the Wahl-silicate of (vii), enter the new pyroxene-variety B (p. 618 above). The nephelite contributes to the zeolite thomsonite.

(*b*) *The facies-change* (*A*) ⟶ (*B*) evolves a liquid differentiate enriched in iron and soda, which normally gives rise to a rock containing yet another distinctive augite-variety, and a new plagioclase. Where this liquid incorporates more lime, the facies changes in the sense augite-plagioclase rock ⟶ titanaugite-nephelite rock:

$$\text{Plagioclase}\begin{cases}\text{Albite} \longrightarrow \text{Nephelite (equation vi).}\\ \text{Anorthite} \longrightarrow \text{Wahl-silicate (equation vii).}\end{cases}$$

viii.—Augite B (p. 618) + Wahl-silicate + ΣR = Titanaugite (C, p. 618).

[1] *Loc. cit.*

(c) *With excess lime the formation of melilite begins*—at the expense of both the titanaugite and the plagioclase (actual or potential):

$$\text{ix.—Titanaugite}\begin{cases}\text{Diopside} \longrightarrow \text{Akermanite (see Fig. 1).}\\ \text{Wahl-silicate} \longrightarrow \text{Pseudo-sarcolite (empirically}\\ \quad\text{“grossularite).}\end{cases}$$

$$\text{x. Plagioclase}\begin{cases}\text{Albite } (\longrightarrow \text{Nephelite}) \longrightarrow \text{Soda-sarcolite.}\\ \text{Anorthite} \longrightarrow \text{Pseudo-sarcolite}\\ \qquad\qquad\qquad + \text{Gehlenite.}\end{cases}$$

$$\text{xi.—Akermanite} + \text{Soda-sarcolite} + \text{Gehlenite} + \Sigma R = \text{Melilite.}$$

8.—Stages in the evolution of the index-minerals to the metamorphic grades in the pelitic schists provide the following interesting studies: [1]

i.—Chlorite ⟶ Biotite.

K/Chlorite-lines traverse the well-defined field of basic micas, and serve to interpret the following equation:

$$\text{Chlorite} + \text{K-atoms} + \Sigma r = \text{Biotite} + \Sigma p.$$

Σr could include iron-oxides and rutile-needles.

The source of the K-atoms may be uncertain; they could be liberated by the breakdown: Sericite ⟶ K-atoms + some kaolin-mineral.

The K/Chloritoid-line traverses a rather dubious part of the biotite-field, and skirts the restricted stilpnomelane-field.

ii.—The Genesis of Almandite.

Almandite could arise directly from chlorite: its locus in Fig. 1 is within the chlorite-range.

According to one standard equation, garnet and orthoclase originate at the expense of biotite and silica. A line drawn from "orthoclase" to "almandite" in Fig. 1 passes through the biotite-field, and graphically expresses the equation:

$$\text{Biotite} + \Sigma R \text{ (including silica)} = \text{Orthoclase} + \text{Almandite} + \Sigma p.$$

But Tilley observes that, as orthoclase does not usually arise at this stage, the origin of almandite from biotite is less typical of the almandite-zone proper than of the high-grade pelites.

iii.—The Genesis of Staurolite.

Chloritoid is regarded by Tilley [2] as one of the principal sources of staurolite. Any chloritoid initiated in the chlorite-zone would tend to adjust its composition to the new conditions, while further

[1] C. E. Tilley, "Some Mineralogical Transformations in Crystalline Schists," *Min. Mag.*, 1926, **21**, 34–46.

[2] *Loc. cit.*, p. 45.

growth would probably be at the expense of the polar terms kaolin and iron-oxides (Fig. 1). All these higher-grade schists could be degraded to sericite-chlorite-quartz schists—liable to contain relics of staurolite, garnet, etc., as clues to the double metamorphism.

Fig. 1 assists also in interpreting the metamorphic differentiation whereby complementary "spots," e.g. embryonic chlorite (cordierite, chloritoid) and chiastolite, are achieved in low-grade contact-altered pelites.[1]

9.—*The influence of pelitic slates, etc., on the genesis of norite-magma* has been investigated by Read,[2] who shows that, in proportion to increment of reactive alumina derived from the slates, etc., gabbroic magma tends to precipitate orthopyroxene in place of augite.

Bowen's reaction-principle entails the corollary that a magma precipitating, say, olivine and labradorite is undersaturated with respect to later terms in the respective reaction-series. The magma could therefore dissolve such terms—of which chlorite, sericite, and free silica (the constituents of slates, etc.) are the common alteration-products. If these products can be, justifiably, included among the "late" terms liable to solution in gabbroic magma, then the case lends itself to reasoning on first principles.

Actual solution of chlorite and sericite would augment the reactive concentration of atoms proper to each of the three poles in Fig. 1. The increased concentration of Al-atoms in particular would tend to inhibit the formation of the non-aluminous compound diopside, $CaMg(SiO_3)_2$, in favour of enstatite, $MgSiO_3$, and anorthite, $CaAl_2Si_2O_8$. The evolutionary route:

Olivine ⟶ Enstatite ⟶ Enstatite-diopside ⟶ Augite

would therefore be blocked at "enstatite."

Consideration of Fig. 1 shows that cordierite, spinels, biotite, and even orthoclase could be evolved by subsidiary reactions engaging the polar terms.

10.—Much has been written to sustain the claim that simple alkali-aluminates are among the free emanations (or distillates, or exudates) from high-temperature magma. As desilicating agents, they would probably be more potent than the nephelite-rich liquor suggested by Bowen[3] as the agent whereby diopside (or augite) is disintegrated—to yield natural melilite, according to the equation:

Diopside (augite) ⟶ Monticellite (or forsterite) + Akermanite (melilite).

Perhaps the extreme case of desilication is that of the change

[1] Author's Thesis (Imp. Coll. Sc.), 1921.

[2] H. H. Read, "Corundum-Spinel Xenoliths in Gabbro," *Geol. Mag.*, 1931, **10**, 446–53.

[3] N. L. Bowen, "The Evolution of the Igneous Rocks," 1928, Chap. XIV.

silica $\longrightarrow$ felspar. To achieve alkali-felspars, this change would require either (*a*) an initial reaction Al $\rightarrow$ Si—in the presence of reactive (KNa)-atoms, or (*b*) the direct reaction (KNa)Al $\rightarrow$ Si. The change is essentially a "syenitisation" process—in contrast with the mechanism (NaSi) $\rightarrow$ (CaAl) characterising evolution towards granite. The best evidence available rests in the analytical and field data for the Newry rock-suite described by Miss D. L. Reynolds,[1] who shows that "syenitisation" is an important process in the complex changes whereby, for example, the following genetic relationships were established:

$$\begin{tikzcd} \text{Biotite-pyroxenite} \arrow[r] & \text{Shonkinitic monzonite} \arrow[dr] & \\ & & \text{Syenite.} \\ \text{Peridotite} \arrow[uu] \arrow[r] & \text{Olivine-monzonite} \arrow[ur] & \end{tikzcd}$$

In the passage: Peridotite $\longrightarrow$ *Biotite-pyroxenite*, the atomic ratio Alks./Ca changes from 1 : 12 to nearly 6 : 12, and the ratio Al/(Alks. + Ca) changes from 3 : 12 to 6·3 : 12—while the RO/SiO_2 ratio remains almost constant. These statistic details alone suggest that *the biotite-pyroxenite is essentially the peridotite locally enriched in its own emanation*—migrant alkali-aluminate, dominantly potassic.

It is this kind of emanation which most satisfactorily accounts for the demonstrable "syenitisation" of the environing sediments (shales and greywackes)—in advance of actual penetration by mobile fractions of the peridotite-magma and biotite-pyroxenite magma *diffusing independently*. Where these independent magma-fractions gained access to the country-rocks, the "mixed" derivative rock-types betray the identity of the invader in a most significant way: their porphyroblastic augites and biotites repeat the varietalism peculiar to these species in either one, or the other, of the ultrabasic rocks. This fact is of fundamental importance.

The passage Peridotite (or biotite-pyroxenite) $\longrightarrow$ *Augite-biotite-diorite* is regarded by Miss Reynolds as due to syntexis of the initial terms with a plagioclase-rich magma. Though Miss Reynolds has entertained the conception of an *independent* plagioclase-magma, it seems at least possible that mobile plagioclase-rich fluid could originate as a by-product of the complex reactions induced by emanations—perhaps an important witness to their reality and effectiveness.

The changes recognised by Miss Reynolds—and still under close investigation—are among the "less familiar" types which impose on us the task of probing still deeper into the natural history of even the commonest rock-forming minerals.

[1] D. L. Reynolds, "The Eastern end of the Newry Igneous Complex," *Quar. Jour. Geol. Soc.*, 1934, **90**, 585–636.

BIRDS DO ATTACK BUTTERFLIES

By G. D. HALE CARPENTER, D.M.

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THIS journal, in October, 1935 (**30**, 272–7), contained an article by L. Richmond Wheeler entitled "Do birds attack butterflies ?" This urges a view held by the school of Professor MacBride which, not agreeing with the view that Mimicry can be ascribed to Natural Selection, derives support from the statement that a particular set of cases—those among butterflies—cannot be due to the action of birds as selective agents.

The argument rests upon (1) the importance of the fact that very many observers have seen very many birds not eating butterflies ; and (2) the decrying of positive evidence that birds do attack them. The force of this argument is weakened by the necessity of treating mimicry among butterflies as an isolated phenomenon, whereas much of the strength of the argument from natural selection lies in the treatment of mimicry of all kinds, among all insects, as merely one form of protective resemblance to a part of the environment and therefore of the same order as resemblance to a dead leaf or stone.

Mr. Wheeler still further narrows the foundations of his argument by introducing a new limitation. He states that while the current theory of mimicry "may apply to the *under wing* colours of certain butterflies", "as applied to the upper wing colours it is unsound." This is presumably the "new view," but it can be easily shown to be a *reductio ad absurdum.*

Certain mimics, such as the forms *hobleyi* and *tirikensis* of the polymorphic Nymphaline *Pseudacraea eurytus*, closely resemble on both surfaces the male and female of the Acraeine *Bematistes* (=*Planema*) *macarista.* The patterns of the two surfaces of both model and mimic, however, are different : Mr. Wheeler would seek an explanation for the deceptive likeness of the upper surface different from that which "may apply" to the under surface. The situation becomes more peculiar if a group with the same pattern on both surfaces be considered, such as the association

centred round the bluish-white and black Oriental Danaines such as those figured in Mr. Wheeler's reference 12.

Here is an appearance which, when on the under surface, can be explained, but when on the upper surface is inexplicable. This discrimination implies that a butterfly is seen *either* by its upper *or* by its under side. Cannot it appear in both ways to the same enemy ? Has Mr. Wheeler never seen butterflies sitting on mud, flowers, or leaves, alternately opening and closing their wings so that the upper and under surfaces are both visible even though the insects are stationary ?

The fact is stressed that Bates (and others of the same era) have not *recorded* (the italics are the writer's) one " attack in which either bird or butterfly was identified." Surely the very fact that Bates noted specifically that he never saw the flocks of slow-flying Heliconidae in the woods persecuted by birds implies that here was a difference to be noted : otherwise, why record that birds did not eat a particular kind of butterfly if they never ate any at all ?

Mr. Wheeler's allusion to " examples carefully collected from as many parts of the world as possible " is an insinuation that cases are so rare as to be negligible. One can only regret that he is not able to see the material in the Oxford University Museum, and ask him whether he would have us collect examples without care and from only one part of the world ? If collectors were more often real naturalists, and not merely selectors of cabinet specimens, we should have little difficulty in amassing vast numbers of specimens showing beak marks or other results of attack. It is only since 1920 [1], following the observation by Dr. W. A. Lamborn, that the importance of this line of evidence has been realised : the writer has seen specimens in the British Museum bearing clear impressions of the beaks of birds which must have been examined by many entomologists without the significance of the marks having been perceived.

The older naturalists were before the days when it was denied, for the purposes of a far-reaching argument, that birds ate butterflies ; why should they have paid particular attention to what they doubtless considered, like Mr. Stuart-Baker, the well-known ornithologist [2], " such a commonplace occurrence in my experience that it is really difficult for me to pick out especial instances " ? Are there, in their books, many detailed accounts of birds eating spiders, caterpillars, or indeed other foods ?

Another of Mr. Wheeler's arguments concerns " the error of imagining that abnormal or even impossible events may occur in remote regions," and he writes that " the mystery is sometimes

shifted into tropical forests." The reference is to a book in which two experts in different groups state mimicry to be more common in forests than among similar insects in open country, and to an argument by the writer to the effect that what is known to happen within the limits of our vision may reasonably be expected to happen more often in the presence of more abundant life even if we cannot see it happen. Major Hingston [3] writes: "The tree roof is a distinct biological zone of which we have as yet only a fragment of knowledge. . . . Butterflies are commonly seen fluttering over the canopy. Some species probably never come to earth. Nymphalids like to crowd . . . around the golden blossoms. . . ." Again: "Whole groups of striking birds live perpetually in the tree-roof" and, "some species, such as the swallow puff-bird, prefer the topmost branches of the canopy . . . the upper surface of the roof makes an excellent station for the operations of insect-catching species."

The following note by a great student of butterflies may aptly be quoted here. Colonel Bingham, in 1888, sent to M. de Nicéville wings found on the ground in Burma to which he replied [4]: "Ferguson found a single wing of *Charaxes schreiberi* in Travancore on the ground. It is curious that the only record so far of the same species from Burma should be the three wings you send me." The extremely complete observation by Mr. T. H. E. Jackson in Uganda [5] of the destruction wrought by birds on butterflies frequenting a flowering tree is much to the point, although to Mr. Wheeler it is merely one observation. Most persons who study that account would consider it to be a very extraordinary coincidence that Mr. Jackson should have been present at one of the few occurrences of this sort; indeed, considering how common are the flowering tree, the birds, and the butterflies which he mentions, they might reasonably conclude that such destruction must take place frequently. Moreover, there is clear evidence of the discrimination which theory demands.

Mr. Wheeler says: "The positive cases are few." Such an argument can only be met by continuing to produce the evidence which exists, but unfortunately is often thought not to be of sufficient importance to be worth publishing. During the last year or two the writer endeavoured to make good this shortcoming, and brought before a certain learned society, as they came to his notice, examples of beak marks on the wings of butterflies and observations of the attacks of birds, so as to get them printed for Mr. Wheeler and others to see. This, however, became such a constant procedure that a suggestion was made to the writer that

if he persisted in these monotonous demonstrations of the obvious a protest would be made at a meeting against using up so much time to emphasize a fact which was common knowledge.

A few notes are here given, and not having been published before it is hoped they will receive the attention which their importance deserves. The observations of Mr. Collenette in Brazil were quoted in the article now under discussion: he apparently was more fortunate than Mr. Schaus, for in a letter to the writer about a year ago he wrote: "I cannot emphasize too much that these instances were really frequent and that I should not have seen them if I had not been interested in both birds and butterflies, and had not been quietly watching. These attacks were certainly a real feature in Matto Grosso." The importance of "quietly watching" is stressed by Mr. C. F. M. Swynnerton [6], who wrote: "A case in which a green bulbul, that I would certainly not have seen had I not been looking at that very point, appeared momentarily (from behind) between outer leaves on which butterflies were basking, seized one of them and as quietly withdrew, is highly instructive."

Commander C. M. Dammers, an experienced entomologist in California, wrote in October, 1935: "Our 'Black Phœbe' (*Sayornis nigricans*) can be observed now daily in our garden catching and devouring butterflies all day," and, on December 10: "*Re* birds eating butterflies. We consider it quite a common occurrence." The following testimony from South Africa comes from Dr. G. van Son, entomologist to the Transvaal Museum: "Regarding the attacks of birds on butterflies, I have witnessed a vast number of cases, mostly on the part of bee-eaters, some bush-shrikes, and wagtails. The bee-eaters are the chief culprits where *Charaxes* are concerned. They attack the latter on the wing from a perch overlooking the tops of trees around which *Charaxes* are displaying." (N.B.—Cf. Mr. Wheeler's reference 15; and also 14, which should be to *Proc. Ent. Soc. Lond.*, 1915: xxxii–xliv; p. xxxvii being especially to the point in connection with what has been written about flowering trees.) "The bush-shrikes are fond of stealing up to the butterflies when they are feeding on bait and pick them off on the run."

Mr. F. W. Frohawk, whose testimony is of unusual value as it comes from a leading authority on butterflies, wrote in 1933 to Mr. C. L. Collenette who was collecting evidence for his paper [7]: "I have watched broods of chaffinches, both adults and young, catching and feeding upon the silver-washed fritillary in the New Forest. This I have witnessed day after day for over a week at a time in mid-July during different years. . . . These large butter-

flies were captured on the wing, when they are so conspicuous. I saw none captured by the birds while they remained settled with closed wings amongst the foliage of the trees, when their green under-surface renders them difficult to detect."

Negative evidence must surely yield before such positive evidence.

We now come to Mr. Wheeler's conclusions, in the first of which he admits that "attacks upon butterflies at rest are fairly frequent, but difficult to observe." He is, of course, correct in pointing out that the advantage of a false head at the hind margin of the wing is not apparent in the flying butterfly. This applies to the straight, stiff tails, but not to the long, flexible, curved, snow-white tails, which, often four in number, float conspicuously behind the flying butterfly and invite attack. These long appendages, which add so much to the beauty of many Lycænids, break off extremely readily and it is difficult to secure a perfect specimen: the base of the wing from which the tail springs is often removed by a V-shaped incision nicely corresponding to the beak of a bird. Mr. Wheeler distinguished, quite properly, between attacks upon butterflies at rest and on the wing: the argument resulting therefrom has been previously shown to be untenable. It is certainly to be regretted that more precise details are not always recorded, yet it is often evident which is the case from the observation.

It is categorically stated that "attacks upon flying butterflies are very rare": perhaps they will seem less rare if due attention is paid to the evidence just quoted. If such a bird as a chaffinch can catch such powerful fliers as large fritillaries, Mr. Wheeler's personal opinion that "most insectivorous birds, except Drongos, are incapable of capturing uninjured butterflies during flight," can be discounted. It is not easy to understand why he considers that bee-eaters are incapable of catching butterflies, for, as the writer long ago showed [8], they habitually prey upon dragon-flies whose powers of flight can hardly be excelled. Dr. van Son's evidence is a direct contradiction to Mr. Wheeler's assumption.

Regarding evidence from beak-marks it is not possible to state categorically that symmetrical marks, or injuries, could only have been inflicted while the insect was at rest: if it were seized in the air at a moment when the wings were apposed before the downward stroke the same coincident marks might be made. Mr. W. E. Wait kindly sent to Sir Edward Poulton a detailed account of the capture of a large *Papilio* in Ceylon by a Paradise flycatcher. One of these darted out from a tree while the butterfly slowly flapped past, "and as the butterfly's wings met at the end of the upward stroke neatly nipped them together and so prevented them from flapping."

Another reason for not believing that birds eat butterflies, urged by Professor MacBride [9], has not been utilised by Mr. Wheeler, but can be discussed here. He mentions, without any reference, an account by Dr. C. B. Williams, of a swarm of Pierid butterflies upon which "hardly any" attacks were made by birds. The passage of this swarm is contrasted with the migration of a shoal of fish at sea. "When a shoal of herring comes into a bay . . . no doubt is left in the mind of the observer that the herring is the natural prey of the sea-bird." The implication is that butterflies are of no importance as food for birds, but it does not seem to the writer that the case of a shoal of surface-feeding fish entering a bay from which they were previously absent is comparable with the migration of a swarm of butterflies through country not devoid of such forms of life.

The following records, apparently unknown to Professor MacBride, are here brought together to show that there *is* evidence of attacks upon migrants, and in the hope that other observations may be forthcoming. Those interested in migrations are apt to be so occupied with estimating numbers, the direction of flight, and all the other facts on which information is so much desired that they may not be concerned with the attacks of enemies on the migrants. However, here is a record from Dr. C. B. Williams himself [10]: "In describing a flight of *Belenois mesentina* Cram. at Amani [N.E. Tanganyika Territory] in February, 1930, R. E. Moreau writes, 'Our turkey poults have been chasing them eagerly, bulbuls hawking them and drongos catching them and snipping the wings off.' Moreau is a first-class observer and ornithologist, so you can be sure of the identification of the birds."

Captain K. J. Hayward [11] described a "great and prolonged migration" of *Pieris phileta automate* Burm. in the Argentine: "The flight commenced on December 4 with males only, all fresh, but at least 95 per cent. badly chipped, the chipping being in a great number of cases uniform on opposite wings. In some cases the apices of the wings had been torn off. . . . On the 8th the proportion of females had risen to 50 per cent., and it was possible to catch any number required of perfect specimens of both sexes. This was no doubt due to the countless millions now passing daily which were far more than the birds could cope with, whereas at the beginning they had been fewer. On many occasions I found insects with a slightly curved mark on one of the wings. . . . (Beak of birds ?) . . . On several occasions I personally noted unidentified birds (high up) harassing these migrant butterflies."

Mr. Stuart-Baker, in his communication already mentioned,

wrote: "I remember watching a pair of King Crows (*Dicrurus macrocercus*) feeding on a swarm of small white butterflies. . . Whilst I was watching, these birds certainly caught a dozen butterflies and probably a great many more."

Miss Cynthia Longfield [12] recorded a large migration of a species of *Catopsilia* in Panama. There was a quantity of a white flowering bush which "was scented and attracted the butterflies, also some large cuckoo-like brown birds, who were lying in wait to make sallies on them." The birds attacked many times.

Mr. W. L. Puxley [13] writes of a migration of vast clouds of "a butterfly of the *Pieris* family." He says: "All that day these clouds passed, preyed upon by flocks of birds, and with their numbers thinned by many accidents. But nothing seemed to render the clouds less dense."

Finally, the records by Yerbury published long ago [14] contain a note on a migration of *Catopsilia* in Burma crossing the Salween River in a continuous stream and being persistently hawked by large bee-eaters mixed with which were some king-crows.

These observations will no doubt be disregarded as being "few." Nevertheless, just as an appeal for records has resulted in a considerable accession of observations on the attacks of birds, so the writer believes that when attention is directed to the argument adduced by Professor MacBride it will result in further records being published (or, better still, sent to Oxford for gathering together) of attacks on migrating and other butterflies.

The writer is indebted to his friends, Mr. E. B. Ford, B.Sc., and Dr. B. M. Hobby, D.Phil., for helpful criticism of his manuscript as originally written.

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PHOSPHORESCENCE AND PHOSPHORS

BY A. J. MEE, M.A., B.Sc.

THE phenomenon of luminescence has been known for centuries. The fact that certain animals and plants, usually of a lowly order, glowed in the dark, must have been observed by the earliest naturalists, and indeed, Pliny records certain instances of it, including that of the bivalve *pholas*. Decaying matter is often observed to be phosphorescent, and this was at one time given as a reason for the phosphorescence of the sea, a phenomenon now ascribed to the presence of certain luminous bacilli. In the vegetable kingdom, it is only algæ and fungi, and a few bacteria which are phosphorescent, whilst in the animal kingdom phosphorescence is frequently found amongst Protozoa, jelly-fishes (*medusæ*), worms, crustaceans, insects and fishes. Probably the best-known example of this is the glow-worm (or fire-fly), which is not really a worm but a beetle. In this case it is only the female which glows in the dark. In fact, in the majority of cases the luminosity appears to be developed for the purpose of attracting the opposite sex, though in other instances it may serve as a warning signal (as in the case of the stinging cœlenterata), or to attract prey (the angler-fish), or, with deep-sea fishes, probably to illuminate the ocean depths, where the light from above cannot penetrate. Of all animals, the glow-worm appears to give the greatest intensity of light, the reason being that its glow has a maximum intensity at a wave-length of 5700 Å., which is the wave-length to which the human eye is most sensitive.

In the previous paragraph the term "phosphorescence" has been used in its popular sense. For scientific purposes it is necessary to classify the various phenomena which to the layman are all included under "phosphorescence." When light falls on a body, part of it is absorbed, and part is reflected. Light is a radiation, and is a form of energy. The light absorbed by the body therefore imparts energy to it, and this energy may be used in different ways. Most commonly it is used to raise the temperature of the body, but if it happens to be of the right wave-length and

intensity it may disturb the electrons in the atoms of which the substance is composed, removing them to orbits of higher energy content, or sometimes ejecting them altogether. If the last occurs, the atoms become ionised, and we are dealing with the phenomenon known as the photo-electric effect. If, however, the electrons are only shifted to higher orbits, they will revert to their normal orbits, either at once, or at some later time. When the reversion is immediate, the phenomenon is referred to as *fluorescence*. Fluorescent substances do not glow in the dark, but whilst light is incident upon them they absorb light of one wave-length (*i.e.* of one colour), and give it out at another. Eosin, the red dye of red ink, is a good example of a fluorescent substance. When the reversion of the "exalted" electron to its original level is delayed, the substance glows after the incident light is switched off, and the phenomenon of *phosphorescence* is observed. Many cases of luminescence popularly called phosphorescence have nothing whatever to do with the absorption of light, but would go on whether the substance were previously exposed to light or not. Examples of these are the luminescent animals and plants referred to above. The way in which the light is produced by these organisms is not yet definitely known, but there seems to be no doubt that it is really a *chemiluminescence*, *i.e.* due to a chemical reaction which proceeds with the evolution of light. A remarkable thing is that some organisms will only glow if they have been kept in darkness. A case in point is that of the ovoid jelly "Beröe." This is the direct reverse of true phosphorescence.

The most common substance which glows in the dark is, of course, white phosphorus, discovered about 1674 (see *S.P.*, 1936, XXX, 402). The glow of phosphorus, however, though commonly called phosphorescence, is not actually an example of this phenomenon. The glow is due to the oxidation of phosphorus, and is therefore an example of chemiluminescence.

Even at the time when phosphorus was discovered, other inorganic substances were known which would glow in the dark, and these were the true phosphors. A so-called phosphorus was prepared by Baldwin in Saxony in 1674, by fusing calcium nitrate, and then allowing it to solidify. The product, of course, contained some oxide, as it is impossible to fuse calcium nitrate without partial decomposition, and it is now known that some other impurities, such as the salts of the heavy metals (copper, bismuth, manganese, etc.) must have been present, for without them the mass would not have glowed. To distinguish this product from other glowing substances, it was called Baldwin's phosphorus.

Another inorganic phosphor known at this time was Bologna phosphorus. This was discovered by an Italian alchemist, Casciarolo, who was in search of the "philosopher's stone." He heated a rock, probably barium sulphate, over a charcoal fire, and thus obtained barium sulphide, mixed with some oxide. When cool, the mass was found to glow in the dark. Here again, it is now known that the rock must have contained traces of heavy metals as impurities.

In spite of their antiquity, inorganic phosphors have remained a scientific curiosity until quite recent times. We owe a great deal of our present knowledge of phosphors to the German physicist Lenard, who worked at Heidelberg, and although, as we have shown, these phosphors have been known for a considerable period, they are now universally termed "Lenard phosphors." Lenard worked out successful recipes for the preparation of these phosphors, examined their structure, and attempted to explain their phosphorescence (see *S.P.*, 1913, VIII, 54).

In general, a Lenard phosphor is prepared by fusing the sulphide of an alkaline earth metal (calcium, strontium, or barium) with a flux, which itself plays no part whatever in the production of the phosphorescence, but merely aids fusion. A minute amount of a heavy metal, such as bismuth, antimony, copper, manganese, or lead, is added in the form of a salt of the metal during the fusion. Without this, no phosphorescence is produced. It is a well-established fact that the pure sulphide will not phosphoresce. The sulphide may be obtained from the sulphate, or from a mixture of calcium carbonate and sulphur, heated to about 1000° C. A good calcium sulphide phosphor is made by heating natural calcium carbonate containing some sulphur. The presence of excess calcium sulphate does not seriously affect the light emission of the phosphors though it has been shown that the presence of the oxide alters the position of the emission bands. In the preparation of a phosphor, the temperature and the duration of heating are of prime importance. The period over which the resulting phosphor will glow is partly conditioned by these factors, whilst the colour of the emitted light depends on the activating metal.

It has since been found that other metals may be substituted for the alkaline earth metals [1], and that oxides and selenides may be used in place of sulphides. From the above outline of the method of preparation of a phosphor, it will be realised that some oxide was inevitably present. It has now been found possible to make phosphors of which the "ground material" is a pure sulphide. Amongst the metals which may be used in place of

the alkaline earths are sodium, rubidium, magnesium, beryllium, zinc, and under certain circumstances, cadmium. It was shown by Tiede (1921) and others, that cadmium sulphide exists in two forms, a dark brown form which cannot act as the basis of a phosphor, and a yellow form which gives a weak and short-lived reddish phosphorescence on activation with copper. Cadmium may partly replace calcium in calcium sulphide phosphors. Alkali halide phosphors may be made by activating sodium and potassium chlorides with traces of copper [2]. An interesting phosphor has as its "ground material" silicon sulphide, and appears to be activated by carbon alone [3].

The materials used in the preparation of a phosphor must be of the highest degree of purity, as the phosphors are activated by minute quantities of heavy metals, and the presence of more than a maximum amount of impurity extinguishes the phosphorescence. It has been estimated that as little as 0·000006 gram of copper in 1 gram of zinc sulphide will render it phosphorescent. It is indeed difficult to obtain zinc sulphide which is so pure that it does not show even a weak phosphorescence without activation by a heavy metal.

The composition of a few typical Lenard phosphors is given below [4, 5]:

1 gm. calcium sulphate + 0·0004 gm. antimony + 0·2 gm. sodium sulphate + 0·06 gm. sodium chloride.

1 gm. strontium carbonate + 0·0001 gm. bismuth + 0·06 gm. magnesium fluoride.

25 gm. barium carbonate + 15 gm. strontium hydroxide + 15 gm. sulphur + 3 gm. starch + 1 gm. magnesium oxide + 1 gm. lithium sulphate + 2 c.c. 0·5 per cent. solution of thorium sulphate + 3 c.c. 0·4 per cent. cupric sulphate solution.

The last gives a golden yellow phosphorescence. Further formulæ may be obtained by consulting the references quoted.

The scientific study of these phosphors has naturally proved a difficult task. The specific action of certain heavy metals in bringing about phosphorescence has still not been explained, though the amounts of them required to give the greatest brilliance have been determined with some degree of accuracy. It is found that a maximum intensity of the glow is reached when the amount of activating metal is still very small. Besides affecting the intensity of the light, the nature of the heavy metal decides the colour of the glow. The concentration of the heavy metal also affects this, for examination of the emission bands of a phosphor shows that these reach their maximum intensity when different amounts of

heavy metal are present. If too much heavy metal is added, the resulting mass will not phosphoresce at all. A careful investigation of the heavy metals which will produce phosphorescence in the oxides, sulphides, and selenides mentioned above fails to reveal any definite regularities between the various phosphors. Even closely related elements, such as the members of the alkaline earth family, are not all activated by the same heavy metals. Thus calcium and strontium sulphides are both activated by manganese, whilst barium sulphide is not. Similarly, closely related heavy metals, such as the members of the rare-earth series, do not all activate the same sulphides. Thus, neodymium will activate calcium and strontium sulphides but not magnesium sulphide, whilst the closely related element samarium is successful in activating a much larger number of sulphides and oxides, and gadolinium can only be used in the case of magnesium sulphide.

As stated above, the colour of the light emitted by a phosphor varies with the added heavy metal. Thus phosphors containing bismuth and thallium give a violet light; those containing bismuth and rubidium give a deep blue light. The light emitted by a phosphor has always a greater wave-length than the absorbed light, in accordance with Stokes' rule. The emission spectra of Lenard phosphors have been very extensively examined, though the emitted light is weak, and the work consequently of considerable difficulty. In the majority of cases the spectra show two or three emission bands, and these can often be separately excited, or at any rate selectively strengthened by altering the conditions of preparation of the phosphor.

The exact nature of the structure of these phosphors is still in doubt. The older view was that a solid solution of the activating metal in the "ground material" was produced. The amorphous appearance of the phosphors led to the conclusion that they did not possess a crystalline structure. Lenard assumed that the fundamental sulphide and oxide possessed a molecule with a particularly open structure, in which an atom of the heavy metal could easily be embedded. Such molecules with their heavy metal atom were termed phosphorescent "centres," and the mass phosphoresced only from these centres, the rest being non-phosphorescent. He supported this view by showing that the molecular volume of the phosphor was considerably greater than that of the pure materials, and that often the application of pressure to a phosphor would destroy its phosphorescence, with a corresponding increase in density.

It is clear that this is a question upon which X-ray analysis

could throw some light. Schleede has applied the Debye-Scherrer method to phosphors, and has shown that a number of them are crystalline, and not amorphous as assumed by Lenard. Of course, it could be argued that the crystal diagram is due to crystallites embedded in a vitreous matrix, and that the vitreous portion is the part which is active, but there are several reasons for believing that it is actually the crystalline portion that is active. Thus, it is possible to sublime zinc sulphide (Sidot's blende) in an atmosphere of hydrogen sulphide and the crystals deposited are phosphorescent. Moreover, Tiede has made phosphors without the use of fluxes by melting alkaline earth sulphides under pressure. If it is taken that the crystal lattice is the seat of phosphorescence, it is easy to explain the effect of pressure noted by Lenard, and mentioned above, as a distortion of the crystal lattice. If this is true it should be possible to regenerate a phosphor which has been extinguished by pressure by gentle heating, when the increased thermal agitation might give the atoms a chance to get back into their original places. This has actually been observed with zinc sulphide.

Unfortunately, however, X-ray analysis does not elucidate the rôle played by the heavy metal atom. It has not been found possible to say just where the activating atom is. Smekal [6] has examined the conditions for the formation of mixed crystals, and has shown how to decide whether mixed crystal formation (*i.e.* interchangeability of atoms in the crystal lattice), or merely adsorptive inclusion of a foreign substance in the spaces in the crystal lattice, may occur. It seems possible that the second phenomenon may occur in the phosphors under consideration. Support is given to this conclusion by the fact pointed out by Tiede [7] that the atomic diameter of the activating metal is always less than that of the metal atom of the "ground material." Taking calcium as an example, its atomic diameter is estimated to be $3{\cdot}93 \times 10^{-8}$ cm., whilst the diameters of bismuth, lead, manganese, copper, and antimony are 3·10, 3·49, 2·58, 2·55, and 2·87 (all $\times 10^{-8}$) cm. respectively. It has also been shown by Tiede and Weiss [7] that copper can activate a zinc sulphide phosphor when the two are heated to a temperature of only 350° C., whilst a temperature of 650° C. is required for mixed crystal formation.

A theory of the spatial structure of phosphorescent mixtures has also been advanced by Schloemer [8]. He states that the middle point of a phosphorescent centre is occupied by an active ion, which is surrounded by molecules, or groups of molecules in the form of the crystal lattice, but somewhat distorted by the presence of the active ion. Alternatively, the molecules are arranged

round the active ion in a similar way to the arrangement of water molecules round a hydrated ion in electrolytes.

This is the sum of our present knowledge concerning the actual position of the activating atom in relation to the molecule of the alkaline earth sulphide or other "ground material." The next question is to discover how the heavy metal atom actually endows the "ground material" with the property of phosphorescence. Lenard assumed that a sort of photo-electric effect was responsible for the phosphorescence. When light is shone on the alkali metals, particularly potassium, rubidium and cæsium, electrons are set free from the metal. This is known as the photo-electric effect. Lenard supposed that electrons were set free from the heavy metal atoms and that these electrons, instead of escaping freely, were retained by the "ground material." When the exciting light was shut off, the electrons returned to the original atoms from which they came, the process being attended by evolution of light.

More recent work by Tomaschek [9] has extended this simple explanation by showing more clearly how the electron is liberated. Work on the emission and absorption spectra of phosphors containing the rare earths has led to the conclusion that the incident energy is absorbed by the molecules comprising the centres, and is stored as exciting energy. Thermal agitation occasionally brings about the collision of an excited centre molecule (which will not be held so securely in the lattice owing to the distorting influence of the heavy metal atom) with a heavy metal atom, the energy being transferred to the latter, which then emits an electron.

There are many other phosphors besides those commonly known as Lenard phosphors, but space forbids any exhaustive treatment of them. The naturally occurring mineral, fluorspar, calcium fluoride, and also the synthetically prepared calcium fluoride show phosphorescence. Here again the phosphorescence depends upon the presence of an impurity.

Then aluminium oxide, in the presence of certain impurities, becomes phosphorescent. The ruby is an example. This is aluminium oxide containing a very small amount of chromium oxide—as little as 0·001 per cent. of chromium oxide, Cr_2O_3, is enough to give a phosphorescence. As an activating element, chromium behaves in many respects differently from the heavy metals and rare earth elements [10]. Thus the absorption and emission spectra of chromium-containing phosphors are identical, and the spectra are very characteristic, being composed of three groups of lines. Chromium appears to be effective chiefly when chromium oxide

is isomorphous with the "ground material," as it is, for example, with aluminium oxide.

A very interesting class of phosphors is that often called the Wiedemann phosphors. It was shown by Wiedemann, in 1884, that if fluorescein were added to liquid gelatin, and the mixture were allowed to set, the product was phosphorescent. These phosphors, owing to their feebleness, are not easy to study. However, the same phenomenon is observed when boric acid is fused with the addition of a little of the sodium salt of fluorescein. On cooling the melt, the mass is found to be extraordinarily phosphorescent, though of short life. The glow does not as a rule last for more than two minutes. These phosphors have been investigated by Tiede and his co-workers [11], and are by far the easiest of all phosphors to prepare. About 1 gram of crystalline boric acid is mixed with a few drops of a solution of fluorescein in alcohol to which a little sodium hydroxide has been added. The mixture is then heated on a crucible lid, or in a shallow metal vessel until it almost completely fuses. Complete dehydration should not be aimed at. If the substance is allowed to cool, and is then illuminated and placed in the dark it glows with a very bright light. The amount of fluorescein added does not seem to be critical, nor does the actual temperature to which the mass is heated, provided this is not too high. Phosphorescence, however, does not manifest itself until the phosphor has cooled below a definite temperature. The rapidity of cooling appears to affect the brightness and duration of the glow. The more rapid the cooling, the brighter the phosphor. This is probably due to a greater distortion of the crystals of fluorescein produced by rapid cooling. The phenomenon is by no means confined to the sodium salt of fluorescein. The author has investigated a number of dyes, mainly of the fluorescein class, and has been able to obtain phosphors of various colours. Thus, with the sodium salt of fluorescein, the phosphorescence is yellowish-green; a beautiful yellow phosphor can be produced if the sodium salt of tetrabromo-tetrachlorofluorescein is used in place of fluorescein. Mixtures of the sodium salt of fluorescein with methylene blue, Biebrich scarlet, phenosafranin, or α-naphthol-benzein give fine phosphors of different shades of blue, whilst methyl red gives a pink phosphor. All the above phosphors are excited by the light of an ordinary electric lamp. Others are known which are excited by ultra-violet light. The intensity of these phosphors gradually decreases on keeping, probably because of rehydration of the boric acid.

Phosphors have found a number of uses. That with which

the layman most commonly comes into contact is luminous paint. Such paint consists of a Lenard phosphor, usually calcium, barium, or zinc sulphides, mixed with a minute amount of a radioactive substance, without which the glow would die out, to be renewed after further illumination. In the scientific world, however, phosphors, particularly zinc sulphide and barium platinocyanide, have been used for making X-rays, cathode rays, and α-rays "visible." These radiations excite phosphorescence in these phosphors, and the tracks of the rays are then easily seen. The use of barium platinocyanide screens in X-ray work before X-ray photography was developed is well known, whilst it will be remembered that Rutherford counted the α-particles emitted from a radioactive preparation by observing the scintillations produced when the particles struck a zinc sulphide screen.

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VERNALISATION

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Vernalisation is the name applied to a treatment given to seeds before sowing in order to hasten the time of flowering. The practical application of this process has been worked out in Russia during the last few years and it has recently been attracting considerable attention in this country. The idea, however, is not entirely new, for it is founded on numerous investigations in plant physiology carried out chiefly in U.S.A. over a long period of years. Before going on to discuss vernalisation in practice and theory it is essential that these antecedents should be indicated.

Vernalisation is concerned with the relation between the vegetative and the reproductive phases of the plant's life cycle. It used to be thought that these two phases were antagonistic, that growth and the reproductive processes could not go on together; when flowering started growth stopped.

This view, however, had to be modified. It was only a half-truth. The antagonism is not between the vegetative phase and the reproductive phase, but between vegetative growth and fruit formation. Indeed, the production of gametes and pollination actually give a stimulus to vegetative growth. This initial stimulation is then, under practical conditions, followed by a retardation due to the monopolisation by the developing fruits of all the available food supplies. If the amount of nitrogen is limited the vegetative tissues may even be broken down to supply nitrogen for the fruits. In ideal conditions where the amount of food material is unlimited there may be enough to allow both fruit growth and vegetative growth to proceed together. The initiation of the reproductive phase, therefore, does not mean the end of the vegetative phase, the two can run side by side. It is only when the developing fruits monopolise all the food material that growth ceases. Fruit formation should be regarded not as a reproductive process, but as a specialised form of vegetative growth. This may seem like hairsplitting and under practical conditions it

certainly is, but nevertheless the distinction is an important one.

The initiation of the reproductive phase is determined by the conditions of the environment in which the plant is growing. One of the most important of these conditions is light. Now, it has been proved by two American workers, Garner and Allard, that the time of flowering is determined, not by the intensity of light and not by the amount of light the plant receives, but by the length of day. In the tropics the days are short, there is a regular alternation of 12 hours' light and 12 hours' darkness and tropical plants are adapted to flower in these conditions. It was found that when grown in day-lengths of less than 12 hours these plants soon flowered, but when grown in longer days of say 16 hours they remained in the vegetative state and showed no signs of flowering. Plants which behaved in this way were called "short-day plants" and they include such economic plants as Soybean and Millet. A more familiar example from our own country is the Violet, which flowers only in the short days of spring. Towards the pole the summer day-lengths become longer till in the Arctic circle there is no darkness at all, and plants of northern latitudes are adapted to flower in these conditions. It was found that when such plants were grown in normal long days they quickly flowered but, when grown in short days, they remained sterile. Plants which behaved in this way were called "long-day plants." Nearly all our common plants are of this type and, of our farm crops, wheat and turnips may be mentioned. Some plants, however, such as annual meadow grass, were found to be uninfluenced by length of day and they were called "everblooming plants." This effect of day-length was very aptly termed "photoperiodism."

It was thought, at first, that this division into short-day, long-day and everblooming plants was a rigid one, and that a plant grown in an inappropriate day-length would remain sterile indefinitely. This view, however, had to be subsequently modified. It seems that a plant has an optimum day-length giving earliest flowering but can flower within a range about this optimum. Some plants have a very restricted range; these are the typical short-day and long-day plants. For instance, Cosmos will remain vegetative indefinitely in long days. Others have a wide range; wheat, for example, will eventually flower in short days. An extreme example of this is annual meadow grass which will flower over the whole range of from 5 to 20 hours' daylight.

Another interesting fact brought to light was that when a plant was grown in the appropriate day-length and then transferred to the wrong day-length before flowering, it would still flower. For

instance, Cosmos, a short-day plant, was grown in short days for 12 days and then transferred to long days and it flowered 8 days after the change. This showed that the effect of day-length is exerted some time before flower buds become visible and that the effect is not negatived by a subsequent change in length of day.

How can photoperiodism be explained? Further experiments showed that the effect is due neither to the amount of light received nor to the influence of darkness. Is the effect due to the alternation of light and darkness? Certainly not in the case of long-day plants as it has been shown that they can flower in continuous light but, for short-day plants, alternation at suitable intervals seems to be necessary. The alternations, however, must not be too rapid. Soya beans given 10 hours' light followed by 14 hours' darkness soon flower, but, if the exposure to light is interrupted by a period of darkness, *e.g.* 5 hours' light, 4 hours' darkness, 5 hours' light, 10 hours' darkness, the plants will not flower even though they receive the same total exposure to light. The truth is that as yet photoperiodism cannot be explained. Our knowledge is purely empirical. It just happens.

Another important factor influencing flowering is temperature. It has long been known that if beet or Brassica seedlings get a touch of frost the plants will bolt, though normally they do not flower till after winter. In this case the plant is subjected to the low temperature long before flowers become visible. This influence of temperature was well shown in an experiment where full-grown cabbages were transplanted to a warm greenhouse in October. They refused to flower and two years later were still growing vigorously. Cabbages transplanted at the same time into a cool greenhouse flowered in 22 weeks while cabbages left out till December so as to get the full benefit of the winter cold and then transplanted into a warm greenhouse, flowered in 6 weeks.

As long ago as 1910 it was shown that the germination of cereals at low temperatures hastened heading. Later work showed that when cereals were grown in warm conditions they tillered excessively but produced no ears. Then, it was observed in America, that when winter wheat was sown late and there was sufficient moisture in the soil to start germination before the cold set in, seedlings appeared in spring and the plants headed normally, whereas, if there was not sufficient moisture, the seeds remained dormant till spring and then germinated but the plants did not head, *i.e.* they behaved like spring-sown winter wheat. This suggested that the winter cold influenced germinated seeds but not

dormant seeds. Other experiments all went to show that temperature has just as much influence on time of flowering as length of day.

The foregoing paragraphs indicate what was the state of our knowledge at the time the Russian work began to assume importance. The position may be summarised as follows:

1. The vegetative and reproductive phases of a plant's life cycle are not antagonistic but can run together.
2. Two of the major factors influencing the time of flowering are length of day and temperature.
3. The influence of these two factors can be exerted long before flowering becomes visible.

And now let us turn to the Russian work. The Russian investigations on vernalisation have been carried out by several workers, but the co-ordinating mind is Lysenko, and it is he who has stated the theory underlying the process. His ideas have been summed up in four postulates:

1. Growth and development are not identical phenomena and they are independent.
2. The entire process of the development of an annual seed plant consists of individual steps or stages.
3. The stages always proceed in a strict sequence and a subsequent stage cannot set in until the preceding stage has been completed.
4. Different stages of development of the same plant require for their completion different external conditions.

Emphasis is laid on the distinction between growth and development. Growth is a quantitative change, it gives an increase in number or size but it produces nothing new. Development, on the other hand, is a qualitative change, it produces something new, in this case the reproductive organs. We have already seen that these two processes can proceed independently.

How many of these postulated stages in development there are is not known, but, so far, Lysenko has distinguished two of the early stages and the external conditions they require. These are the "vernalisation stage" and the "photo stage." The vernalisation stage is a stage requiring a definite temperature. We have already seen that Brassicas and cereals must have a low temperature some time in their life cycle before they can flower. This is the stage for which the low temperature is necessary. Once this stage has been completed temperature no longer has any effect on flowering. Similarly, the photo stage is a stage requiring a particular length of day, and, after this stage has been passed, the

day-length is of no consequence. The photo stage cannot proceed until the vernalisation stage has been completed.

These two stages of development can be passed at any phase of growth. The essence of Lysenko's treatment of seed is that they are passed before the seed is sown and yet the seed is still in a condition to be sown by normal methods. The young plant is made to pass these stages by applying the appropriate external conditions artificially. It is no use doing this to the dormant seed, the embryo must have germinated and started growing. If seed is germinated in the usual way growth is too quick and long radicles are produced. In this condition it would be quite impossible for the seed to be sown. Growth is therefore slowed down by limiting the amount of water. It has been found that seed will germinate slowly if the moisture content is raised to 50 per cent. and this can be done in practice by adding to the seed 30 per cent. of its weight of water. The seed is germinated until the radicles are just beginning to burst their seed coats and then the treatment is applied. After treatment the seed can either be sown immediately or dried and kept until later. As treatment does not interfere with vegetative growth the yield from treated seed is satisfactory.

The treatment applied varies for different plants. Here we will consider two crops typical of different requirements, wheat and maize.

Winter wheat requires a low temperature for its vernalisation stage and long days for its photo stage which normally it receives in winter and summer respectively. When winter wheat is sown in spring it tillers excessively but rarely heads the same season. This is not because winter wheat requires a longer time before heading but because the absence of low temperatures prevents its passing the vernalisation stage. Vernalisation treatment consists in keeping the partially germinated seeds at a temperature between 0° and 5° C. for anything from 15 to 60 days according to the variety, thus enabling them to pass the vernalisation stage. When sown in spring the plants can pass the photo stage in the natural conditions of long days and will head and ripen seed the same season. By means of this process, therefore, winter wheat can be sown in spring, and, in Russian conditions, the resulting crops are earlier and give bigger yields than spring wheats. The difference between winter and spring wheat is that, for the vernalisation stage, winter wheat requires a long period of intense cold while spring wheats require very much less. The result of artificial vernalisation, therefore, is to bring winter wheats into the

condition as spring wheat. Indeed, the meaning of the word "vernalise" is "to bring into the spring condition" although the term is now applied to any process of seed treatment designed to accelerate flowering.

In maize the vernalisation stage requires a temperature of 20–30° C. This temperature is quite common in temperate regions and, therefore, this stage can be passed in the field. The photo stage, however, requires a short day and the long days of temperate regions prevent this stage being passed and so prevent flowering. It is this factor of day-length and not temperature that is said to prevent maize growing in northern latitudes. The Russians believe that this can be remedied by supplying the conditions for the photo stage artificially. Lysenko holds that for short-day plants alternation of day and night is unnecessary, that darkness alone brings about the photo stage and light hinders it. From this it is concluded that the requisite darkness can be supplied in one dose and the treatment consequently consists in keeping the partially germinated seeds in complete darkness. The photo stage must of course be preceded by the vernalisation stage which requires a high temperature, and, in practice, the two conditions are supplied together, the seeds being kept in darkness at a temperature of 20–30° C. for 15 days. The seed subsequently sown will produce plants which, it is claimed, will flower in long day conditions.

This work embodies two new ideas of great importance. These are:

1. The slow germination of seeds with low water content and the application to this material of the external conditions necessary for flowering.
2. That short-day plants require darkness for flowering and that this darkness can be supplied in one dose.

Work on similar lines, and to a certain extent independently, has been proceeding in the U.S.A. and it is interesting to compare the Russian and American results. American workers agree that the cold treatment of wheat accelerates flowering but believe that the day-length factor cannot be left out of account. It is pointed out that vernalisation reduces yields as compared with winter-sown wheats. From their results it appears that the life cycle of winter wheat has two phases—an early phase requiring low temperatures and short days and a late phase requiring high temperatures and long days. These, of course, are the natural conditions under which wheat is grown, the early phase being passed in winter and the late phase in summer. It was found that day-length in the early phase had as much influence on the time of flowering as

temperature and that when the correct conditions for both phases were supplied, there was earliest flowering combined with highest yield. It is emphasised that these are *optimum* and not essential conditions. Winter wheat sown in spring does not flower because it lacks the low temperature and short days required for the early phase, but it would flower if the season were long enough. This has been demonstrated by removing the plants to a greenhouse and thus prolonging the season. In these conditions the plants eventually flowered even though they never experienced low temperatures.

As for the theory that short-day plants only require darkness for flowering there is really no experimental evidence for it but some definitely against it. Garner and Allard's experiments on short alternations of light and darkness showed that the total exposure to darkness would not explain the effect of short days and their attempts to make Cosmos flower by keeping it in the dark failed. Vernalisation of maize has been tested in America and the most striking effect of the process was found to be a serious reduction in field germination. The hastening of flowering was found to be negligible though a slight acceleration was found when vernalisation was carried out in *continuous* light instead of darkness. Finally, it was shown that maize can flower when grown in continuous light. In face of such evidence the vernalisation of short-day plants cannot yet be regarded as an accomplished fact.

The value of vernalisation to British farmers can only be decided by trials with our own varieties and under our own conditions but, at the moment, it seems unlikely that it will ever play a prominent part in British agriculture. It is true that there is a lack of really good spring wheats but, at the same time, there is no serious need for them. Winter sowing will always be preferred to spring sowing, for not only does it relieve the congestion of work in the spring but it also gives a better yield. Vernalised winter wheat will give a better yield than spring wheat but winter-sown wheat will give a better yield than either. Spring sowing is sometimes unavoidable but it would not be economic for a farmer to maintain an expensive cooling plant for such emergencies, though it might be done by contract. The possibility of making Brassicas flower in their first season should not be overlooked. If this proves to be practicable seed growers will indeed benefit. As for short-day plants, if the vernalisation of these plants is ever achieved it is difficult to imagine that they will ever be grown on a large scale. A possibility would be the small-scale growing of maize for green cobs to be used as a vegetable.

In Russia conditions are altogether different. There the greatest need is for a wheat which will head before the dry season sets in. The number of famines in the past is a measure of how far the wheats commonly grown in Russia have failed, in this respect, and it looks as though vernalisation will provide the remedy. The drought and long days of Russia are all against growing crops such as maize and millet, but there seems no reason to doubt that if vernalisation of short-day plants ever proves possible, they will be grown.

In America, of course, short-day plants are already extensively grown, particularly maize and Soybean. The day-length is certainly more than 12 hours but it is less than in Russia. Trials of vernalised wheat have shown that it is inferior both in yield and earliness to spring-sown Marquis. Marquis is the ideal wheat for American conditions and it cannot be improved upon by vernalisation. In fact vernalisation is unnecessary in the States and it would probably be true to say that, if it were of any practical advantage there, it would have been put into practice long ago. Was it not an American who wrote: "To convert winter into spring wheat, nothing more is necessary than that the winter wheat should be allowed to germinate slightly in the fall or winter, but kept from vegetation by a low temperature or freezing, until it can be sown in spring. This is usually done by soaking and sprouting the seed and freezing it while in this state and keeping it frozen until the season for spring sowing has arrived. Only two things seem requisite: germination and freezing. The experiment of converting winter wheat into spring wheat has met with great suc- ' "? And that was written in 1857!

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., F.R.S.E., Royal Observatory, Edinburgh.

The 200-inch Telescope.—In the *Astrophysical Journal*, **82**, No. 2, Dr. G. E. Hale describes the progress to date of the new astrophysical observatory of the California Institute of Technology. This ambitious undertaking was made possible by large monetary grants made by the Rockefeller Trustees to the California Institute, conditional on an assurance of active co-operation by the Carnegie Institution of Washington, and the provision of an adequate endowment for the new observatory by the California Institute. The plan for the new establishment is due largely to Dr. Hale, and it includes (1) a 200-inch reflector with subsidiary instruments erected on the most favourable high altitude site in the neighbourhood of Pasadena ; (2) a laboratory situated in Pasadena equipped for the reduction and interpretation of the observations, for physical experiments, and for general research ; and (3) instrument and optical shops for the construction and testing of the special apparatus required. In the words of Hale, " the chief points to be borne in mind are (1) our purpose to supplement rather than to duplicate existing apparatus ; (2) to multiply the efficiency of all our instruments not merely by increase in size but by every possible improvement in methods of design and construction, and especially by the development of new auxiliary apparatus ; and (3) to render the results of these investigations available at once."

The giant 200-inch telescope is to be an equatorial reflector, mounted so as to permit observation at the principal focus, at the Cassegrain focus below the large mirror, and at the coudé focus within a constant temperature laboratory south of the polar axis. The mirror disc has not been cast without considerable difficulty. Fused silica was first selected as the material on account of its very low coefficient of temperature expansion. A mass of nearly pure quartz sand was fused in a circular electric furnace comprising the mould, and the disc thus obtained was coated with a layer of transparent fused quartz produced by spraying the pure

crystalline substance in finely granular form on to the hot surface. Complete success attended these efforts up to a diameter of 25 inches, but for much larger discs the difficulties encountered proved insurmountable and the method had to be abandoned. Pyrex glass was then adopted as the material, and, after successful experiments with smaller sizes, a 200-inch disc was cast in March 1934 at the Corning Glass Works. Unfortunately the extremely high temperature of the glass proved too much for the stability of the mould, and another attempt had to be made eight months later before success was attained. An electrical control system was devised for lowering the temperature of the disc during the annealing process, which, as expected, has taken nearly twelve months to complete. The disc has now been accepted as satisfactory. As both the 60-inch and the 100-inch mirrors on Mount Wilson have been successfully aluminised at the California Institute, it is not expected that the coating of the 200-inch will present any difficulty.

Experiments have also been made with other possible materials for the mirror. Stainless steel has certain advantages in its heat conductivity, the permanent brightness of its surface, its high coefficient of reflection in the ultra-violet, and its relatively small cost. A special type of metallic disc having a thin layer of glass of the same coefficient of expansion securely fused to its surface has been found to be remarkably insensitive to sudden large changes of temperature. It should therefore be of special advantage for solar work with a cœlostat, and with this object a 36-inch plane mirror of this type has already been obtained from the Philips Lamp Works.

The mounting of the telescope is to be of a modified yoke type, with the declination axis placed not far below the centre of the tube. This arrangement will increase its stability, and allow the use of a relatively smaller dome. The observer will be carried in a cartridge-shaped house at the principal focus, and no Newtonian flat mirror will be necessary.

The summit of Mount Palomar, latitude 33° 21′ N., altitude 6,100 feet, is the probable site for the erection of the telescope. Situated 93 miles south-east of Pasadena and 50 miles north of San Diego it should be free from the interference of city electric lighting, and yet be sufficiently accessible to permit the necessary co-operation with the staffs at Mount Wilson and at the California Institute. A series of meteorological observations extending over five years shows the average wind velocity and rainfall to be similar to those on Mount Wilson, and the seeing has been pronounced superior.

The optics of the new instrument has received special attention from Dr. Frank E. Ross of the Yerkes Observatory. The only aberration which is important in the focal plane of the usual paraboloidal reflector is coma, which is directly proportional to the radius of the field and to the square of the aperture ratio of the mirror. The high speed ratio F/3·33 planned for the 200-inch mirror, will permit the study of very faint stars and nebulæ, but it implies a very small field. According to the simple theory of coma, if β'' is the angular distance of a star from the axis of the mirror, D the aperture, F the focal length, and K″ the total length of the comatic image in seconds of arc, then

$$K' = \frac{3}{16}\left(\frac{D}{F}\right)^2 \beta'',$$

and the breadth of the image is $\frac{2}{3}$ K″. Plates taken with the 100-inch reflector and with the 36-inch instrument at Lick show that an image does not appear defective until it is comatically enlarged 35 per cent. On this assumption the above equation shows that for the 200-inch reflector coma will become noticeable at an angle $\beta = 87''$ from the axis. The total coma-free field will accordingly be only 14 mm. in diameter, but Ross is inclined to increase this estimate by 50 per cent. and to expect reasonably good definition in a field of about 20 mm. Even so, the field will be too small for successful work in photographic photometry and astrometry, though adequate for most other researches.

In the *Astrophysical Journal*, **81**, No. 2, Dr. Ross describes his investigations regarding the possibility of introducing a lens system near the focus, and thus correcting the coma without causing other aberrations of a serious amount. Employing the third-order equations of general optical theory, he has obtained a solution for a two-piece lens system which eliminates coma and astigmatism, while leaving a certain amount of the less important spherical aberration. The lens is a two-piece one, with separated components of the same glass. On account of the necessary thickness of the lens elements it is not possible to design a system of strictly zero power and also satisfy the colour correction, but the increase in the focal length of the instrument and the outward displacement of its focus have been kept within admissible limits. It was found that a double family of lenses could be designed with different amounts of spherical aberration varying inversely as the extent of the field free from astigmatism. Correcting lenses, based on the theory of this paper, have already been constructed for the 60-inch and 100-inch reflectors at Mount Wilson, and have resulted in a

very satisfactory enlargement of the field of good definition. For the 200-inch telescope Dr. Ross has designed two separate correcting lenses, both of which will remove coma. The first, with a spherical aberration confusion disc 1·5″ in diameter, is for use under the best seeing conditions, when the area of the useful field at the principal focus of the instrument will be increased tenfold. The second lens, to be used when conditions of seeing are not so favourable, will have a confusion disc of 2·5″, and will increase the field from twenty to thirty times. The loss of light caused by the introduction of the lens is only about 0·25 of a magnitude, and is therefore only slightly more than that produced by the silvered surface of a Newtonian flat.

The Astrophysical Laboratory is situated in Pasadena, and the building has already been completed. To facilitate work which requires a constant temperature there have been provided two and a half storeys completely under ground, in addition to a rectangular well, 13 × 30 × 60 feet, designed for a 75-foot spectrograph to be used with a cœlostat with plane mirrors 36 and 30 inches in diameter. This is to be placed in a tower 68 feet in height, from which the solar beam is reflected nearly vertically downwards. By means of a system of concave and convex mirrors it will be possible to place solar images 22, 14, and 7 inches in diameter on the slit of the spectrograph. Provision is to be made for the special polarising apparatus required in the study of solar magnetic phenomena, while the auxiliary equipment includes a large magnet for work on the Zeeman effect, an electric furnace, and a special interferometer spectrograph. The building also includes offices, library, photographic, photometric and chemical laboratories, and six dark-rooms.

The Instrument and Optical Shops occupy adjoining buildings near the Laboratory. The former is equipped with modern machine tools which, while not large enough to undertake anything so huge as the mounting of the 200-inch telescope, are competent to deal with the construction of its auxiliary apparatus. A 5-ton electric crane runs overhead through the central bay, so there will be no difficulty in handling heavy castings. The numerous machine tools are all motor driven and are very comprehensive. The Optical Shop, connected by an arched entrance-way, consists mainly of a large single room of a size sufficient for figuring and testing the 200-inch mirror, a task estimated to occupy at least five years. The inner walls are lined with a 2-inch cork, and a special air-conditioning system, giving control of the temperature and humidity of the air, has been installed. The 200-inch grinding

machine is now being built, and suitable ones are already provided for smaller sizes. Work is at present proceeding on a Pyrex disc of 120 inches.

Such lavish expenditure on research is possible only in America, and workers in other less favoured countries have to be content with much simpler equipment. Nevertheless, there is considerable truth in Einstein's remarks in his book *The World as I see it*, "We are unjust in attempting to ascribe the increasing superiority of American research work exclusively to superior wealth; zeal, patience, a spirit of comradeship and a talent for co-operation play an important part in its successes. I have a warm admiration for the achievements of American institutes of scientific research."

Private generosity in aid of astronomy is, however, happily not confined to the United States, for on May 31 there was a formal opening of the David Dunlap Observatory, which has been presented to the University of Toronto by Mrs. Dunlap in memory of her husband. *The Journal of the Royal Astronomical Society of Canada*, September 1935, contains a full account of the opening, together with a description of the Observatory by the new director, Dr. R. K. Young. The principal instrument is the 74-inch reflector, the largest instrument in the British Empire. The mirror was cast of pyrex glass by the Corning Co., New York, but the figuring and polishing as well as the construction of the mounting were entrusted to Messrs. Grubb-Parsons of Newcastle-on-Tyne. It will be used chiefly in the field of stellar velocities, and spectral photometry.

Variation in Solar Radiation.—In the *Smithsonian Miscellaneous Collections*, **94**, Nos. 10 and 12, C. G. Abbot gives an account of the measures of solar radiation which have been made during the years 1920–34 on certain mountain peaks in desert lands. The three stations now in use average over 8,000 feet in altitude, and are situated in California, Chile, and the Sinai Peninsula, the last named having superseded in 1933 the earlier establishment at Mt. Brukkaros, South-West Africa. The radiation has been measured by silver disc and water flow pyrheliometers, supplemented last year by the electrical compensation pyrheliometer. The sun's ultra-violet rays are cut off by the layer of ozone in the higher atmosphere, while water vapour performs a similar function in the infra-red, but it has been found that observations of the brightness of the sky in the neighbourhood of the sun suffice to give an estimate of the losses of radiation at all spectral wave lengths through traversing the atmosphere. The method in use is explained in the *Smithsonian Annals*, **5**, and by its aid the "solar constant," the intensity of the sun's radiation outside the atmosphere at the

earth's mean distance is computed. Variation in the solar constant has been demonstrated by simultaneous measurements at stations in opposite hemispheres, where seasonal effects are quite different. To casual inspection the variations appear irregular, but closer examination has found evidence of a well marked 23-year cycle, together with some integral submultiples of this period. This is, of course, approximately equal to the solar magnetic spot cycle, and twice the ordinary sun spot period. Various terrestrial phenomena depending on weather have been examined, and several have been found to show the influence of this twenty-three-year cycle. They include the flow of the River Nile, the levels of the American Great Lakes, the rainfall of southern New England, and the widths of the annual rings in the trunks of trees. Abbot claims that, on these lines, successful forecasts of weather, relating to both temperature and rainfall, have been made several months in advance for various localities in the United States.

The connection thus established between solar and terrestrial phenomena may prove useful in long-range weather forecasts for regions where conditions are relatively stable, but it seems hardly likely that such forecasts will ever be very reliable for so variable a climate as that of Great Britain. Apart, however, from all meteorological possibilities, the existence of a definite period in the variations of the solar constant of radiation is of great interest from the standpoint of the sun regarded as a typical star of spectral class G.

PHYSICS. By W. N. Bond, M.A., D.Sc., F.Inst.P., The University, Reading.

The Velocity of Light.—A report has now been published (*Astrophys. J.*, **82**, 1, 26–61, July 1935) of the measurements of the velocity of light in a partial vacuum, that were carried out by A. A. Michelson, F. G. Pease and F. Pearson between September 1929 and March 1933.

The name of Michelson will always be associated with his outstanding work on interferometry and on the velocity of light. About fifty years had passed since Professor Michelson made his first measurements of the velocity of light. The results that he obtained in 1879 and 1882, namely 299,910 ± 50 and 299,853 ± 60 km./sec., were of remarkable accuracy. Apart from Newcomb's measurement made in 1882 (299,860 ± 30), Michelson's results remained unsurpassed for about forty-five years. Then, in 1926, he was able to carry out a further determination by the rotating mirror method, in which a considerable improvement was made.

In measuring the velocity of light by the rotating mirror method, it was usual for the beam of light to be reflected from a rotating mirror, pass to a distant stationary mirror, and on its return be again reflected by the rotating mirror. The subsequent path of the beam did not quite coincide with its original path, owing to the change that had occurred in the position of the rotating mirror. In such experiments three things had to be measured: the distance to the fixed mirror, the angular velocity of the rotating mirror and the rather small angle between the original and final paths of the light.

In the newer form of apparatus, the rotating mirror consisted of a regular prism with four or more faces. The speed of rotation was so large that the light, on returning from the distant mirror, impinged on the *succeeding* (or some later) face of the prism. It was arranged that the original and final paths of the light almost coincided. During the time taken for the light to pass to and from the distant mirror, each face of the rotating mirror had moved nearly to the place formerly occupied by the preceding face. The angles between adjacent faces of the rotating mirror must be almost exactly equal; but the angle between the two beams of light now only appears as a small term in the calculation.

By 1921 preliminary experiments had been carried out using the new method; and between 1924 and 1926 an extensive series of measurements was made, between Mount Wilson and Mount San Antonio (*Astrophys. J.*, **65**, 1, 1927). The final result may be given as 299,796 $\pm$ 4 km./sec. (R. T. Birge, *Nature*, **134**, 771, Nov. 1934), indicating a very considerable increase in accuracy.

There were two reasons why Michelson thought that it was desirable to repeat these experiments. In the first place, it was felt that a short straight base line on level ground could be more accurately measured than the distance between the tops of two mountains. And, secondly, it was advisable to carry out the measurements in air at quite a small pressure, so that only a very small and easily estimated correction would be required to deduce the velocity of light in a vacuum. Professor Michelson planned, therefore, to have the experiment carried out in a straight tube, from which most of the air had been removed. This arrangement would have the additional advantage of giving a small, well-defined image, unaffected by atmospheric disturbances.

Michelson proposed and planned the work, obtained funds for the project and lived to see the apparatus installed; but he was unable to take part in the measurements. The apparatus was erected at the Irvine Ranch near Santa Ana, California, and the

measurements were carried out by F. G. Pease of Mount Wilson Observatory and F. Pearson of the University of Chicago.

Much of the apparatus was installed inside a steel pipe, 26 inches in diameter and a mile long, evacuated to pressures which ranged from 0·5 to 5·5 mm. of mercury. Light from an arc lamp (operating on 110 volts and 40 amperes) passed through a condensing lens, to an adjustable slit about $\frac{1}{300}$ inch wide, and was then reflected by the rotating mirror.

The rotating mirror was of well-annealed optical glass and had 32 faces on its edge. The effective aperture of each of these faces was about $\frac{1}{8}$ inch square. The angles between the faces were correct to 1″ and each face was flat to 0·1 of a wavelength of light. Below the mirror, and on the same spindle, was an air turbine, capable of rotation in either direction. The whole rotating system was supported on plain journal bearings and a single-ball step-bearing below.

The rotating mirror was adjusted into synchronism with a tuning fork, so that it made 585, 365 × 2 or 366 × 2 revolutions per second, according to the particular experiment. The exact frequency of each fork was measured in terms of a pendulum that had formerly been used by the United States Coast and Geodetic Survey. The pendulum was housed in a bronze box in which the air was maintained at a low pressure; and for most of the time the pendulum was enclosed in a constant temperature case. The pendulum was standardised by time signals sent by wireless from Arlington four times each day, using for the comparison a recording chronograph and either a ship's chronometer or a system controlled by an oscillating quartz crystal. It was estimated that the time of rotation of the revolving mirror could be determined in this way to one part in a million.

The light from the rotating mirror entered the evacuated tube through a glass window in the side. After being reflected eight or ten times along the tube it returned to the rotating mirror, and finally formed an image of the slit at the adjustable cross-wire of an eye-piece. The total path of the light was about 8 miles in some experiments and 10 miles in others.

Two modifications of the optical system were tried but abandoned because they were less satisfactory. The experiments had all to be carried out at night, when the air that remained in the long pipe was at a fairly uniform temperature. By doing sets of readings with the mirror rotating in the reverse direction, the necessity for a zero reading was avoided.

The long tube had its axis nearly in a North-East–South-West

line. To form a suitable base line for measurement, six piers were placed in a line parallel to the tube but 10 feet to the North-West of it. The chief distance was measured by the U.S. Survey and also by F. G. Pease, with the following results:

1931	(Garner; 9 Traverses)	1,594,259·2 mm.
1932	(Latham; 8 Traverses)	1,594,265·8 „
1933	(Latham; 31 Traverses)	1,594,272·3 „
1933	(Pease; 8 Traverses)	1,594,263·8 „

The slight changes may indicate a real change in the earth's surface. The maximum disagreement, 13 mm. (or about 1 part in 100,000), was of considerably less importance than disagreements between certain of the velocity determinations, and therefore a mean of the base-line measurements was used for all the final calculations.

Each "set" of observations made with the apparatus, generally gave three separate determinations of the velocity of light, and took about a quarter of an hour to carry out. The total number of separate determinations of the velocity was 2885½. The results of single "sets" of observations deviated from the mean of a small group of sets (or from the mean of all the observations) by about ± 11 km./sec., on the average. Hence we may conclude that each of the 2885½ separate determinations had an average error of about ± 14 km./sec.

There was, however, definite evidence of small systematic errors. This may be illustrated by the arrangement of the results into four groups:

Date.		Number of Determinations.	Mean Velocity.
1931	Feb.–July	493	299,770 km./sec.
1932	March–May	753½	299,780 „
1932	May–Aug.	742	299,771 „
1932	Dec.–1933 Feb.	897	299,775 „
	Total . . =	2885½	299,774 = mean.

In the absence of any systematic errors, the above four mean values would be expected to differ from the final mean by amounts of the order of magnitude $\pm 14/\sqrt{700} = \pm 0{\cdot}5$ km./sec., whereas the differences are actually about ± 3½ km./sec. Repeated measurements of the base line and checks on the clock-rate revealed nothing capable of accounting for the systematic errors; no effect of the instruments was discovered that would explain the variations.

When the velocity measurements were plotted against time,

they appeared to resemble the tidal curve at the coast (about 6 miles away) but leading by 10 hours on the tide. Now the local tide lagged 10 hours behind the lunar force. This suggested that the apparent variations in the velocity of light were partly due to action of the lunar forces. But the direct action of the lunar forces in producing earth expansion or in changing the period of the pendulum (used in timing) is supposed to be too small to account for the effects. Nevertheless, curves of the components of the sun-moon tide forces were computed. A slight correlation with the measurements of light velocity was suggested in the case of the horizontal component perpendicular to the tube. The authors remark "The scattering of the points is so large, however, that it is questionable whether the plot has any real significance." This seems a surprising conclusion. From the considerable number of data plotted in the paper, I deduce the coefficient of correlation as $-0 \cdot 28 \pm 0 \cdot 06$. There seems to be quite definite evidence for a slight correlation; for if it were only an accidental or spurious correlation, it would only be expected about once in 500 occasions!

The authors also find slight indications that the measurements of light-velocity vary with the nearness of the moon, lower velocities tending to occur when the moon is at about its average distance from the earth. I believe that there is not enough evidence to support this conclusion, and the authors describe the results as having "low weight."

The more general question of whether the apparent velocity of light is decreasing with time or varying in a periodic way, has formerly been discussed. Reference may be made to a letter from Professor Birge (*Nature*, **134**, 771, Nov. 1934). There seems to be insufficient evidence to support either suggestion.

The Value of the Electronic Charge.—It is well known that there is a rather marked disagreement between the value of the electronic charge deduced from Millikan's oil drop experiment, $(4 \cdot 769 \pm 0 \cdot 005) \times 10^{-10}$ e.s.u., and values deduced by using X-rays to measure a crystal-lattice in terms of the known spacing of a ruled grating (4·804, 4·805 and 4·806). G. Kellström has recently made a determination of the viscosity of air (*Nature*, **136**, 682, 1935) obtaining the value $(1834 \cdot 8 \pm 3 \cdot 0) \times 10^{-7}$. This differs considerably from the value Millikan used $(1822 \cdot 7 \pm 0 \cdot 9)$, and would change the oil drop estimate of e to $4 \cdot 816 \pm 0 \cdot 013$, in agreement with the X-ray estimate.

K. Shiba had previously suggested that Millikan had assumed too low a value for the viscosity (*Inst. Phys. and Chem. Research, Tokyo, Sci. Papers*, **19**, 97, 1932). It may be felt that the viscosity

data leave the question unsettled. There seem, however, to be three reasons for believing that there is a real discrepancy between the two methods of measuring e. Firstly, a recent determination based on the automatic counting of more than 1,000,000 α-particles (E. Schopper, *Zeit. für Phys.*, **93**, 1, 1935) gives a value $4 \cdot 768 \pm 0 \cdot 005$, agreeing well with Millikan's value. (Schopper later found variations in the e.m.f. of his standard cell, and decided to repeat the experiments: *ibid.*, **94**, 649, 1935.) Secondly, if e be assumed to have the higher value, other discrepancies will be produced or increased (see Birge, *Phys. Rev.*, **48**, 918, Dec. 1, 1935, and *Nature*, **137**, 187, 1936). Thirdly, Eddington's theories give, directly, $e/m = 1 \cdot 770 \times 10^7$ and $e = 4 \cdot 776 \times 10^{-10}$, agreeing with Millikan but disagreeing with the experimental values of e/m; but when modified by a factor 136/137, the values become $1 \cdot 757 \times 10^7$ and $4 \cdot 811 \times 10^{-10}$, agreeing with the experimental values of e/m and giving almost the X-ray value for e (Bond, *Nature*, **135**, 825, May 1935). It seems, therefore, that the discrepancy in the e values is not experimental, but due to incomplete theory, and that the omission of theoretical factors such as 136/137 may be the cause of some of the other discrepancies (mentioned by Birge) that still remain. This point of view is supported by Eddington, who states (*Proc. Roy. Soc.*, **152**A, 269, 1935), that according to his theory the factor 136/137 "vitiates many of the so-called observational constants."

THE NUMBER OF PARTICLES IN THE UNIVERSE.—The gradual welding together of theoretical physics into a single structure has been achieved largely by finding that certain important physical quantities are related to one another. One of the simplest instances is the discovery that the ratio of the electro-magnetic and electrostatic units of charge is equal to the velocity of light in a vacuum. A more complex instance is Bohr's expression of the Rydberg spectroscopic constant in the form $2\pi^2 e^4 m/h^3 c$.

Basing his work on Relativity theory and on a modification of Dirac's equation for an electron, Sir Arthur Eddington has suggested relationships between the masses of electron and proton; between Planck's constant and the electronic charge ($hc/2\pi e^2 = 137$); and between the rate of recession of spiral nebulæ and certain fundamental physical constants. These suggestions have not, as yet, met with anything like universal acceptance. There is reason to believe that their importance will gradually be realised. As long ago as 1929 (*Naturwiss.*, June 28) Professor Sommerfeld remarked in this connection that "the most complete solution of a problem mathematically is often the one found in practice."

In a recent paper (*Proc. Roy. Soc.*, **152A**, 253–72, 1935) Sir Arthur develops his theories further. (A brief account appeared in *Monthly Notices, Roy. Ast. Soc.*, **95**, 636–8, June 1935.) Using experimental values of certain atomic constants, together with the value of the gravitational constant, he deduces that the number of particles in the Universe is

$$N = 1{\cdot}573 \times 10^{79} = 135{\cdot}82 \times 2^{256}$$

to about $\pm$ 1 or 2 parts in 1000. There are supposed to be N/2 protons, N/2 electrons; and positrons and negatrons count as minus electrons and protons. Eddington remarks that "it seems probable that the exact value of N is 136×2^{256}." He was helped to reach this conclusion by Fürth's earlier suggestion that the core of the number N is 2^{256}; by the association of the numbers 256 and 136 with the double wave function; and by the fact that N must be an integer. The first indications of the present suggestions can be seen in an earlier paper (Eddington, *Proc. Camb. Phil. Soc.*, **27**, 15, 1930).

THE SURFACE TEMPERATURE OF SLIDING METALS.—F. P. Bowden and K. E. W. Ridler give a preliminary account (*Proc. Camb. Phil. Soc.*, **31**, 431–2, July 1935) of experiments on the surface temperature developed between two metals in sliding contact. By the ingenious device of using the contact of the two metals as a thermocouple, they are able to show that the temperature rises as the speed of sliding is increased, till it attains a maximum temperature which is equal to that required to melt the more fusible of the two metals.

METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

IN the *Quarterly Journal of the Royal Meteorological Society* for July 1935 there is a paper by Dr. A. E. M. Geddes entitled "Temperature Trend at Aberdeen from 1870 to 1932" which constitutes an important addition to the literature dealing with a subject that has occupied a good deal of attention in recent years—the question whether the climates of the world, or at least of certain parts of the world, are changing.

Mr. J. B. Kincer of the Weather Bureau, Washington, D.C., has studied the temperature trends for a large number of places in America and in some other parts of the world, and has discussed the results in detail in a paper published in the *Monthly Weather Review* (**61**, No. 9, Sept. 1933) under the heading "Is our Climate Changing? A Study of Long-time Temperature Trends." It has

been a matter of common opinion that mild winters have been rather frequent this century and that severe winters like some that were experienced last century have been correspondingly scarce, both in the United States and in the British Isles, and it is of considerable interest to see to what extent the recorded temperatures support this impression.

Kincer employed a method of analysing temperature records that eliminates to some extent any short-period fluctuations that may be present. Graphs were employed which show twenty-year summations of annual mean temperature that advance a year at a time. Thus, the first point is the sum of the annual means for the first twenty years of the record, the second point is obtained by subtracting from this the annual mean of the first year and adding that of the twenty-first, and so on. The method is excellent for showing whether a long-period trend is present or not but it has the drawback that an exceptionally warm or cold year produces a rather marked change in the graph twenty years later when it drops out of the summation and there is a tendency to attribute such a change incorrectly to the new year that has just been added, the result being uncertainty as to when any long-period trend begins and ends.

Kincer, referring to the indications of these graphs, writes as follows: "This study shows that temperature trends in middle latitudes of the northern hemisphere and also, though less pronouncedly, in the southern hemisphere, have been prevailingly high for a long time. . . . There is a somewhat irregular, but very definite, upward swing in the curves, shown to have been in progress for more than half a century; and there is as yet no evidence of a recession. The records for the different seasons of the year show that the winters are the most erratic, with up-and-down trends of greater frequency and shorter duration than the other seasons. For the spring and fall the trends have been more uniformly upward, with fewer interruptions by short cold spells. The curves for the fall season show a remarkably steady upward trend for nearly a century; that is for nearly a hundred years our fall seasons have been trending progressively to warmer . . . for the fall, winter and spring seasons the average in temperature for the twenty years to and including 1933 are from 2·5° to nearly 4° F. higher than similar averages sixty or seventy years ago. Temperature records of other countries of the northern hemisphere, and also of the southern hemisphere, show strikingly similar conditions."

The longest of the records used is that for New Haven, Connecticut, beginning in 1780, the curve for which is plotted

close to that for Copenhagen which begins in 1798. It must be admitted that the graphs of twenty-year summations for these two stations show little resemblance in the case of those periods ending earlier than about 1850, and that while each curve lies above its average for totals ending after 1907, there was a rise throughout the New Haven curve during that period which was not maintained at Copenhagen beyond the twenty years ending in 1914. The graphs for places in the southern hemisphere—in Chile, Argentina and Cape Town—do not go back nearly so far in time, the first point shown being that covering the twenty years to 1892, and the last point the twenty years to 1930. One can go so far as to say that at all three places the average level of the curve in the more recent half of the curve is higher than that for the earlier half, but apart from this the curve for Cape Town does not greatly resemble the others, and the differences in the annual means for the two halves of the period is only of the order of half a degree (Fahrenheit). On the whole, however, there seems to be a strong case in favour of a gradual increase of temperature of the kind suggested and no definite suggestion that this increase is drawing to a close. It has been plausibly suggested that the effect is an artificial one, due to the fact that most of the long records that show it are made in large towns where the air temperature is affected by the heating of the houses, and that this effect has become larger in accordance with the increasing size of the towns. The suggestion is the more plausible in that it is generally recognised that the mean temperature within a very large town is normally two or three degrees higher than that of rural places round about, but it is easily tested by comparing the temperature trend in a large town with that of a neighbouring country place. Kincer has done this for a number of pairs of temperature records and in each case finds that both town and country have varied in a very similar manner. The pair covering the longest period is Philadelphia and West Chester, for which simultaneous records are available for the seventy-eight years 1855 to 1932. Not only do the lesser ups and downs and the general trends resemble each other, but there seems no evidence that the change in degrees is any larger on an average for the towns. Geddes has carried out a similar analysis to those made by Kincer by the method of moving twenty-year summations of temperature, for the record maintained at Aberdeen Observatory with the aid of thermometers housed in a screen 40 feet above the ground on the north wall of a building.

The first point on his graph is for the twenty years ending in 1889. He writes of this as follows: " After a short slightly down-

ward trend, the graph takes a very decided upward trend until the period ending in 1915 is reached. Thereafter the tendency is much less marked, though from 1919 onwards it is again distinctly upwards. This figure therefore apparently indicates a tendency for the climate to become rather milder, though that tendency has of late years become less marked."

He then goes on to consider the four seasons separately as did Kincer for some of the American records, but with very different results. At Aberdeen the progressive increase of temperature is most clearly shown in winter, while in summer it is almost non-existent. The analysis is carried still further, to individual months, and to arrive at greater precision the twenty-year summations were set aside and a table was constructed showing side by side individual monthly means for three equal periods of twenty-one years, *viz.* 1870–90, 1891–1911, and 1912–32. His summing up of this more detailed analysis is as follows: "When we sum up the evidence from the four seasons taken month by month we find that in the period considered certain changes in mean temperature have shown themselves. Spells of warm seasons and of warm months have occurred, but these spells have not occurred simultaneously in all seasons nor in all months of the same season even. A marked feature is the spell of milder winters during the latter half of the 63-year period. There is no real evidence, however, to show that this change is progressive, nor any guarantee from a consideration of temperature alone that a return to colder conditions will not take place. The increase in the number of houses in the neighbourhood of the station and the increase of fuel consumption in the city in the winter months during the last thirty years might be considered as having some slight effect on the mean winter temperature. Any such effect would be purely local, however, and conditions similar to those experienced in Aberdeen have been encountered in the north-eastern district [of Scotland] generally. The cause of the milder winters must be looked for elsewhere."

In the course of a discussion that followed the reading of Geddes' paper Dr. C. E. P. Brooks, who has made a number of investigations into long weather records for various parts of the British Isles, stated that in his opinion there is no doubt that a real change of climate took place about the beginning of this century practically throughout the British Isles, the difference between the average winter temperature for 1901 to 1930 exceeding that for 1871 to 1900 in most places by more than 1° F., and that the change in Central Europe is even greater. He attributed part of the change to an increase in the frequency of south-westerly winds—an explana-

tion which, it should be noted, is consistent with the comparatively small change in summer, when south-westerly winds show little or no tendency to be associated with temperatures above the average—and attached little importance to the fact that the change of temperature appeared to occur in different years when individual months are considered, on the ground that irregular variations of short period are sufficiently large to cause displacements of the time of change of the magnitude of those observed at Aberdeen. As to there being any certainty that a return to cooler conditions may not take place at any time, probably all will agree that there is no such certainty, for the ultimate cause of the setting in of the milder climate is unknown and there can therefore be no basis for predicting when the removal of the cause may take place.

BIOCHEMISTRY. By W. O. Kermack, M.A., D.Sc., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

The Nature of Viruses.—The difference between living and dead matter has always been of great interest to the ordinary man, as well as to the scientist. Biochemistry is concerned with the rôle of chemical processes in living material, and so it might well be expected to have something to say on the general question as to what the phenomenon, known as life, really is, and to what extent it can be explained in terms of those laws of physics and chemistry which have been found to hold in ordinary inanimate systems.

When considering this question, it seems natural to turn our attention to the systems which exhibit the phenomena characteristic of life, but which, nevertheless, are of the simplest possible kind. We are thus led to consider that mysterious group of agents which includes the viruses and bacteriophages; and any work which sheds light on their essential nature is worthy of very close attention.

Viruses appear to be the causal agents in a wide variety of human, animal and plant diseases. They differ from bacteria in being of smaller size, and in being able to pass through the filter candles which retain ordinary bacteria. They are also invisible in the ordinary microscope even under the highest powers. On the other hand, they are able, like the bacteria and other living things, to reproduce themselves, but, at least up to the present, it has not been found possible to grow them in any culture medium in the usual way; it would seem that they can only grow in the living cell. In the same way, bacteriophage, the agent which is characterised by its power of bringing about the lysis, or solution, of bacteria, is also capable of apparently unlimited multiplication,

but only in a culture of actively growing bacteria, which it is able to lyse.

The properties of the plant viruses have been discussed recently in this journal in a very interesting article by Smith (SCIENCE PROGRESS, 1936, **30**, 413) in which will be found an account of some of the recent work which has been done in this field, more especially on the biological side.

A number of recent papers have dealt with the question of the chemical nature of the viruses and especially the virus of tobacco mosaic disease. Evidence has been accumulated to show that protein is an essential constituent of these agents, as, for example, by the study of the effect on them of various proteolytic enzymes. The results are somewhat conflicting, but Stanley (*Phytopath.*, 1934, **24**, 18 and 1055) has recently shown that pepsin definitely acts on the virus, and inactivates it at a pH at which the digestive action of the pepsin is most pronounced. The effect seems to depend on the hydrolysis, either of the virus itself, or of something necessary for its activity. On the other hand, although trypsin, when added to the virus, appeared to inactivate it, it has been shown that this is not due to any hydrolytic action on the agent, but results from the fact that preparations of trypsin have the curious effect of increasing the resistance of the plant leaves when in contact with them. As few native proteins are hydrolysed by trypsin until after they have been acted upon by pepsin, the above results are consistent with the view that the virus contains protein as an important constituent. This conclusion is supported by the work of Chester (*Phytopath.*, 1934, **24**, 1180), who has shown that the virus, when injected into rabbits, produces antibodies which specifically neutralise its action on plants. It is generally agreed that substances, which produce antibodies, usually contain protein as an essential constituent, and so this piece of evidence, though not conclusive, is of considerable weight.

Stanley (*Science*, 1935, **81**, 644) claims to have separated, from the infected juice of tobacco plants suffering from mosaic disease, a crystalline protein, apparently homogeneous in character, possessing, in very high degree, the characteristic action of the virus, namely the power to infect other plants specifically with the disease. It is not necessary here to describe in detail the methods employed by Stanley for the isolation and crystallisation of the protein. It is sufficient to state that it consists essentially of precipitation at or about the isoelectric point. The product consists of small needles about 0·03 mm. in length, and it appears to be homogeneous, though it is naturally very difficult to prove this definitely. The

protein is one hundred times more active than the suspension made by grinding up diseased Turkish tobacco leaves, and one thousand times more active than the twice frozen juice from diseased plants. One cc. of a 1/1,000,000 dilution of the crystals is usually infective, and the disease produced cannot be distinguished from that which occurs naturally. Further, the immunological properties of the protein are very interesting. The sera of animals, injected with tobacco mosaic virus, give a precipitate when mixed with a solution of the crystals diluted to one part in 100,000, whereas the sera of animals injected with the juice from healthy tobacco plants give no precipitate when mixed with a solution of the crystals. Conversely, solutions of the crystals, injected into animals, cause the production of a precipitin, which is active with solutions of the crystals and with the juice of plants infected with tobacco mosaic virus, but which is inactive with the juice of normal plants. It is not stated, however, whether such an anti-serum definitely inhibits the infectious action of the virus on the plants. Nevertheless, the high activity of the crystals, which remains apparently constant throughout many recrystallisations, the apparent homogeniety, together with their immunological properties, give a very strong, though not conclusive, support to the view that this protein is actually the virus. The alternative hypothesis is that the protein adsorbs very tenaciously a minute quantity of an extremely active agent. This possibility is very difficult to disprove, and further work will be required before a definite conclusion can be reached.

Though the idea that an active virus might be isolated as a crystalline protein is, at first sight, very surprising, yet the possibility is one which is quite consistent with the known physical properties of viruses. Thus native proteins are known with molecular weights ranging from about 34,000 up to several millions and the diameters range from about $5\mu\mu$ to about $30\mu\mu$. These diameters are of about the same order of magnitude as those ascertained by Elford for the smaller viruses and bacteriophages. Measurements of osmotic pressure, or of diffusion, show that the crystalline protein isolated by Stanley has a molecular weight of the order of several millions, so that in size, this protein molecule is certainly larger than the smallest known virus. It is therefore clearly quite possible for a crystalline protein to have sufficiently large molecules to accommodate the necessary molecular complexity of a virus. In fact it might be expected that an agent, obviously so homogeneous and specific as a virus, and of a molecular size of the order of which it is known to have, would crystallise

if conditions were favourable. That the protein isolated by Stanley is really this active agent does not, of course, necessarily follow.

CRYSTALLINE ENZYMES.—Along with viruses which have many of the properties characteristic of life, it is natural to consider enzymes; for these are intimately associated with vital processes, and form indispensable constituents of living cells. It is therefore interesting to note that during recent years a number of claims have been made to have isolated enzymes in a crystalline form. In 1926, Sumner (*J. Biol. Chem.*, 1926, **69**, 435) announced that he had succeeded in crystallising urease, and though this claim has met with considerable criticism, especially from continental workers, there would seem to be a substantial volume of evidence in support of the truth of Sumner's contention. There is good reason to believe that the crystalline protein, isolated by Northrop and his collaborators from bovine gastric juice as well as from a commercial pepsin preparation, is actually the enzyme pepsin itself in a pure form (*Ergebniss. d. Enzymeforsch.*, 1932, **1**, 302 and *J. Gen. Physiol.*, 1933, **16**, 615) (however, *cf.* Waldschmidt-Leitz and Kofranyi, *Naturwiss.*, 1933, **10**, 206), whilst, more recently, Kunitz and Northrop (*J. Gen. Physiol.*, 1933, **16**, 267) have published a number of papers dealing with the crystallisation of trypsin and associated enzymes. Thus trypsin is said to crystallise in rectangular prisms and fine needles. Chymotrypsinogen (*Science*, 1933, **78**, 558), which forms elongated needles, is itself inactive, but, when acted upon by trypsin, it yields chymotrypsin, which crystallises in the form of plates. This latter enzyme has a relatively weak proteolytic effect but is highly active in curdling milk. Trypsinogen (*Science*, 1934, **80**, 505), the precursor of trypsin, has also been described as forming short triangular prisms.

Crystalline pepsin and trypsin both show a very interesting phenomenon. Being proteins, they may be denatured by heat or other means, but under suitable conditions the denaturation process may be reversed, and the native protein regenerated. As might be expected, denaturation of these proteins results in loss of their enzyme activity. It is much more remarkable that reversal of the denaturation process should result in the proteins regaining their original proteolytic powers. Northrop believes that he has established this reversal of enzyme activity, and the result, if true, is of especial interest, for it has a bearing, both on the protein nature of the enzymes, and on the reversibility of the process of denaturation.

Another very interesting, and somewhat related phenomenon,

has been described by Herriot and Northrop (*J. Gen. Physiol.*, 1934, **18**, 35) in a paper dealing with the acetylation of crystalline pepsin by means of ketene. This reagent acts under very mild conditions, and various numbers of acetyl groups may be introduced into the pepsin molecule. When the product contains 3–4 acetyl groups per mol (the molecular weight is approximately 35,000), it still retains the original enzymatic activity practically intact. As more and more acetyl groups are introduced, the activity gradually falls, until when the product contains 20–30 groups per mol, it retains only 10 per cent. of the original activity, though, like the other less highly acetylated product, it is still crystallisable. It is shown that the first 3–4 acetyl groups attack the primary amino groups, and that afterwards, other positions in the protein molecule become acetylated. Thus the acetylation of the free amino groups is accomplished without reduction in enzyme activity. When the partially acetylated protein, containing 6–11 acetyl groups per mol, which is 60 per cent. active, is allowed to stand in strong acid solution, partial de-acetylation takes place accompanied by increased proteolytic activity. The recovered protein is probably identical with the 3–4 acetyl derivative mentioned above, and its enzyme activity is equal to that of the original protein. These results may be compared with those obtained by Freudenberg and Dirscherl (*Zeit. Physiol. Chem.*, 1928, **175**, 1), who found that acetylation of crystalline insulin, also a protein of molecular weight about 35,000, results in loss of physiological activity, but that de-acetylation partially restored its former action.

All the crystalline enzymes so far mentioned, it should be noted, are hydrolytic in their action. This is also true of the crystalline pancreatic amylase which Caldwell, Booher and Sherman (*Science*, 1931, **74**, 57) claim to have isolated as elongated isotropic needles. However, Theorell (*Biochem. Zeit.*, 1934, **272**, 155) has described the isolation, in a crystalline form of the Warburg yellow oxidation enzyme which, as its name implies, is concerned with tissue oxidations, whilst oxyhæmoglobin, though not usually regarded as an enzyme, has a well-marked peroxidase activity and it is, of course, well known that this protein can be readily crystallised.

Many chemists have been inclined to be sceptical about the interpretation of work such as has been described above. The alternative hypothesis that these alleged enzymes are really inert proteins with small quantities of highly active substance adsorbed on them is very difficult to disprove. However, the cumulative evidence, which is now available as the result of quite a large amount of work, does seem to make it probable that certain

enzymes at least can be obtained in a crystalline form, and this in turn makes it easier to accept, at least provisionally, the claim by Stanley that he has isolated a crystalline virus.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., The University, Glasgow.

Stratigraphical and Regional Geology—Extra-British.—The structure of the Sarre and Lorraine coalfield, as expounded by P. Pruvost in a new memoir (*Études des gites minéraux de la France*, 1934, 174 pp.) is that of a synclinorium composed of a series of folds, synclines and anticlines aligned along the main axis of the basin, and produced by movement from north to south.

Dr. Maria M. Ogilvie Gordon's exhaustive memoir "Geologie von Cortina d'Ampezzo und Cadore" (*Jahrb. d. Geol. Bundesanst. Wien*, **84**, Heft 1–4, 1934, pp. 59–215) does not lend itself easily to summarisation. The formations dealt with are the Crystalline Basement rocks, Permian, Trias, Jurassic and Cretaceous. The complicated tectonics of the region are very fully described.

S. Hjelmqvist's memoir, "Zur Geologie des Südschwedischen Grundgebirge. Die kristallinischen Gesteine des Romeleåses" (*Medd. Lunds Geol.-Min. Inst.*, Nr. 58, 1934, 183 pp.), deals with the southernmost Archæan inlier in southern Scania. This inlier, 20 miles long by 3 miles wide, strikes in a N.W. to S.E. direction. It consists of orthogneisses enclosing fragments of older leptite-gneisses which are both cut by amphibolite dikes. Next come two successively-intruded granite masses, and the whole complex has finally been riddled by a great number of later dikes, mainly of diabase.

The memoir by the late J. J. Sederholm, "On Migmatites and Associated Pre-Cambrian Rocks of South-Western Finland. Part III. The Åland Islands" (*Bull. Comm. Géol. Finlande*, Nr. 107, 1934, 68 pp.), is of melancholy interest as it is the last work from Sederholm's able and prolific pen. As its subject is mainly petrological it will be dealt with more fully under the heading of "Metamorphism" in a later instalment of these notes.

In his memoir "Tausend Geschiebe aus Lettland," P. Eskola (*Ann. Acad. Scient. Fenn.*, Ser. A, XXXIX, 1933, 1–41) describes the results of his examination of glacial boulders from Latvia. He finds that the proportions of petrological and formational types are approximately the same as those between the areas covered by these rock-types in Finland, although gneisses and migmatites are relatively more abundant than schists. Professor Eskola points out how important boulder investigations may be for the deter-

mination of the constitution of the floors of the Baltic Sea and the Gulf of Bothnia.

A valuable paper by S. V. Semichatov (*Geol. Mag.*, LXXII, 1935, 433–41) summarises recent work on "The Middle Carboniferous of Russia." The author attempts to solve the question whether the Moscovian stage is or is not equivalent to the whole of the Middle Carboniferous of the Russian platform, and comes to the conclusion that the major stratigraphical unit known as the Middle Carboniferous does not coincide in time with the Moscovian stage as classically developed in the Moscow region.

Lauge Koch has briefly summarised the "Geologie von Grönland" in the new series of regional memoirs entitled *Geologie der Erde* (Berlin, 1935, 159 pp.). The value of this work, however, has been gravely questioned on the grounds of alleged incompleteness, inaccuracy and inexact working methods in a long review entitled "Remarks upon Lauge Koch's Geologie von Grönland, 1935" (*Medd. Dansk Geol. For.*, **8**, 1935, 497–511) over the signatures of O. B. Bøggild, R. Bøgvad, K. Callisen, H. Frebold, H. Gry, K. Jessen, V. Madsen, A. Noe-Nygaard, C. Poulsen, A. Rosenkrantz, and C. Teichert, all of whom have had experience of Greenland geology.

Dr. C. Teichert's geographical memoir "Nordöstgrönland" (*Zeitschr. f. Erdkunde z. Berlin*, 1935, 169–215) contains an admirable summary of the geology of that region.

In response to Dr. L. Koch's query "Do you believe in a Caledonian orogeny in East Greenland?", Dr. C. E. Wegmann has written an illuminating memoir entitled, "Preliminary Report on the Caledonian orogeny in Christian X's Land, North-East Greenland" (*Medd. om Grønland*, **103**, No. 3, 1935, 59 pp.). His conclusions may be given in his own words: "The sediments of the [Caledonian] chain indicate that they were collected in a subsiding trough; they comprise a thickness of about 9000 m. During the Caledonian orogeny large masses were transformed by igneous activity. A kinetic analysis shows an upper structure of wide folds in the non-metamorphic sediments, and a deeper-lying structure of migmatitic flowing-folds. On top of this, above a deep gap, we get the Devonian molasse. Flysch formation could not be ascertained. But this missing Flysch being an element of great importance, the Caledonian mountain-chain cannot be looked upon as a complete mountain-complex, but only as an enormous fragment. It is of great vertical extent and has consequently to be looked upon as a deep-going interruption of the old continental block."

In his memoir "Some New Investigations of the Devonian Stratigraphy and Tectonics of East Greenland," H. Bütler (*Medd. om Grønland*, **103**, No. 2, 1935, 35 pp.) shows that the Old Red Sandstone of East Greenland is an orogenic sediment. East-west movements within the Caledonian belt of folding repeatedly elevated, folded and disrupted the Devonian sandstones during their deposition.

A detailed investigation of the "Geology of the West End of Ymer Island, East Greenland" has been carried out by A. B. Cleaves and E. F. Fox (*Bull. Geol. Soc. Amer.*, **46**, 1935, 463–88). A valuable discussion of the origin of the fjords of north-east Greenland is included.

A. Noe-Nygaard describes the stratigraphy of an area around Fleming Inlet, East Greenland (*Medd. om. Grönland*, **103**, No. 1, 1934, 88 pp.). A crystalline basement series is followed by formations of Devonian, Carboniferous, Permian, Trias, Rhætic and Jurassic ages. Igneous activity and tectonic disturbances took place in the Late Palæozoic and in the Tertiary.

Extremely valuable results have accrued from the thorough study by O. Kulling of the Hecla Hoek formation in its most typical development around Hinlopen Strait, Spitsbergen (*Geogr. Ann.*, Stockholm, **4**, 1934, 161–254). The C. Hansteen formation at the base of the series, consisting of porphyries, agglomerate and tuff, is shown to be of Pre-Cambrian age. It is followed by slates and sandstones with overlying dolomites—the Murchison Bay formation—with traces of fossils. Then comes a thin series consisting of beautiful tillites and varved sediments (Sveanor formation), believed to be of Eo-Cambrian age. Finally the C. Sparre formation of quartzite, shale and dolomite, contains recognisable fossils of Cambrian, probably Lower Cambrian, age.

K. Keilhack's paper, "Beiträge zur Geologie der nordwestlichen Halbinsel von Island" (*Zeits. d. Deutsch. Geol. Ges.*, **85**, 1933, 621–30), deals mainly with the thickness and age of the Tertiary basalt lavas. The average thickness of the flows works out at about 45 feet. The discovery of bauxite and laterite suggests a parallelism of the succession in North-West Iceland with that in Antrim. The two main ice caps of the region appear to have greatly diminished in area during the last century.

R. Pfalz's "Die Hauptzüge im geologischen Bau Italienisch-Libyens" (*Geol. Rundsch.*, XXV, 1934, 111–34) is a summary and discussion of recent work.

Dr. K. S. Sandford has detailed his "Geological Observations on the North-West Frontiers of the Anglo-Egyptian Sudan and

the Adjoining Part of the Southern Libyan Desert," made on Major Bagnold's recent expedition (*Quart. Journ. Geol. Soc.*, XCI, 1935, 323–81). He describes an Archæan and Pre-Cambrian complex, thick sandstones of Palæozoic age, followed by Nubian Sandstones after a marked unconformity. Dr. Sandford thinks that the term "Nubian Sandstone" should be applied only to the last-mentioned series of sandstones. Two volcanic fields and some isolated outcrops of post-Nubian hypabyssal rocks were also encountered.

In a paper on "The Structure of the Esh-Mellaha Range (Eastern Desert of Egypt, 27° 30′–28° N.)," G. Andrew (*Bull. Inst. Egypt*, XVI, 1934, 47–59) shows that this range is a Pre-Miocene feature which was partially or wholly submerged in the Miocene sea. Professor Andrew also describes a series of rocks, mainly igneous, from the South-Eastern Desert of Egypt and West-Central Sinai (*ibid.*, XVII, 1935, 205–21).

W. Pulfrey describes the geology of an area in the Kavirondo district of Kenya (*Geol. Mag.*, LXXIII, 1936, 26–38). A large roof-pendant of Muva-Ankolean sediments rests on the great Nyanza granite batholith, the metamorphic effects of which are described. The sediments have also been invaded by two earlier granitic bosses.

E. Polinard's memoir, "Le Socle ancien inférieur à la Série schistocalcaire du Bas-Congo" (*Mem. Inst. Roy. Col. Belge*, Sect. Sci. Nat. et Med. tom. II, fasc. 4, 1934, 100 pp.), is a study of the rocks exposed along the railway from Matadi to Leopoldville. They consist of a series of granitoid gneisses, paragneisses and schists, of which the degree of metamorphism decreases from west to east. The petrology, stratigraphy, tectonics and metamorphism of these ancient basement rocks are dealt with in some detail.

The "Carte géologique de l'Angola. Notice explicative," by F. Monta and H. O'Donnell (*Portuguese Ministry of Colonies. Colony of Angola*, Lisbon, 1933, 87 pp., 12 pls. and coloured map), represents a welcome addition to the number of excellent geological maps of Central Africa which have lately appeared. The map, on a scale of 1 : 2,000,000, shows 22 formations the stratigraphical relations of which are discussed in the accompanying text. Another memoir on the geology of Angola is that by P. F. W. Beetz, "Geology of South-west Angola between Cunene and Lunda Axis" (*Trans. Geol. Soc. S. Afr.*, XXXVI, 1934, 137–76).

The following are two papers on the stratigraphical geology of South African areas : L. T. Nel, "The Witwatersrand Series outside the Rand" (*Trans. Geol. Soc. S. Afr.*, XXXVI, *Proc.* 1934, xxiii-

xlviii); L. J. Krige, "The Geology of Durban" (*ibid.*, XXXV, 1933, 37–68).

The following are three papers on broad structural problems in the geology of Asia, the two last being more or less compilatory: H. G. Backlund, "Zur tektonischen Gliederung Asiens" (*Geogr. Ann.*, Stockholm, 1935, 242–54); S. Obruchev, "Der Bau von Nordöst-Asien nach neueren Forschungen" (*Geol. Rundsch.*, XXV, 1934, 388–422); K. Gundlach, "Das Pamir-System (auf Grund neuerer Literatur)" (*ibid.*, 330–55).

Dr. W. D. West of the Geological Survey of India has published an exceedingly valuable series of articles under the general title, "Some Recent Advances in Indian Geology" (*Current Science*, III, 1934–35). Part I (pp. 137–44) deals with "The Archæan Rocks of Peninsular India"; Part II (pp. 185–8) with "Deccan Trap Volcanic Activity"; Part III (pp. 286–9) with "The Geology of the Himalaya"; Part IV (by V. P. Sondhi, pp. 339–46) with "The Geology of Burma"; and Parts V and VI (pp. 412–16) with "The Geology of the Salt Range," the series concluding with an Epilogue. This series will prove of great value to geologists both within and beyond the boundaries of the Indian Empire.

Dr. D. N. Wadia has published a memoir on "The Cambrian-Trias Sequence of North-western Kashmir (Parts of Muzaffarabad and Baramula Districts)" (*Rec. Geol. Surv. India*, LXVIII, Pt. 2, 1934, 121–76). This Palæozoic synclinorium, with its outliers of Trias, lies in the centre of the Kashmir *nappe* between two orographic lines.

Dr. L. R. Wager has contributed a chapter entitled "A Review of the Geology and Some New Observations" to the book, *Everest 1933* (pp. 312–36). He deals with the scenery and its development, and the mechanism of formation of the Himalayas. He regards the latter as due in the first place to the formation of a high plateau by horizontal compression folding, and second, upward arching of the southern border of the plateau to give the localised belt of high peaks.

In his Presidential Address to the Geological Section of the 22nd Indian Science Congress, Calcutta, 1935 (29 pp.), Dr. M. S. Krishnan dealt with "The Dharwars of Chota Nagpur—Their Bearing on Some Problems of Correlation and Sedimentation." The Dharwar System is one of the basement series of Pre-Cambrian rocks in India. A correlation between different regions is attempted, and Dr. Krishnan includes a valuable discussion of some of the unusual types of sediment found in the Dharwar, namely, the

marbles, carbonaceous phyllites, manganiferous rocks and banded hæmatite-quartzites.

According to C. S. Pichamuthu the conglomerates and grits belonging to the Dharwar Schist series at Kaldurga, Kadur District, Mysore (*Proc. Indian Acad. Sci.*, II, No. 3, 1935, 254–79), are not of autoclastic origin as formerly believed, but are of true sedimentary derivation, although, in common with the neighbouring schists, they have undergone intense crushing and shearing at a later period. Similarly the quartzites of the Bababudan area, as shown by the occurrence of cross-bedding and well-rounded pebbles, are not crushed and recrystallised acid igneous rocks as formerly thought (*Current Science*, IV, No. 3, 1935, 179–81).

Professor B. Sahni, on the basis of a considerable body of direct palæobotanical evidence and from a newly-discovered collection of silicified palms and dicotyledons, reverts to the early view that the Deccan Traps are of Eocene age, and not late Cretaceous, as maintained by several investigators (*Current Science*, III, 1934, 134–6).

An excellent account of "The Geology of Ceylon" has been prepared by J. S. Coates, formerly Government Mineralogist to the island (*Ceylon Journ. Sci.*, B. **19**, 1935, 101–87). This memoir is well summarised in *Nature*, October 5, 1935, p. 556.

Belated attention may be drawn to a remarkable compilation by many authors on the Stratigraphy and Palæontology of the Dutch East and West Indies in honour of the eightieth birthday of Professor K. Martin ("Feestbundel uitgegeven ter eere van Prof. Dr. K. Martin, 1851–24 Nov.–1931," *Leidsche Geol. Med.*, V, 1931, 739 pp.). These extremely valuable summaries are mostly written in English.

The separate publication entitled "Gravity, Geology and Morphology of the East Indian Archipelago," by P. H. Kuenen, J. H. F. Umbgrove and F. A. Vening Meinesz (*Publ. Netherlands Geod. Comm. Gravity Exp. at Sea*, 1923–1932, Vol. II, The Interpretation of the Results. 1934 (?), pp. 107–94), forms part of Meinesz's second volume of the results of his famous gravity measurements at sea. It contains the chapters concerning the Dutch East Indies, beginning with the last part of Chapter IV which gives a general discussion of gravity results in the Archipelago by Meinesz who, in Chapter V, also attempts an interpretation of the data. Umbgrove follows with a study of the geology of the Archipelago since the beginning of the Tertiary in connection with the gravity results, and a summary of the genetic theories which have been put forward. The final chapter, by Kuenen, discusses the morphology of the Archipelago in relation to the gravity data. This

fine volume is illustrated by a large number of figures (maps) and by magnificent coloured charts.

In an illuminating memoir on the tectonics of the East Indies, P. H. Kuenen (The Snellius Expedition in the Eastern Part of the Netherlands East Indies, 1929–30. Vol. V, Geological Results. Part I, Geol. Interpretation of the Bathymetrical Results. 1935, 124 pp.) states that the echo soundings have shown the existence of very steep and vertical scarps on the sea floor which evidently represent fault-scarps. Comparison of the East Indies with the Alps yields important results. Alpine structure shows what the East Indian sub-structure must be like, and the East Indies illustrate a former stage of Alpine orogenesis. Coarse detritus, however, is not yet being delivered into the East Indian troughs.

Dr. R. W. van Bemmelen applies his undation theory (see SCIENCE PROGRESS, Jan. 1936, p. 491) to various regions of the East Indies in the following papers: "Die Neogene Struktur des Malayischen Archipels nach der Undationstheorie" (*Proc. Kon. Akad. v. Wetensch., Amsterdam*, XXXVI, 1933, 888–97); "De tektonische Structuur van Zuid-Sumatra" (*Nat. Tijdsch. v. Nederlands.-Indie,* XCIV, 1934, 7–14); "Über die Deutung der Schwerkraftanomalien in Nederländisch-Indien" (*Geol. Rundsch.*, XXVI, 1935, 199–226); "Ein Beispiel für Sekundärtektonogenese auf Java" (*Geol. Rundsch.*, XXV, 1934, 175–94). In the last paper van Bemmelen describes a great volcanic complex near Bandoeng which was domed up until the critical slope of its material was exceeded. The dome then spread under its own weight, pressing up at least one broad fold on its northern border. Tensional faults simultaneously intersected the central dome.

On the basis of the occurrence of pre-Tertiary plutonic rocks and more or less metamorphosed volcanics and sediments, H. S. Ladd ("Geology of Vitilevu, Fiji," *Bern. P. Bishop Mus. Bull. 119,* Honolulu, 1934, 263 pp.) erects the hypothesis of a former Melanesian continent which included Australia, New Zealand, the East Indies and the Philippines. This hypothesis receives criticism from R. T. C. (*Journ. Geol.*, XLIII, 1935, 781–3).

A useful outline of the Geology of Victoria has been compiled by Professor E. W. Skeats and other authors (Handbook for Victoria, Australian and New Zealand Assoc. for Adv. of Sci., Melbourne, 1935, pp. 78–135).

The memoir by the late H. T. Ferrar, "The Geology of the Dargaville-Rodney Subdivision" (*Bull. Geol. Surv. N.Z.*, No. 34 (N.S.), 1934, 86 pp.), describes a portion of the extreme northern peninsula of North Island, N.Z. The sedimentary rocks are Trias,

Cretaceous and Tertiary, and there is also an extensive igneous series ranging from Eocene to Recent times.

Two brachipods, one of which has been identified as *Spirifer sp. ind.*, have been found in high-grade metamorphic rocks (calc-silicate-hornfels and garnetiferous mica schist) on Mt. Clough, New Hampshire, by M. P. Billings and A. B. Cleaves (*Amer. Journ. Sci.*, XXX, 1935, 530–6). Along with other lines of evidence this find confirms the view that the metamorphic rocks of western New Hampshire are Devonian and not Pre-Cambrian.

The paper, "Basin-Range Faulting of 1915 in Pleasant Valley, Nevada," by B. M. Page (*Journ. Geol.*, XLIII, 1935, 690–707), records fault scarps of at least three ages, the most recent of which were formed as late as 1915 as the consequence of an earthquake. Two fault blocks rose relatively to the adjacent valley, producing low scarps at the foot of the mountains.

Dr. M. A. Peacock's study of the "Fiord-Land of British Columbia" (*Bull. Geol. Soc. Amer.*, **46**, 1935, 633–96) leads to the conclusion that the apparently opposed tectonic and glacial erosion theories of the origin of fiords can be reconciled. Earth movements, stream and glacial erosion have all played important parts in the evolution of the fiord system of British Columbia. Longitudinal faulting has contributed to the formation of the Island Fringe, and regional subsidence has been partly responsible for submergence of the fiord basins and the Intermont Valley Belt. Powerful glacial excavation has greatly modified the details of the topographic forms.

New observations on the Gondwana Series of southern Brazil are recorded in a memoir by V. Oppenheim (*Min. Agric. Dept. Nac. Prod. Min.*, Bol. No. 5, Rio de Janeiro, 1934). A summary of this paper is given in *Nature*, June 29, 1935, p. 1080.

Professor C. E. Tilley's "Report on Rocks from the South Orkney Islands" (*Discovery Reports*, X, 1935, 383–90) throws some light on the geological constitution of this remote and inaccessible Sub-Antarctic group. It now appears that there is an older metamorphic series, and a younger sedimentary series consisting of greywackes and shales followed by conglomerates. The greywackes are mainly derived from the denudation of sodic igneous rocks. This sedimentary series may be tentatively correlated with the *Pleurograptus* shales discovered by Pirie on the *Scotia* expedition.

In a paper on the "Structure of the Queen Maud Mountains, Antarctica," L. M. Gould (*Bull. Geol. Soc. Amer.*, **46**, 1935, 973–84) shows that this area consists of an extensive fault-block mountain range in which the horsts are bounded by nearly vertical fault-

planes. The mountains consist of a basement of Pre-Cambrian gneisses and schists surmounted by the thick horizontal Beacon Sandstone formation which is extensively injected by diabase sills.

PEDOLOGY. By Professor N. M. Comber, D.Sc., A.R.C.S., F.I.C., The University, Leeds.

Soil students have suffered a great loss by the death of Dr. C. F. Marbut, who for many years had been in charge of the soil survey of the United States. Dr. Marbut was one of the first to recognise the importance of the genetic study of soil as developed by the Russian School, and himself became a leader in its further development. Not only did he revolutionise the U.S. soil survey but his services to soil studies in other centres were by themselves a great achievement. Dr. Marbut died in China, where he had gone to conduct soil studies for the Chinese Government.

Alike for his notable work and for his personal charm of character he was held in high international esteem by soil students.

Soil Mineralogy.—It has been remarked in recent years in this section of "Recent Advances" that the mineralogical study of the clay fraction of soils has been a subject of outstanding interest and importance to soil students. A concise account of the present position is available to readers in an article by Marshall in Science Progress, No. 119, January 1936. It is sufficient here to say that it is now reasonably well established that colloidal clay consists largely of a limited number of secondary crystalline minerals, derived by weathering from the primary minerals.

Soil Classification.—Apart from the studies of colloidal clay it is difficult to point to any particular branch of soil study which has been the subject of general experimental research during the last year. At the International Congress of Soil Science at Oxford in July 1935 there were many valuable technical papers, but other papers and contributions to the discussions made it evident that there is in many parts of the world a recognition of the need to reconsider the basis of soil classification which is in fact the basis of Pedology. For a long time attempts to secure the recognition of soil science as a "subject" with a name of its own failed, and quite rightly so. There was no justification for such recognition so long as soil science consisted merely of chemical studies, physical studies, etc. As is now well known the outlook on this matter was profoundly altered by the recognition of a natural genetic classification of soils which came from the study of the soil profile, and the structure of its various horizons, considered mainly as a product of the climate.

The pursuit of soil investigations on so broad a basis as this has not unexpectedly led to some confusion. As typical of the difficulties that have arisen may be taken the definition of a "solonetz" which has been discussed by Shaw and Kelley (*Trans. Inter. Cong. Soil Sci.*, **1**, 330, 1935). In the original Russian scheme the chief characteristics of a solonetz were (i) the exchangeable ions were mainly sodium; (ii) there was an excess of sodium carbonate; (iii) because of the excess of sodium carbonate, humus tended to be carried down from the surface and, on drying out, to give a columnar structure to a "B" horizon in which it was deposited. A solenschak differed from a solenetz in containing only neutral sodium salts and in consequence a quite different structure. Now Shaw and Kelley point out that so eminent an authority as Glinka classified certain of their Californian soils as solenschak on the basis of examination of their structure while Gedroiz on the basis of chemical analysis labelled them Solonetz. They point out moreover that some soils with a well-developed solonetz-structure have very little replaceable sodium. This is typical of a general difficulty that is arising everywhere, namely that certain features, the coexistence of which have been regarded as essential and which are considered to be related as cause and effect, do not in fact always coexist.

Some interesting comments and suggestions which bear on these difficulties have been made by E. M. Crowther (*Trans. Inter. Cong. Soil Sci.*, **1**, 339, 1935), who points out in the first place that the names of the genetic groups of soils have been given a spurious academic status in so far as they are descriptive words in the Russian peasant language—meaning "much salt," "black soil," etc.—and were applied for the most part long ago. It may happen that a name refers to one particular characteristic and tends to put that characteristic out of perspective. Crowther emphasises the fact that in considering a genetic classification of soils it must be recognised that there have been influences at work in every period of the history of the soil and also that some horizons in a profile may be of different age from others. It is also pointed out that empirical systems of soil mapping in a restricted area may tend to exaggerate differences that have little genetic significance and to obscure those properties which are of genetic significance and by which alone the soils can be validly compared with those of other regions. Following these observations Crowther suggests that genetic interpretations might usefully be expressed on a logarithmic basis of time in a manner similar to that by which acidity is expressed on the pH scale. A pT scale is suggested,

using the negative logarithm to the base 10, of the time in years. In this country recent cultivations might occur up to pT = 1, draining systems, etc., up to 3, forest clearances between 3 and 4 and so forth. Whether such a scale will be a practical possibility some may doubt, but its contemplation in the present difficulties will have great value in helping to preserve perspective.

A number of papers have been put forward urging far more rigid and universally agreed definitions of soil types. On the other hand, it may be argued, as the foregoing considerations show, that an extensive arbitrary terminology may hinder rather than help the further study of the genetic processes.

Climate and Soil Conditions.—The relation of rainfall to certain soil conditions has been studied by Jenny and Leonard (*Soil Sci.*, **38**, 363, 1934) in the following way. Samples were collected along the 11° C. isotherm over a rainfall range of 14–38 inches. The layer of carbonate was found to fall fairly regularly by about 2·5 inches for every inch of rain. The pH figure fell from nearly 8 at 14 inches of rainfall to 5·2 in the region of highest rainfall, exchangeable hydrogen appeared at a rainfall of about 25 inches. Nitrogen increased continuously with increasing rainfall and the colloidal clay also increased by about 1 per cent. for each inch of rainfall. It must be carefully noted, however, that appreciable variations occurred in each of these conditions at any given rainfall.

An "inductive" study of soil type and climate has been carried out by E. M. Crowther (*loc. cit.*) with the object of testing the alleged equilibrium between well-recognised soil types in Russia and climate. The mean rainfalls of about 300 points read off at equal distances over 20° of latitude and 30° of longitude were plotted against the mean temperatures. The points were grouped according to 12 types of soil. If the soil and climate are in equilibrium there should be a characteristic temperature rainfall band for each type. It was found that over the podsol area the rainfall increased by 1·6 cm. for each degree Centigrade increase in temperature. Over the chernozen area there was an increase of 1·2 cm. for each degree Centigrade. The distribution of chestnut soil was found to depend mainly on rainfall since within narrow rainfall limits they covered a fairly wide temperature range. Other soils were irregularly distributed.

Soil Colloids and Soil Classification.—The study of soil colloids and the study of soil classification have each occupied much attention lately and it is interesting to see that the bearing of one upon the other is now being considered. Bradfield (*Trans.*

Inter. Cong. Soil Sci., **3**, 134, 1935) deals with this after outlining the recent work on clay. The correlation of soil characters with climate (*i.e.* temperature and rainfall) must always be crude in the sense that it admits of the recognition of broad zones of soils but is useless for detailed survey work in a restricted area. According to Bradfield we have now gone as far as we can in our use of climatic information and the soil morphologist will now need the services of the physical and colloid chemist in obtaining a more detailed morphological classification. In other words, climate is a highly complex combination of factors and the limits have probably been reached within which it may usefully be considered in a broad way as one factor.

The soil geneticist has been dealing with the broad view of the world soils as influenced by climate and geology. At the other extreme the physical chemist has been studying the colloidal products and is now working backwards, so to speak, to consider the processes that led to those products. In this way the laboratory studies of clay and humus are tending to put into the genetic scheme the details without which some of the present difficulties cannot be overcome.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

The geographical distribution of the flora of an area of about 580 sq. km. in north-west Sweden has been described in detail by Segerstad (*Arkiv f. Botanik*, **27**, 405, 1935). The account is accompanied by distribution maps of about 200 species. Such an intensive study of an area less than half the size of most English counties reveals striking restrictions. The flora includes continental, atlantic, northern and southern types several of which attain their geographical limits within the area studied.

Some interesting ecological features are presented by the study of A. McTaggart of sixteen plots of different artificial pasture mixtures in which one-third of each plot was pastured by sheep and the other two-thirds cut respectively at six- and eight-week intervals. In all types the ground covered by the species with shallow root systems fluctuated in a manner corresponding closely with the variations in rainfall. *Lolium perenne* and *Dactylis glomerata* showed a marked capacity to withstand competition. *Poterium sanguisorba* owing to its palatability soon diminished under grazing and, despite a comparatively deep root system which probably accounts for its marked drought resistance, showed marked variation in development with rainfall. *Medicago sativa*

was very drought resistant owing to the deep rooting and showed little variation of cover with rainfall. Both *Trifolium repens* and *T. subterraneum* were severely affected by drought, and pasturing and frequent mowing alike led to almost complete elimination (*Jour. Council. Sci. Indust. Res.*, **8**, 177–86, 1935).

In a paper by R. M. Barnette and H. Mowry in *Soil Science* (**41**, 71, 1936) the results of experiments with Azalea are described which indicate a *p*H range for healthy growth of from 5·0 to *p*H 7·0. In more alkaline soils growth is slow and the leaves chlorotic whereas in soils more acid than *p*H 5·0 the growth was also slow and the habit dwarfed.

An interesting Algal collection from Lake Macotama in the Santa Marta Mountains, Colombia, has been described by W. R. Taylor. The interest of the records is due to the great altitude of the lake, namely 14,400 feet above sea level. The species recorded comprise 31 Myxophyceæ, 2 Heterokontæ and about a hundred Chlorophyceæ. The Desmidaceæ is the preponderating group as regards the number of species, *viz.* 77. Two new species of *Zygogonium* are described, two of *Cosmarium*, one each of *Staurastrum* and *Euastrum*. Amongst the myxophyceæ *Clastidium setigerum* Kirchn was abundant as an epiphyte on *Tolypothrix* in one sample, and this appears to be the first recorded occurrence for the American continent (*Amer. Jour. Bot.*, **22**, 763–78, 1935).

G. W. Musgrove (*Soil Science*, **40**, 391, 1935) describes an instillation with undisturbed soil columns for measuring evaporation, run off and percolation. The results for four conditions of treatment in triplicate are furnished. The precipitation was just over 25 inches and of this about 14 inches or approximately 56 per cent. was lost by evaporation from the fallow surfaces with and without organic manure. The addition of the latter only reduced evaporation by about 2 per cent. The addition of organic manure was accompanied by an increased percolation both in cropped and uncropped soils. Water lost by run off amounted to about a quarter of the precipitation on fallow soil and about a fifth when under maize but from manured soil bearing maize the loss in this way was only about one-tenth. From unmanured soil bearing maize the percolation was about 3 per cent. as compared with about 16 per cent. from fallow soil.

The study of viability and germination of the seeds of prairie plants and of the early phases of their development, by A. K. Blake (*Ecological Monographs*, **5**, 405–60, 1935) represents the type of investigation that is greatly needed if Ecology is to emerge from the dominance of purely descriptive studies. The viability

was tested by means of successive plantings of seeds at intervals of one to two months. Of six grasses the maximum percentage germination of all sowings ranged from 8 per cent. (*Andropogon scoparius*) (*Sorghastrum nutans*) to 87 per cent. for *Elgmus conadensis* and 71 per cent. for *Sporobolus asper*. Although all six species yielded some germinations when seeds were sown in June there was a marked tendency towards maxima in spring and autumn, especially the former. Three herbs were studied of which *Œnothera biennis* yielded up to 55 per cent., *Kuhnia glutinosa* up to 57 per cent. and *Liatris punctata* up to 83 per cent. In each the maximum germinations occurred in the early part of the year. Considerable differences were observed in the germination of seeds harvested in successive years as for example those of *Kœleria cristata* which yielded 26 per cent. (1928), 49·5 per cent. (1929), 29 per cent. (1930) and 63 per cent. (1931). A slight benefit resulted from freezing the seeds which was more marked for the dicotyledonous plants than for the grasses ; 7 out of 14 species of grass gave higher percentage germinations after storage in moist soil and twenty of the twenty-six dicotyledonous herbs, but the germination of *Liatris punctata*, *L. scariosa* and *Hieracium longipilum* was diminished by stratification. One-third to one-half saturation was found most favourable for germination. Seedling mortality is high, mainly attributed to drought. Many are killed at a very early stage and of the survivors some 57 per cent. subsequently perish. Rapid growth is essential to survival but growth is usually slow owing to shade and competition.

The changes in osmotic pressure of the sap of various psammophytes and halophytes has been studied by C. Killin (*Ann. de Physiologie*, XI, 70–124, 1935). *Euphorbia peplis* was found to have an osmotic pressure in the aerial organs of about 11 atmospheres throughout the life history, although slightly lower during the wetter months. The osmotic pressure of the root, however, which, in the early part of the life cycle, is below that of the aerial organs, rose to about 20 atms. towards its termination. Various hemicryptophytic dune plants yielded values between 10 and 15 atms. during the wet season rising to from 15 to 30 atms. towards the end of the dry period. *Glaucium flavum* and *Daucus gummifer* showed pressures respectively of 10 to 20 atms. and 12 to 21 atms. according to the season. The annuals yielded from 5 to 10 atms. in the wet season to from 5 to 20 atms. in the dry. Diurnal fluctuations observed attained to just over 2 atms. in the roots in *Orlaga maritima* and nearly 7 atms. in *Asteriscus maritimus*. Seasonal fluctuations in the halophytic Plantagos ranged from *ca.* 7 to 20

atms. and in *Frankenia lævis* from *ca.* 18 to 83 atms. The author regards high osmotic pressures as having nothing to do with the drought resistance of species.

Bulletin No. 90 of the Council of Sci. and Indust. Res. is an important contribution to timber identification which furnishes descriptions of, and a key to, the chief Australian commercial timbers other than those of the genus Eucalyptus. The Bulletin which is by H. E. Dodswell and A. M. Eckersley is illustrated by 56 photomicrographic reproductions.

CYTOLOGY.—D. C. Cooper reports the mode of development of the embryo-sac in *Lilium Henryi* and eight other species of the genus as exhibiting the same abnormal mode of development as that recorded by Bambacione (1932) for *L. bulbiferum* and *L. candidum*. At the four-nucleate stage three of the nuclei pass to the chalazal end. The single micropylar nucleus divides to form two haploid nuclei, *viz.* the egg nucleus and a polar nucleus. The three chalazal nuclei fuse during a further division so that there are produced two triploid nuclei of which one provides the second polar nucleus. The second male gamete fuses with the two polar nuclei of which one is haploid and the other triploid yielding an endosperm which is pentaploid (*Bot. Gaz.*, **97**, 346, 1935).

From a study of polyploid individuals and polyploid chimaeras of *Biscutella lævigata* and *Iberis semperflorens*, I. Manton (*Proc. Roy. Soc.*, **118**, 522–47, 1935) finds that the number and position of the prochromosomes in meristematic cells corresponds with those of the chromosomes. In *Biscutella lævigata* material with 18 chromosomes ($2n$), 27, 36, 45 and 54 chromosomes was available. For all but the last two of these the number of prochromosomes in the resting nuclei showed an exact correspondence with that of the chromosomes in the dividing cells. In the pentaploid and hexaploid cells the agreement was very close considering the difficulties for interpretation. A study of *Allium ursinum* leads to the conclusion that the telophose enlargement of the chromosomes results in a doubling of their diameter and the " reticulate " resting nucleus is considered to be a relatively solid structure devoid of free nuclear sap until prophase.

Clarke and Anderson (*Amer. Jour. Bot.*, **22**, 711–16, 1935) describe chromosomal interchange without ring formation as occurring in *Zea mais*. In the same journal (pp. 741–61) Lewis deals with the form of the tracheids in the Pine which depart from the 14-hedral form owing to a curvature originating in the cambium as a consequence of which 18 or 22-hedral conditions result.

FLORAL MORPHOLOGY.—A general comparative account of the

morphology of the male and female infloresences in the Betulaceæ is furnished by E. C. Abbe, which includes a number of illustrations of the detailed structure and anatomy of the partial infloresences of a great variety of representatives. These show a much wider range of variation than is generally known to occur. From this comparative study the author concludes that the partial female infloresence was primarily three-flowered (though two-flowered in all genera but *Betula* by suppression of one flower) and that these bicarpellate flowers owe their diverse orientation to derivation from a tricarpellate condition with suppression of one carpel. The three-flowered male partial infloresence consists of flowers which were primarily trimerous in construction, a condition that occurs in *Carpinus japonicus* where each flower has six stamens, in *Ostrya virginiana*, and frequently in *Alnus rhombifolia* and *A. nepalensis*. The male flowers of *Betula* show a wide range of structure. Trimery occurs commonly in the sub-section Costatæ. In the sub-section Albæ dimery is the rule whereas in *Betula pumila* each of the three flowers of the partial infloresence consists of a single perianth member and a single stamen. In *B. alnoides v. pyrifolia* each flower has four perianth segments and two stamens but in *B. maximowieziana* the lateral flowers may be strictly tetramerous with four perianth segments and four stamens whereas the central flower is trimerous with three perianth segments and three stamens (*Bot. Gaz.*, **97**, 1–67, 1935).

PLANT PHYSIOLOGY. By Professor Walter Stiles, Sc.D., F.R.S., The University, Birmingham.

Water Relations of the Plant.—While it is generally assumed that water is absorbed by the roots of higher plants through the root hairs some writers have doubted whether this view exactly expresses the true state of affairs. The question has recently been reopened by K. Höhn ("Die Bedeutung der Wurzehaare für die Wasseraufnahme der Pflanzen," *Zeitschr. f. Bot.*, **27**, 529–64, 1934) and by H. Sierp and A. Brewig ("Quantitative Untersuchungen über die Wasserabsorptionszone der Wurzeln," *Jahrb. f. wiss. Bot.*, **82**, 99–122, 1935).

Höhn measured the rate of uptake of water by different lengths of root of *Vicia faba*, *Zea mais* and *Tradescantia fluminensis* with a very sensitive potometer by means of which an uptake of less than 0·1 cubic mm. of water could be measured. It was not found possible to exclude any part of the root from taking up water by the simple device of surrounding it with a medium impermeable to water. There appears to be always a capillary movement of

water between the root and the substance covering it. Höhn therefore examined the uptake of water by whole roots of different ages, where the different regions such as root-hair bearing zone, stretching zone and older zone constitute different proportions of the whole. The conditions of experiment were said to be such that the absorbing surface was the limiting factor so that the rate of absorption of water was not affected by the transpiration rate. From his experiments Höhn concluded that water uptake by the root-cap zone is comparatively small, that the stretching zone of the root, devoid of root hairs, is not the region of maximum water uptake, and that the rate of absorption by the older part of the root above the stretching zone is generally greater than that of the younger zone. Comparative experiments with roots, with and without root hairs, showed that the presence of root hairs does not affect the quantitative relationships of water absorption in the different regions of the root, and Höhn supposes that in water culture the root hairs have no significance for water uptake. He points out, however, that it does not follow that the root hairs are without significance for water uptake in dry soil or for the uptake of dissolved substances.

The experiments of Sierp and Brewig were carried out mainly with *Vicia faba*, but a few experiments were also made using *Ricinus communis* and *Zea mais*. They attempted to determine the relative rates of water uptake by different zones of the root by the use of a complex potometer consisting of a main tube with a number of side tubes and taps between the insertions of the various side tubes. The main tube contained the root under observation while each of the side tubes acted as a micropotometer. The rate of water absorption by each zone of the root was determined by the movement of the water in the various micropotometers. By experiments with this apparatus Sierp and Brewig concluded that, in unbranched roots of *Vicia faba* about 10 to 17 cm. long, the apical zone of about 5 mm. absorbed no water for transpiration, but on the contrary often gave off water, while the next zone to it, without root hairs, only absorbed an insignificant quantity of water. The zone of greatest water absorption when water uptake by the whole plant is low was found to be that between 15 and 80 mm. from the apex and corresponds with that of the living root hairs. With increasing transpiration the position of greatest absorption shifted towards the older part of the root and with a high transpiration rate more water was taken in by the older part of the root, that more than 10 cm. from the apex, than by the younger parts.

The relation between transpiration and the conditions of tension in the water-conducting tracts of the xylem have formed the subject of investigations by F. M. Haines. This worker some years ago ("A Method of Investigating and Evaluating Drought Resistivity and the Effect of Drought Conditions upon Water Economy," *Ann. Bot.*, **42**, 677–705, 1928) defined the term *pressure deficit* as the difference between the pressures in the tracts and in the atmosphere, and it is the relation between this pressure deficit and transpiration with which Haines deals in two recent papers. In the first of these ("Transpiration and Pressure Deficit—I. Apparatus and Preliminary Experiments," *Ann. Bot.*, **49**, 213–38, 1935) he points out that direct experiments on this question can be made in two ways, firstly, by freely exposing the transpiring surfaces of a cut leafy branch to the atmosphere and allowing the branch to take up water from a closed vessel in which the pressure is either naturally or artificially reduced, and secondly, by allowing the plant to take up water from an open vessel at atmospheric pressure but enclosing the transpiring parts of the plants in a vessel in which the pressure is increased above that of the atmosphere. By the first method the effect on transpiration of pressure deficits of less than one atmosphere only can be measured, but by the second method the effect of pressure deficits up to 30 atmospheres can be used with an appropriate choice of the plant species used. Such deficits actually occur in nature because of the existence of cohesion tensions in the liquid in the tracts; where these exist the actual pressure deficit is the atmospheric pressure + the cohesion tension.

Haines used both these methods as well as a variant of the first method in which the plant was allowed to take up water from a vessel which could be closed and opened at will, but in the paper cited above only results obtained with the second method are recorded. Branches of various plants, such as *Acer* and *Æsculus*, were used in a potometer, and the rate of absorption of water by the branch determined when the upper part of the latter was enclosed in a strong metal cylinder containing air at pressures up to 100 atmospheres. The cylinder possessed a glass window for illumination. It was found that the rate of water absorption always decreased with increase in pressure, and vice versa. If a pressure above an atmosphere is maintained the rate of water absorption is at first reduced below that observed at atmospheric pressure, the rate later rises and approaches in value the original rate at atmospheric pressure. If the pressure is reduced there is a temporary disproportionate increase in the rate of water absorption, but the latter soon lessens. It is thus understandable that the

actual rate of water absorption at any pressure at any time depends not only on the value of the pressure at that time, but on the immediate previous history of the branch as regards the pressure deficit. The observed effects are attributed partly to alterations in the volume of the cells of the leaf and partly to changes in transpiration rate.

A later paper (" Transpiration and Pressure Deficit—II," *Ann. Bot.*, **49**, 521–65, 1935) records the results of further experiments by the same worker. Thus it is shown that when the pressure deficit was increased by successive increments at 2-minute intervals from 0 to 60, 80 or 100 atmospheres or more, the rate of water absorption by branches of *Acer* decreased with each increment of pressure deficit, the final rate with the maximum pressure deficit being only a fraction (one-eighth to one-third) of the initial rate when the pressure deficit was zero.

The rate of absorption was also measured by observing the distance penetrated by eosin in one minute after the cut end of the branch had been dipped in a 1 per cent. solution of this dye. This method also indicated that with increasing pressure in the cylinder the rate of absorption falls with increase in pressure, while reduction in pressure brings about an enormous increase in the rate of water absorption. With pressures maintained above that of an atmosphere the rate of absorption, as observed with potometric measurements, increases towards the rate observable with atmospheric pressure. This is attributed to the compression of the leaf cells resulting from the increased pressure reaching its equilibrium position with the consequent cessation of the tendency towards a downward flow of water resulting from the compression of the cells. The final equilibrium rate of water absorption is thus to be regarded as a measure of transpiration rate under the particular pressure employed. When the transpiration rates found in this way are plotted against the pressure deficits, it is found that while increase of pressure deficit reduces transpiration rate, the reduction is relatively most marked at lower deficits.

Another recent paper by Haines deals with the presence of air in the water conducting tracts of woody plants (" Observations on the Occurrence of Air in Conducting Tracts," *Ann. Bot.*, **49**, 367–79, 1935), a matter on which different views have been expressed. He showed that in a number of such plants, after careful removal of the bark, tracheæ containing air can be seen as lighter-coloured streaks. The bark must be removed so as to expose the surface of the wood uninjured. It appears that air is sometimes present and sometimes absent from the tracheæ, the presence or absence

depending largely on external conditions. Thus in early spring no air was detected, but a few weeks later air was always found to be present in the hotter parts of the day during a spell of fine weather, but to be absent during wet weather.

The effect of wind on transpiration rate has been examined by Maria Wrenger (" Über den Einfluss des Windes auf die Transpiration des Pflanzen," *Zeitschr. f. Bot.*, **29**, 257–320, 1935), whole plants of 38 different species of various morphological types being used for her experiments. In experiments of short duration, lasting only one hour, a wind of approximately 1·85 metres per second always brought about a considerable increase in transpiration rate, although the rate of transpiration during the first 10 to 15 minutes was always higher than during subsequent periods. The observed increase in transpiration rate varied from 12 to 150 per cent. of the rate in still air, but depended on the species, the effect of wind in increasing the rate being generally greater in mesophytes than in xerophytes. This effect of wind was greater in plants with open stomata than in those with the stomata almost or quite shut. In plants where transpiration is high in still air it was observed that although a wind increases initially the transpiration rate, the subsequent rate in the wind falls off below that obtaining in still air, and even when transferred to still air the transpiration rate remains depressed for a time.

With *Helianthus annuus*, *Solanum melongera* and *Tropæolum majus* it was shown that the rate of transpiration varies with the wind velocity in such a way that for each species there is an optimal wind velocity for which the rate of transpiration over a short period is a maximum.

Even with exposure to a wind for periods of from a day to a fortnight a definite increase in transpiration rate as a result of exposure to the wind was observed, the magnitude of the increase depending both on the species of plant and on the other external conditions.

ZOOLOGY.

By Emeritus Professor W. Garstang, M.A., D.Sc.; E. B. Ford, M.A., B.Sc., and J. A. Moy-Thomas, B.A., The University, Oxford.

Louis Gallien (*Trav. Stat. Zool. Wimereux*, XII, 1, 1935, 1–181) has redescribed the life-history of the Trematode *Polystomum integerrimum*, and defined with much more precision the conditions which govern the production of the neotenic form discovered by Zeller in 1872. In the frog's bladder the parasite becomes ripe at the same time as its host (in France round about March 5–15),

and the highly organised, ciliated ("Gyrodactyloid"), larvæ readily find tadpoles. When they alight upon tadpoles more than 13 days old, they enter the spiracular cavity, remain there as in a mere waiting-room—without feeding or growing—until the tadpole's metamorphosis, and then pass by the gut into the newly formed bladder of the young frog, where they begin to feed and grow slowly (2 mm. in 2 years) into normal *Polystomum*, reproducing for the first time when 3½ years old at a size of 8–10 mm. If, on the other hand, they secure a young tadpole less than 8 days old, they begin at once to feed on its internal gills, sucking blood plasma through the unperforated skin. Here they grow rapidly into the neotenic form. The gut remains a simple broad sac, and the prospective large subterminal hooks of the normal form are absorbed after first undergoing a phase of enlargement. The parasites become sexually mature at a size of 1½–2 mm. but without copulatory organs or uterus, and lay self-fertilised eggs in 3–5 weeks. These produce normal Gyrodactyloid larvæ, which settle upon old tadpoles (young ones being no longer available), and grow up into the normal bladder-form. The neotenic parent, on metamorphosis of the tadpole, does not seek the bladder, but drops off—often through the hole made by the front leg—and dies. Finally tadpoles of 8–13 days provide a threshold which leads to a variable medley of results.

Gallien's account makes clear that there is no genetic difference between the two forms, and that the chief factor in determining the production of pædomorphs is the early diet of tadpole blood, in which the tadpole's hormones may play a special part. Similarly he is inclined to attribute the co-adaptation of the sexual cycles in the normal form and its host to a common influence of the frog's hormones, transmitted to *Polystomum* with its food. We have summarised only the normal course of these remarkable life-cycles, so reminiscent of the Axolotl, but—*à moins de croire à un finalisme puéril*—the misfits, irregularities, and blind alleys recorded by the author are no less interesting than the marvellous synchronisms of adaptation to which he draws attention.

More use should be made of this worm in our teaching courses. In France some 25–30 per cent. of the frogs (*temporaria*, not *esculenta*) seem to be infected, and, with a supply of eggs from an infected frog (N.B. at least 3 years old), it is easy to raise either form at will by introducing the right tadpole into a vessel with the larvæ. Young larvæ ($<$ 20 hours) go straight for the tadpoles. After 24 hours of free life they become torpid, lose their cilia, and die.

Another interesting metamorphosis, recently revealed, concerns the peculiar stalk-eyed deep-sea fish called *Stylophthalmus* by Brauer, with a soft, flexible, transparent body no stouter than a darning needle, minute pectoral fins but no pelvics, and a pair of small eyeballs at the end of a long slender stalks. Dr. W. Beebe (*New York Zoologica*, XVI, 4, 1934) has traced all the stages between larvæ of this type from Bermuda and the rapacious normal-eyed *Idiacanthus fasciola* of the same region—the "Tiger-fish" of his more popular articles. Moreover, he shows this species to exhibit a marked sexual dimorphism, the males remaining small and larvoid, though normal-eyed.

A. G. Lowndes (*Proc. Zool. Soc.*, **1**, 1935, 35–48, 5 pl.) has detected for the first time and filmed active rotatory movements of the sperms of freshwater Ostracoda, which are correlated with the spiral bands described by previous observers. These movements only take place in the spermatheca of the female. Both sperms and spermathecal ducts are of extraordinary relative length, the former being 10 times the length of the adult body. Previous orientations of the sperm must now be reversed, since it is the slender part which is anterior in movement (as well as being tactile, apparently, in function), while the stouter end—still very slender—corresponds to the flagellum. A nucleus could not be identified. Lowndes believes that in most fresh-water Ostracoda the sperms are now useless, sexual reproduction having been replaced by parthenogenesis.

In Professor A. C. Hardy and E. R. Gunther's "Plankton of the South Georgia Whaling Grounds, 1926–7" ("*Discovery*" *Reports*, XI, 1935, 1–456) will be found not merely a substantial contribution to the *Discovery's* primary task in the Antarctic, but an intensive study of pelagic ecology raising the widest issues. The quantitative distribution, horizontal and vertical, of the various elements of the phyto- and zoo-plankton is fully set out in tables, charts, and sectional diagrams (the latter perhaps too big for the best synoptic effect), and is considered both in relation to the physical environment and to that of the Whales which feed upon it. Both the great rorquals—the Blue and Fin Whales—had been shown by Mackintosh and Wheeler to feed almost exclusively on *Euphausia superba*, which is also the favourite food of Crab-eating Seals, Emperor and Gentoo Penguins, Nototheniid fish, and other Vertebrates. Hardy and Gunther now show that this dominant Crustacean, apart from certain minor features, is at one with the zooplankton generally in its relations to the common diatomaceous food-supply. The distributional facts reveal the same

inverse correlation between phyto- and zoo-plankton in the Antarctic as in our home waters. Where diatoms were dense, zoo-plankton and Whales (which followed the Euphausians) were scarce, the main concentrations being in regions of moderate phytoplankton. Much of this report is devoted to a sustained attempt by Professor Hardy to get to the bottom of this intriguing paradox. In a region of unlimited phosphates, does phytoplankton become dense from a local shortage of grazing animals, or does it by multiplication create conditions repulsive to them ? In brief, do pelagic animals control the diatoms, or diatoms the animals ? A clear-cut answer is not provided, but Professor Hardy leaves no doubt as to his belief in "the hypothesis of animal exclusion," and discusses a host of variable phenomena concerning the vertical migrations of animals which may be connected with it. Briefly stated, his idea is that while pelagic animals, with few exceptions, must visit the upper zone for food, the extent of their sojourn there may be adjusted to the density of the phytoplankton by changes in the range and time of their vertical migrations—the denser the diatoms, the shorter their visits and the deeper their subsequent descent.

Whether the external products of intense photosynthesis (increased alkalinity, etc.) are repulsive or not to most animals clearly calls for settlement by experimental methods, and a preliminary experiment from Professor Hardy's laboratory may be cited as a type. Pairs of long tubes were set up, and their lower halves blackened. One of each pair contained sea-water alone (A), the other a culture of Nitzschia, of variable strength (B). Equal numbers of Mysids were put into the tubes of each pair, and the numbers counted in the upper halves of the tubes from time to time. The numbers observed in the A tubes almost invariably exceeded those in the B tubes, the average of half a dozen counts for three separate pairs of tubes being (A : B) as follows, 13 : 4, 25 : 16, 36 : 28. These results establish a difference of behaviour, but need additional information to explain it. Were the Mysids in the B tubes feeding below, like *Antarctomysis maxima* of the report ? If so the results merely illustrate the activity of hunger in A, and the relative quiescence of satiety in B. However, with the relations of such experiments to the great Whale and Herring fisheries explained to boys beforehand, there would seem here to be an attractive field for ecological experiments in many a school laboratory, with specific Cladocera and Copepoda as subjects (*cf.* Clarke, G. L., *Jour. Exp. Biol.*, VII, 2 ; IX, 2).

Dr. Hans Grüneberg (*Proc. Roy. Soc.*, B., **118**, 1935, 321), working in Professor D. M. S. Watson's laboratory, has described

and illustrated the remarkable effects of a new gene ("grey-lethal," *gl*) in the common house-mouse. It arose in a laboratory stock of "extreme dilutes." "Extreme dilution" is an allelomorph of albinism and dilutes the coat to a dirty white or very faint grey. The mutant was a pure grey, owing to complete absence of yellow, in this respect approaching Chinchilla, but darker. The new factor is a simple recessive (genetics described), but it causes disturbances in an unusually wide variety of organs, reducing growth, affecting the calcification of bones and teeth, and thereby inducing secondary changes in the length of the snout (which is very short and blunt), reduction of masticatory muscles, and in the form of various associated bones of skull and jaws. Altogether some 150 affected individuals were raised, and all showed the same succession of symptoms. All died in their third week a few days after weaning, since the abnormalities of their bones and teeth prevented adult modes of feeding. Readers of Darwin will recall that the association of defective teeth with hairlessness in dogs was one of the great naturalist's favourite examples of "correlated variation." Association of the same feature with a change of coat colour towards albinism is sufficiently near to encourage the hope that the mystery of both may soon be cleared up. Dr. Grüneberg promises further studies of these grey mice, especially of their endocrine glands.

PALÆONTOLOGY.—White (*Phil. Trans. Roy. Soc.*, B., **225**) has given a masterly account of the Ostracoderm *Pteraspis*, and discussed the general question of the affinities of the Agnathous Vertebrates. Perhaps the most interesting part of the account of the anatomy is the description of the body and particularly the caudal fin; the latter has never been described before, and the former only from isolated scales or small fragments.

The tail is hypocercal or reversed heterocercal, the lower lobe being the longer and produced posteriorly into a narrow process. The body is covered with scales which do not overlap and there are no paired or median fins. The dorsal and ventral surfaces of the body each bear a row of ridge scales.

Dealing with the affinities of the Pteraspids and other Ostracoderms, White finds no evidence to support the view of Stensiö that the Pteraspids are closely related to the Myxinoids, whilst the Cephalaspids and Anaspida are more closely related to the Petromyzontids; this theory required an independent origin for the two groups of modern Cyclostomes. White holds the view that the "*Heterostraci*, *Anaspida*, *Osteostraci* and *Cyclostomata* are related to one another inasmuch as they are classes of the group *Agnatha*,

but none ancestral to one another, the classes branching off the main stem at successive periods in the order named."

Brough (*Mem. and Proc. Manchester Lit. and Phil. Soc.*, **79**, 1935) has described a new Hybodont shark, *Lissodus*, from the Karoo and discussed the affinities of the Hybodonts in general. He believes that they form a distinct group and that *Cestracion* should be excluded from it. Brough holds that *Ctenacanthus costellatus* from the Lower Carboniferous is a true Hybodont and should not be classed with the Cladoselachii. This latter view is perhaps too sweeping, as further evidence has recently shown that the Ctenacanth sharks form an interesting connecting link between the Hybodonts and the Cladoselachii.

Gross (*Palæontographica*, **83**, 1935) has reviewed the microscopic structure of the scales of nearly all the groups of Agnatha and true fishes. In general his work supports and amplifies the classic researches of Goodrich and amongst other information two facts of particular interest have emerged: the scales of the Cœlacanths are found to be modified Crossopterygian or Cosmoid scales, and in the hind part of the scales of *Lepidotus minor* true cosmine appears to be present, whilst anteriorly they are Lepidosteoid. Therefore these scales represent an intermediate condition between the true "Palæoniscoid" and "Lepidosteoid" types of scales.

Genetics.—The results obtained by R. A. Fisher (*Phil. Trans. Roy. Soc. Lond.*, B., **225**, 195–226), in his study of Dominance in Poultry, are of exceptional importance. In formulating his well-known theory of dominance-modification, Fisher had observed that while the characters of domesticated races are generally recessive to those of wild individuals, a number of them are recorded as dominant in poultry. This he interpreted as due to human selection which, in the early stages of domestication, could only preserve and accentuate such peculiarities as were detectable in the heterozygote. For, at this stage, the flocks would be liable to constant crossing with wild birds.

If the general theory, of which this is a special application, were sound, it should be possible to recover the original effect of the mutant genes by placing them in the wild-type gene-complex. This has now been carried out by back-crossing domestic birds, showing different dominant characters, to Indian jungle fowl for five generations. Excluding the gene to be tested and those with which it is closely linked, only one gene in thirty-two is then of domestic origin. As explained in this paper, the results are now available for three of the genes chosen: crest, polydactyly, and barring. They confirm the expectation that the dominance of the

characters which they control is lost when they are introduced singly into a wild strain. Such dominance is therefore not a property of the gene itself, as it would be were it due to mutation or the selection of a different member of the same multiple allelomorph series, but it must result from the interaction of the gene with the genetic environment in which it is placed.

A number of other facts of importance have emerged during the course of this work. Among them may be specially mentioned the discovery that cerebral hernia and crest are due to the same gene. In the wild stock crest is present in the heterozygote, but more pronounced in the homozygote, while the disadvantageous herniated condition occurs in the homozygote only. Thus the former character may be described as a semi-dominant effect, the latter as a recessive effect, of the same gene.

U. Philip (*J. Gen.*, **31**, 1935, 341–52) has demonstrated crossing-over between the X- and Y-chromosomes of *Drosophila melanogaster*. It will be recalled that the gene *bb* (bobbed) is the only one known which can occupy a locus in either the X- or the Y-chromosomes. Philip has found that bobbed and its normal allelomorph exchange places between X and Y with a frequency of about 1 in 3000 experiments. This occurs both by double and inverted crossing-over. He points out that ordinary single crossing-over will not maintain the original distribution of genic material in an organism possessed of two differential segments in the Y-chromosome, as is *D. melanogaster*.

T. S. Painter (*Genetics*, **20**, 1935, 301–26) is making important contributions to our knowledge of the morphology of the chromosomes of *Drosophila melanogaster*. By using the remarkable material to be obtained from the salivary glands of the larvæ, he has already constructed a genetic map of the X-chromosome upon the new principles of studying translocations in this material. He has now extended this work to the third chromosome, of which he has prepared a similar map, figured in this article.

PHYSICAL ANTHROPOLOGY. By L. H. Dudley Buxton, M.A., D.Sc., Exeter College, Oxford.

The question of "racial purity" and hybrid races is one which at present is much exercising the minds both of the layman and the professional anthropologist. In the past it has not been seriously investigated. Anthropologists have been content to presume certain abstract races and to fit in modern types as best they could. Recently, however, both Dr. Morant, of the Biometric School, and Professor Raymond Pearl, in collaboration with John R.

Miner, have written important papers on the subject, following on the general method of approach suggested nearly forty years ago by Pearson in his classic memoirs on human evolution. Dr. Morant in the *Zeitschrift für Rassenkunde* (Band II, Heft 3, 1935) makes "an attempt to estimate the relative variabilities of various populations." He uses methods which he himself admits to be crude. He has collected statistics to show the variations of various measured characters in the populations from different parts of the same countries. He then takes the average squared standard deviation of each character which is found from the weighted squared standard of each province (as he calls the different parts of the same country). Then he takes the ratio of the result found for each character and calculates a mean for all the characters possible. We shall then have between any pairs two figures, one of which will in all probability be above unity and the other below unity, and will show the relative variability between the inhabitants of two countries; say South America might be 1·5 times more variable than Mexico, and then approximately Mexico would be 0·75 as variable as South America, though of course it does not work out quite as exactly as that, in fact in one of Morant's cases both the ratios are slightly above unity, suggesting, as the author says, that probably their variability is about the same. He has worked out his calculations for Norway, Sweden, Ireland, Macedonia, Egyptian Criminals, East Africa, Northern India, Central Asia, Korea and Japan, and so has covered much of the world. He concludes that the total populations of countries are more variable than their component provinces. This perhaps is not in itself a very important result but what follows is of greater theoretical importance. He finds that the variations of different peoples in different parts of the world are remarkably similar to one another and, using the method of computation he has adopted, none of the populations are 1·5 times as variable as any of the others. He then turns to ancient crania representing the earliest populations for which statistical data is available. He found that the Predynastic Egyptians and some Neolithic populations of Europe appeared to be little less variable than modern peoples. He issues a warning against artificially subdividing samples and so getting apparently old homogeneous stocks. "The whole conception of a number of ancestral types existing in the same modern population appears to be based on the assumption that there were homogeneous ancestral populations which exhibited far less variability than their modern descendents and if the ancestral population of that type cannot be found then the conception must be considered invalid." It is

obviously of great interest to know whether some of the stocks which have been considered hybrids are really hybrids or not. The real difficulty would seem to be whether or not variability in the measurements anthropologists normally take is really a good test of racial purity. It may be noticed in passing that the present writer has attempted on as yet unpublished material to estimate variability among the Primates. In a series of baboon skulls from the same herd he found on similar characters a similar variability to man. Raymond Pearl and John R. Miner (*Annals of Biology*, VIII, 1935) attack the same problem, using the method of the coefficient of racial likeness. This coefficient briefly takes the weighted difference between two groups in terms of the standard deviations of each character. Pearson, who invented the coefficient, applied it only to means, but Pearl and Miner extend it to measures of dispersion as well; the actual method is somewhat technical and need not be discussed here. In the only example they give (white cardiacs and white non-cardiacs) they find that the differentiation between the two groups is of the same order of magnitude as between such races as Anglo-Saxons and Prehistoric Swedes, not only in the means, where the greatest differences occur, but also in the variance, except in the case of indices. They end their paper with a discussion of the coefficient of racial likeness from the technical point of view. They promise to continue the subject at greater length and their conclusions will be awaited with interest; clearly if pathological conditions can introduce differences of the same magnitude as racial differences our ideas of racial differences will have to be very carefully reconsidered.

Human evolution in its broader sense is considered by Professor W. K. Gregory in a paper in the *American Naturalist*, LXIX, 1935. He discusses the rôles of undeviating evolution in the origin of man. He defines these terms as follows. By undeviating evolution he means cases in which over a fairly long period of geologic time certain characteristics that are already observable in a moderate degree in remote ancestors become more and more accentuated in their descendants, citing as an example the feet of the horse. By evolutionary transformation he means a case in which there has been a profound change both in the life medium and in the "anatomical habitus," so that wide differences may occur between members of either the same order or family. Under such conditions the descendant bears little resemblance to his remote ancestors, and he cites methods of locomotion as an example. Turning to man and his immediate relations and comparing the skulls of man and the chimpanzee, Professor Gregory considers that their differ-

ences are due both to undeviating evolution, for example the extension of the braincase in man, and also to transformation. This transformation is exemplified by the sharp abbreviation of the upper jaw in man, accompanied by a forward expansion of the upper part of the nasal chamber and the consequent lifting of the nasal bridge. In other words the braincase of man differs from the braincase of the chimpanzee in degree, not in kind, but whereas the chimpanzee has, in non-technical words, a snout, man has a mouth and a nose. Looking at the problem from the point of view of the palæontologist, Dr. Gregory declares that " if we did not have the chimpanzee we should have to invent him." On the other hand the same author concludes elsewhere (*Bull. School of Medicine Univ. of Maryland*, **20**, 1935) that many skeletal differences between man and the chimpanzee which are ordinarily attributed to divergence may rather be the measure of profound transformation from ape to man. These studies are based for the most part on the skeleton, but work along similar lines is being done by members of the Anthropological Society of Paris. In their last volume (*Mem. Soc. d'Anthrop. de Paris*, Tome 5, Serie VIII, 1935), P. Ardouin discusses the anatomy of the ear among certain apes, the Orang and Chimpanzee. Like Professor Gregory he finds profound differences between the Chimpanzee and Man, but suggests that the study of these minute anatomical points is worth developing. A similar study by Clavelin and Coulouma, in the same number, discusses the termination of the pneumogastric nerves in man and the anthropoids. In fact, we shall soon have as much data about the detailed anatomy of the apes as of man, material which cannot but be of great value in estimating the trend of human evolution.

No sensational discoveries of early man seem to have been reported in the last few months. Parts of a Mousterian skeleton were found by Professor Kadic in a cave (now called Grotte Mussolini and it would seem formerly Subalyck) near Eger in Hungary. We shall have to wait for a complete publication, but the journal cited above (*Mem. Soc. d'Anthrop. de Paris*, Tome 5, Série VIII) states that a portion of a mandible, sacrum, sternum, knee cap, and vertebræ were also found, together with the maxilla of an infant. The latter is declared to be recent, but the mandible is said to be intermediate in form between the jaws from Heidelberg and Krapina.

In the matter of racial differentiation Dr. Paul Stevenson, in the *Peking Natural History Bulletin*, **8**, 273 ff., discusses body-size and form and the endocrine factors involved. He concludes that

recent work, while possibly somewhat altering Keith's original allocation of endocrine responsibility for certain physical features in man's make up, has not offered any serious challenge to the original assumption put forward at a meeting of the British Association fifteen years ago. This assumption, it will be remembered, was that racial differentiation in regard to physical character, such as body-size and proportions, head-shape, and facial features, is merely the outward expression of differential fluctuations of the endocrine balance, while the direction and degree of such fluctuations is subject to strict genetic control. Dr. Stevenson suggests a good deal of evidence is being collected which is responsible for the growth of what he calls most aptly "the metabolic viewpoint in anthropology."

The somewhat dull matter of standardisation of anthropological measurements continues to occupy a good deal of attention. There appears to be a constant trickle of papers, most of them of a highly technical nature, directed partly towards the exact definition of particular points used in measurement and partly towards standardisation of technique. It is becoming more and more clear that anthropologists all over the world are trying to find a more common ground than they have found hitherto and to realise that so much of their laborious work is apt to be wasted when their results cannot be entirely correlated with those of other workers because different techniques are used. A typical example of this careful standardisation work is a paper by M. F. Ashley-Montagu (*American Journ. of Physical Anthropology*, **20**, No. 1 and Suppl.) on the method of determining the location of the nasion in the living.

NOTES

Crude Fibre (A. G. N.)

Crude fibre determinations are much beloved by agricultural chemists, though perhaps not by the routine analyst who actually has to carry out the determinations under the conditions and in the time allowed by " official " methods. The procedure is a very old-established one, being already known and used in the fifties of the last century under the name of the Weende method. Since that time its shortcomings have been pointed out repeatedly; it was for a time discredited, and then crept back into general use and acceptance. By incorporation in the Fertiliser and Feeding Stuffs Acts, the method has been so dignified as to acquire a significance out of all proportion to its real value. If the limitations be fully realised, there is no doubt that the figures obtained are very useful in checking the composition of commercial feeds. But since the residue obtained bears no regular or definite relationship to the structural or fibre constituents of the material analysed, its use for research purposes in digestibility studies may produce misleading results. The crude fibre residue ordinarily represents 60–80 per cent. of the cellulose of the material, and an extremely variable proportion of the lignin, usually not over 40 per cent. As an indication of " roughage " the crude fibre figure is definitely low, and its use in the evaluation of the less digestible parts of feeding stuffs is quite misleading, since the lignin which is presumed largely to influence the availability of the cellulose is mostly removed by the alkaline treatment. Agriculturists and experts in animal nutrition are in the habit of thinking in terms of the present crude fibre figure, because of the multitude of digestibility experiments and systems of calculation of food value and " starch equivalents " which depend thereupon. However, it is a matter of opinion whether they are thinking correctly. Methods of analysis of plant materials have advanced sufficiently to make practicable and worth while digestibility studies carried out with a full knowledge of the actual composition of the material, rather than arbitrary and empirical fractions such as " crude fibre." The gain in precision would more than justify the extra analyses involved.

The Proceedings of the Prehistoric Society (R. E. M. W.).

The new Prehistoric Society, which has just issued the first volume of its Proceedings, is, in origin, but the old Prehistoric Society of East Anglia writ large. The time had come for the change, for the last decade has seen a development and broadening of British prehistoric studies without parallel in the history of the science. The East Anglian Society, under the direction of Mr. Reid Moir and his colleagues, had already achieved a position of distinction which left no doubt as to the medium through which this development should properly find expression; and, in promoting their Society to national status, these pioneers have rendered timely and, in a sense, self-sacrificing assistance to a study which must in any case continue to owe much to their local labours. In its new form, the Society issues its Proceedings from the University Museum of Archæology and Ethnology at Cambridge—an address which at the same time ensures a continuance of East Anglian connections and guarantees a wide national and international outlook. The new editor is Dr. Grahame Clark, whose central interest in the mesolithic and neolithic periods implies a catholic understanding alike of the earlier and of the later phases of prehistoric archæology, and is itself a safeguard against any tendency towards scientific sectarianism. On all grounds, the new Society and its publication enter upon their considerable heritage under the happiest auspices.

This is not the context in which to comment in detail upon the contents of the volume, but it may be useful here to indicate their range. They begin with the presidential address of the first president of the remodelled Society, Professor Gordon Childe, who reviews current tendencies and difficulties in the classification of archæological knowledge, and deals appropriately therefore with a fundamental problem that is liable to embarrass the beginner and sometimes to confuse the expert. Papers by Mr. Reid Moir, Mr. Henry Bury and Mr. J. E. Sainty deal with aspects or illustrations of the palæolithic, whilst papers by Mr. W. J. Hemp, Mr. C. W. Phillips and Mr. Stuart Piggott are concerned with the neolithic long barrows that are at last beginning to receive adequate scientific attention. Other papers by Mr. and Mrs. C. F. C. Hawkes, Mr. G. A. Holleyman and Dr. Cecil Curwen deal with phases of neolithic and Bronze Age culture; but perhaps the most important single contribution is that by the Editor on the prehistory of the Isle of Man. This subject is here dealt with as a whole for the first time, and, both as a collection of unpublished material and as a study in cultural insularity, Dr. Clark's paper is one of outstanding value. Situated

at just too great a distance from the mainlands of Britain and Ireland to come within the normal range of primitive seamanship, the island presents a sustained Bronze Age culture which even Scotland cannot rival. Indeed it would seem that the "ultimate Bronze Age" which, in southern Britain, came to an end about the 6th century B.C., lasted in the Isle of Man until the 5th or 6th century A.D.—a remarkable possibility which admittedly requires further proof but, in the consistent absence of Early Iron Age and Roman material, is difficult to gainsay.

Finally, not the least useful feature of the volume is an illustrated section summarising the results of the year's fieldwork on prehistoric sites in Great Britain and Ireland. As the complement of the annual summaries of Romano-British fieldwork published in the *Journal of Roman Studies* and of publications relating to prehistoric Britain in the *Archæological Journal*, this section is of outstanding value, and is, incidentally, an eloquent testimony to the quantity and the quality of the fieldwork in question.

Mapping in the U.S.A. (C. B. F.)

The Science Advisory Board of the U.S.A. has a status and function comparable to that of the three bodies which do similar work in this country, *viz.* the Advisory Council to the Department of Scientific and Industrial Research, the Medical Research Council, and the Agricultural Research Council. Its second annual report,[1] now before us, records as the chief achievement of its first year a plan for establishing a scientific advisory service to the government on a permanent basis; and the first steps towards realising that plan have been taken. This link between scientific workers and the Government has already proved its value; and it is likely to become of increasing importance.

The General Report gives a statement on the relations of Science to Government, and the principles on which the Board has acted, together with a summary of the results of the recommendations made in the first report. Two of these which have not yet been carried out, though approved in principle, recommended an acceleration in the work of mapping the country and a consolidation of certain mapping Services.

Perhaps most people assume that the surface of the earth, at least in all civilised countries, is now adequately surveyed and mapped. It is true that most of the coasts of the world, outside the Polar Regions, are charted—largely by the work of the British

[1] Second Report of the Science Advisory Board, Washington, D.C., Sept. 1935.

Hydrographic Service in the eighteenth and nineteenth centuries—and that the positions of most important physical features and permanent settlements are known. But it is also true that reliably accurate surveys on fairly large scales (of one inch to one mile or more) extend over less than a quarter of the land surface; and that the area of precise levelling, which enables us to know altitudes and slopes, is even less extensive. In Britain the production of topographic maps has been the work of one technical service, the Ordnance Survey, for nearly a hundred and fifty years; and we have perhaps the best national maps in existence, though the recent internal movements of population and accompanying changes have produced an urgent need for revision of all our large-scale maps. In the U.S.A., owing largely to historical accidents, there is not yet any one Service responsible for mapping the country. The chief Mapping Services are the Coast and Geodetic Survey, which is responsible for geodetic and magnetic survey over the whole country and for hydrographic survey on the sea (but not the Lake) coasts, and the Topographic Branch of the Geological Survey, which came into existence because no one can map geology until he has a topographic base map. The Report notes that twenty-six other agencies of the Federal Government carry out some mapping; but only the two just mentioned, for the whole country, and two local services, the Lake Survey and the Mississippi River Commission, are of real importance in this connection. Amongst them these mapping Services have by now produced topographic maps, mostly on scales of 1 : 62,500 or 1 : 125,000, for half the country; but of these those which are recognised as up to modern standards of accuracy cover only 26 per cent. of the area of the continental United States; so that barely a quarter of the country is adequately mapped. Europe is much better off, since nearly the whole of its inhabited lands are well surveyed, an advantage largely due to the military needs of its governments in the nineteenth century. During the present century the Survey of India has completed the mapping of two-thirds of that subcontinent on the scale of 1 : 63,360; and comparable progress has been made by the U.S.S.R. So the U.S.A., hampered by accidental divisions among its Federal Services and free from pressure of military needs, is relatively backward in its lack of exact knowledge of its own territory. The Science Advisory Board, as many sectional reports show, is fully justified in urging that the completion of the topographic map is one of the most urgent needs; for an accurate map is an essential tool for the scientific study and utilisation of the Natural Resources of any country.

The chief series of topographic maps of the U.S.A. are drawn on scales of 1 : 62,500 and 1 : 125,000, which are the decimal scales nearest to our inch-to-the-mile (1 : 63,360) and half-inch-to-the-mile (1 : 126,720). There are also series on scales of the inch-to the-mile group, and a number of maps on scales derived from the inch-to-the-foot relationship, *e.g.* 1 : 12,000 and 1 : 24,000. This mixture of scales is a little worse than that of the British Ordnance Survey, which begins and ends its series with decimal scales, the 1 : 2,500, which is commonly called the 25-inch, and the 1 : 1,000,000; but uses the inch-to-mile group of scales in between, as in the 6-inch (1 : 10,560), the 1-inch, the ½-inch, and the ¼-inch (1 : 253,440). Outside the British Empire and the U.S.A. practically all important large-scale maps are on decimal scales, usually simple multiples of the 1 : 1,000,000 scale of the International Map. And the Report before us recommends decimal scales for the mapping of the U.S.A.

Although questions of mapping occupy more than half of the space given to scientific reports in this volume, some other less urgent matters referred to are of no less importance. Most of the other reports are brief preliminary statements which we may expect to see followed up by further investigations. Some of them are statements of problems and suggestions for investigation rather than suggested solutions. They relate to the work of the Weather Bureau, the Public Health Services, the Patent System in its relations to industry, the Railways, and the Mineral Wealth of the country, and indicate several lines along which organised scientific investigation can be of direct service to the community.

Revision of British Ordnance Survey Maps (C. B. F.)

The vital importance of accurate and up-to-date maps for a planned economy, and the fact that our O.S. maps have been allowed to fall badly out-of-date, owing to the war and the post-war "economies," have now produced a widespread demand for revised maps. Experiment in mapping has been extremely active during recent years, aided by the development of the International Map and stimulated by the work in many new states and colonial territories. And the beginning of a long overdue revision of our maps gives an opportunity for the introduction of many improvements, which should not be missed. It seems to be generally agreed that the revised 1 : 2,500 base map should be redrawn on one meridian for the whole of Great Britain, so that any adjoining sheets shall fit each other exactly; in place of the existing series of county-groups, each of which is on its own meridian and independent of all the rest so that maps of different groups do not fit each other.

There is also discussion about the symbols and scales to be used. Here all scientific workers will support the proposal that the various scales should be simple and exact multiples or sub-multiples of each other; and, further, that they should be in the decimal series used in nearly all other countries. The base map of our series is already on a decimal scale, 1 : 2,500 (though it is usually called 25 inches to 1 mile). At the other end of our series we use, and are pledged to, the 1 : 1,000,000 scale of the International Map. The introduction of the uniform grid system of reference makes it even more important that the several scales should be exactly related. And the revision of the maps gives a convenient opportunity to redraw them on intervening scales of this decimal series. Such a change involves no technical difficulties, nor does it involve the use of metric measures of length; but it would be a departure from established custom though for ordinary purposes the change would not be noticeable on any map other than the 1-inch.

Wind From Quartz Crystals (S. K. L.)

The use of thin slabs cut from quartz crystals as frequency-stabilisers for high-frequency oscillators is well known. Compared with the piezo-electric property which is utilised in these applications, little is known of the other phenomena associated with the mechanical vibrations of these discs.

Some interesting new observations on the wind produced by vibrating quartz crystals are described by S. C. Hight in the *Bell Laboratories Record* for December 1935. A quartz plate 5 cm. square and 2·86 mm. thick was connected in an oscillating valve circuit tuned to the resonant frequency of the crystal plate, which was, in this case, one megacycle per second. Each of the two crystal electrodes consisted of a thin metallic film deposited on the faces of the crystal. A stream of air was found to come from each surface, the stream having a cross-section about the same as that of the crystal, and diverging at a distance of about 1 foot. Turbulence was observed at a distance of several feet from the crystal.

Although a completely satisfactory explanation has not yet been found, a simple analysis of the conditions at the vibrating surface indicates a probable cause of the phenomenon. The surface of the plate reaches a maximum velocity of 500 cm. per second, the acceleration being 20,000 miles per second per second. Air molecules, under normal conditions, have rather less velocity than the maximum velocity of the surface of the plate, and therefore those that impinge on it at the peak velocity receive an outward accelera-

tion of large value. As the surface recedes the accelerated molecules continue to move outwards, and the molecules around the edges of the plate, which still have random motion, move into the vacant space and there receive an outward acceleration during the next cycle of vibration.

In this way a valve action is set up, and if this action were perfect, simple calculation shows that the displacement of air would amount to 2 litres per second. This valve action is nearly independent of the frequency of vibration.

Miscellanea

The New Year Honours List included the following names: *Knights:* B. C. Burt, agricultural expert to the Imperial College of Agricultural Research; Prof. A. Harden, emeritus professor of biochemistry in the University of London; Dr. H. J. W. Hetherington, vice-chancellor of the University of Liverpool; Prof. D. P. D. Wilkie of Edinburgh, member of the Medical Research Council; *C.M.G.:* Willi Fels, for his work in New Zealand on ethnology; W. Nowell, director of the East African Agricultural Research Station; *C.B.E.:* Lieut.-Col. A. MacD. Dick, professor of ophthalmology, King Edward Medical College, Lahore, Punjab; Dr. P. Hartley, director of the Department of Biological Standards, Medical Research Council; J. F. Marshall, honorary director of the British Mosquito Control Institute, Hayling Island; *O.B.E.:* Dr. S. S. Bhatnagar, professor of physical chemistry, Punjab University; T. Crook, principal of the Mineral Resources Department, Imperial Institute; Dr. R. W. Dodgson, director of shellfish services, Ministry of Agriculture and Fisheries; G. E. Holden, technical adviser to the Dyestuffs Advisory Licensing Committee; H. L. Stevens, principal scientific officer, Air Ministry; T. Waites, Government statistician, State of New South Wales.

Sir William H. Bragg was elected president of the Royal Society in succession to Sir F. Gowland Hopkins at the anniversary meeting of the Society on November 30. Sir Henry Lyons was re-appointed treasurer of the Society, Sir Frank E. Smith and Prof. A. V. Hill were elected secretaries and Prof. A. C. Seward, foreign secretary. The occasion was notable in that these, the nominees of the Council, were opposed by others nominated by a group of fellows dissatisfied with the procedure adopted for the election of officers and fellows of the Society.

It is announced that Lord Rutherford will direct the work of

the Royal Society Mond Laboratory at Cambridge, Dr. Kapitza's magnetic apparatus having been sold to Russia.

Prof. Ejnar Hertzsprung of Leiden has been appointed to succeed the late Prof. W. de Sitter as director of the Leiden Observatory. Prof. Hertzsprung was awarded the gold medal of the Royal Astronomical Society in 1929 and is known in particular for his work on double star clusters.

The biennial award of the Symons gold medal of the Royal Meteorological Society has been made to Prof. W. Schmidt of the Central Institution for Meteorology and Geodynamics, Vienna. Dr. F. J. W. Whipple has been elected president of the Society.

Dr. R. S. Clay has been elected president of the Royal microscopical Society.

Sir William H. Bragg has been awarded the Faraday medal of the Institution of Electrical Engineers.

The gold medal of the Royal Astronomical Society has been awarded to Prof. H. Kimura of the Mizusawa Observatory, Japan, for his work on the variation of latitude.

Dr. C. V. Drysdale has been awarded the Duddell medal of the Physical Society for his work on electrical and optical measuring instruments.

Dr. G. A. Young, chief geologist of the Canadian Geological Survey, has been elected President of the Royal Society of Canada.

Dr. H. S. Glassen, professor of physiology in Cornell University, will succeed Dr. Simon Flexner as director of the Rockefeller Institute.

We have noted with regret announcements of the death of the following well-known men of science during the past quarter: Prof. J. H. Ashworth, F.R.S., of Edinburgh University, zoologist; Dr. J. H. Breasted of the University of Chicago, egyptologist; Prof. W. E. Byerly of Harvard, mathematician; Prof. J. D. Cormack, C.B.E., of the University of Glasgow, engineer; Prof. A. F. Dixon, of Trinity College, Dublin, anatomist; Capt. S. R. Douglas, F.R.S., deputy-director of the National Institute for Medical Research; Mr. C. P. Gilchrist, F.R.S., metallurgist; Prof. D. C. Gillespie of Cornell University, mathematician; Sir Richard Glazebrook, physicist; Dr. A. A. Gray of Glasgow, otologist; Prof. V. Grignard of the University of Lyons, chemist; Prof. A. S. Hitchcock of the U.S. Department of Agriculture, agrostologist; Dr. Howard McClenhan, secretary of the Franklin Institute; Prof. Charles Richet, N.L., physiologist; Sir Alfred Sharpe, African

explorer; Prof. J. E. A. Steggall of the University of Dundee, mathematician; Dr. V. K. Ting of Peiping, geologist.

The British Association will meet at Blackpool this year during the week September 9–16. Sir Josiah Stamp is president of the Association and the presidents of the various sections are as follows: A (Mathematics and Physics), Dr. A. Ferguson; B (Chemistry), Prof. J. C. Philip; C (Geology), Prof. H. L. Hawkins; D (Zoology), Dr. Julian Huxley; E (Geography), Brig. Winterbotham; F (Economics and Statistics), Dr. C. R. Fay; H (Anthropology), Miss D. A. E. Garrod; I (Physiology), Prof. R. J. S. McDowall; J (Psychology), Mr. A. W. Walters; K (Botany), Mr. J. Ramsbottom; L (Educational Science), Sir Richard W. Livingstone; M (Agriculture), Prof. J. Hendrick.

A Society for the Study of Alchemy and Early Chemistry has been formed in London with Prof. J. R. Partington as its first president. It is proposed to hold regular meetings and to publish a Journal which will be edited by Dr. F. Sherwood Taylor.

Dempster (*Nature*, Dec. 7, 1935) has confirmed the existence of the two isotopes of iridium (191, 193) whose presence Venkatesachar and Sibaiya deduced from an examination of the hyperfine structure of certain lines in the ultra-violet spectrum of that element. With this work the isotopic constitution of all the elements has been found. It appears that there are in all 261 isotopes and that tin with 11 has more than any other element.

The *Bell System Technical Journal* for October 1935 contains a digest of two papers published in the *Transactions of the Electrochemical Society* dealing with the behaviour of lead, lead-antimony and lead-calcium alloys when used for the grids of storage batteries. At present lead-antimony alloys are usually employed and it has been shown that in the course of the normal operation of such cells antimony passes through the solution from the positive electrode and deposits on the negative where it causes local action and self-discharge. Further stibine is generated in perceptible amounts on overcharge. Until recently, however, no other alloys having suitable physical characters were known. Now it has been shown that, at its melting-point, lead will dissolve 0·10 per cent. of calcium. When this mixture cools the calcium comes out of the solid solution as Pb_3Ca only 0·01 per cent. remaining dissolved at room temperature. The metal so obtained has a strength compar-

able with that of the lead-antimony alloys generally used in storage batteries and an electrical conductivity 20 per cent. greater than that of lead—9 per cent. antimony. Used experimentally for cell grids it has functioned excellently; cells constructed with it having a greater efficiency and losing much less of their charge when left standing than those in which lead-antimony is used.

The *Journal of Research* of the National Bureau of Standards for September 1935 contains a report of an investigation of the effects of fumigants, intended for the destruction of insect pests, on book and writing papers. The results indicate that normal fumigation, *e.g.* with hydrocyanic acid gas, ethylene chloride—carbon tetrachloride or carbon disulphide has no significant deleterious effects. In the same issue of the *Journal* Hoffman and Scribner describe the purification of gallium by fractional crystallisation and in the October number Lundell and Hoffman give an account of a determination of the atomic weight of the element in which a sample containing less than 0·001 per cent. of impurity was used. Their "rounded value" is 69·74. The November issue contains a useful paper by Mueller and Wenner dealing with various adjustable resistance devices, in particular with that devised in 1902 by Waidner and Wolff.

The Report of the Water Pollution Research Board for the year ending June 30, 1935, states that two experimental plants have been erected at Ellesmere, Shropshire, to enable a large-scale test of the processes devised by the Board for the treatment of dairy effluents to be made. The industry, through the Milk Marketing Board, is contributing £3,000 a year for two years towards the cost of the investigation. The *Report* summarises the principal results obtained during the last few years in an investigation of the base-exchange process of water softening. This process is employed by several water supply undertakings and by many industrial firms; it is the basis of the household softeners in common use in various parts of the country. Base-exchange materials in use for softening water include synthetic products and treated minerals. Some of the synthetic products are manufactured in this country but the minerals are imported, mainly from abroad. As a result of the investigation methods of treatment have now been devised whereby materials suitable for softening water can be prepared from certain British clays. From a few of the clays materials have been obtained which are equal in softening value to imported clays and are less liable to deterioration.

One outstanding discovery resulting from the Board's work is that certain synthetic resins possess marked base-exchange properties, greater in some instances than the base-exchange values of existing commercial materials. Other resins have been prepared which remove acids from solution in water. By treatment of tap water with a resin of one type and then with a resin of the second type, the salts in solution were reduced from 33 parts to about 1 part per 100,000. Similar treatment repeated two or three times removed most of the salt from sea water.

Sir Frank Smith gave some interesting figures relating to the use of coal and the development of power in this country in his presidential address to the Junior Institution of Engineers in December last. During the period 1913–34 the results of applied research have effected economies in the use of coal of the order of 35 million tons per year (*i.e.* about a fifth of the total used in 1913) while the total home consumption has diminished by only 22 million tons, the balance being taken by new-users. The economy is most striking in the generation of electrical energy where the average consumption of coal, now 661 tons per million units, has been halved since 1920. At Battersea indeed only 407 tons are used—a figure which represents a thermal efficiency of 29 per cent. Remarkable results have been achieved also in the marine engineering industry where 10 tons of coal now serve to do what required 16 tons in 1913, even so, however, the tonnage of oil-burning ships has increased fifteenfold to 19,885,000 tons in the last twenty years and that of motor ships forty-sevenfold to 11,000,000 tons.

The import of oil in 1934 was equivalent, on a heat basis, to 12 million tons of coal and thus, in a sense, displaced 43,000 men from the coal industry, a figure which becomes quite insignificant when it is remembered that, in one way or another, the motor industry gives employment to about one million. It is to be hoped, however, that in the not very distant future much of this oil may be manufactured in this country. The hydrogenation plant at Billingham has a capacity of about 150,000 tons of petrol per year or 4 per cent. of our present consumption. In Germany the Fischer process which does not require high pressure equipment is being developed and the progress of the work is being carefully watched over here.

We have received a copy of the second Bulletin of the University of London Council for Psychical Investigation containing a description in minute and illustrated detail of a demonstration of fire-

walking by a Muhammadan named Kuda Bux in a garden at Carshalton on September 9 and 17, 1935. The test on September 9 was made to discover the proper conditions for the main experiment on September 17. On that day a trench 11 ft. long, 6 ft. wide and 9 in. deep was used. (A 25-ft. trench was provided but the demonstrator used only half of it.) A fire built up of oak logs and firewood was lighted at 8.30 a.m. and fed during the morning so that, at lunch time, the trench was filled with white hot embers up to ground level. At 2.30 p.m. a load of charcoal was raked over the trench to provide an even surface and this, by 2.50, was cherry red. At 3.14 Bux, his feet bare, the skin soft and dry, made his first walk traversing the trench in four strides so that each foot came twice into contact with the embers and for about 0·5 sec. each time. The walk was repeated in much the same manner at 3.17 and neither immediately nor 48 min. afterwards did the soles show any signs of blistering though an Englishman and another Indian who attempted the walk afterwards blistered their feet after only one contact with the embers. Bux weighed only 120 lb., the two others about 160 lb. Reading the account it may be imagined that a light body and a certain skill in walking so as to give a small time of contact form part of the secret of immunity. One had imagined fire-walking to be a much more sensational affair, but an observer, who had seen ceremonial fire-walking in South India, stated that the test was unusually severe, the layer of embers being too shallow and the fire, by reason of the wind, too hot.

The Bulletin (price 5*s.*) is published at 13D Roland Gardens, S.W.7, and is not an official publication of the University of London.

ESSAY REVIEWS

THE LIVING GARDEN. By SIR ARTHUR HILL, M.A., Sc.D., F.R.S., Director of the Royal Botanic Gardens, Kew. Being a Review of **The Living Garden or The How and Why of Garden Life,** by E. J. SALISBURY, D.Sc., F.R.S. [Pp. xii + 338, with 17 plates and 60 text illustrations.] (London: G. Bell & Sons, Ltd., 1935. 10*s.* 6*d.* net.)

" A GARDEN is a lovesome thing " all the more because it is a living thing and like other living creatures which claim our affection it is lovesome because of its waywardness and fickleness. " Where grows ? Where grows it not ? If vain our toil we ought to blame the culture not the soil " :—" The how and why of garden life " is the concern of the learned Quain Professor of Botany in this admirable book, and he discusses with charm and authority the many questions, often of a fundamental character, which are always being asked and may often be fully answered by the gardener with a perceiving eye, and all the more certainly after a careful study of this work which will make him both think and seek to find the answer.

It is pleasing to find a professional botanist who is also a keen gardener and therefore realises how much can be learnt from the actual handling of plants, not only as to their physiological requirements, but also as to their habits and general morphology.

The bulbous Oxalis, for instance, that curse of a sandy soil, becomes a fascinating study as one uproots it to discover its innumerable bulbils for reproduction, and its corrugated, fleshy, contractile tap root. Or again the willow herb which one may think one has eradicated until in the winter one discovers its brussel-sprout-like rosettes almost buried in the soil ! To the real gardener, studying the scientific principles underlying his craft, it is not only the flowers but also the weeds which provide him food for thought and study and shed light on problems of plant physiology and ecology in particular. All these matters are discussed more especially in Professor Salisbury's chapters on sunlight and shade, vegetative propagation, seed production, weeds and on cutting lawns and hedges. The trained botanist is able to point out to the gardener

the physiological explanation of many a matter of common observation about which he may before have attempted in vain to seek an explanation. Why is it that some plants prefer shade and others bright sunlight? that some succumb to frost and some cannot tolerate unduly wet soil? The competent gardener knows that such things are so, but now he is given the explanation. Professor Salisbury is most illuminating and instructive in his explanation of the all-important rôle of carbon dioxide in relation to the plant and why it is that some plants need all the sunlight they can get in order fully to utilise the carbon dioxide for the making of the materials necessary for their growth. The answer being that such plants admit carbon dioxide very readily into their leaves and need ample sunshine to turn the supply to good advantage. Then there are other plants which do not admit carbon dioxide so readily and so do not need much sunshine, since the plant machine does not require a great amount of power for its successful working. This affords an interesting explanation of the success of Aspidistras in Bloomsbury, since these useful plants are true shade-lovers, and is a sufficient reason for the botanist or gardener to refuse to contribute to any well-meaning society anxious to give them an outing in the sun.

Of equal interest are his remarks on the length of exposure to light that different plants require, for we can now understand how the long summer day of our northern climes is essential to those plants which have to get through their life cycle quickly, while plants like *Cosmos* refuse to flower until the days get shorter. A twelve-hour day is the normal daylight period for Cosmos in its own home, and instead of benefiting, as one might imagine, from our long summer daylight it may even be cut by frost before it comes into flower. Restrict its daylight period to twelve hours, however, by artificial means and earlier blossoming will be the result. Needless to say our old friends chickweed and groundsel can flourish and flower however much or little daylight they receive, and a hard frost is our only hope of ensuring their riddance. Why, however, should the cucumber produce more male flowers when the light is bright and an increased number of females if the light intensity is diminished? How many gardeners, before reading Professor Salisbury's chapter on "Sunlight and Shade," could give a satisfactory answer as to why they religiously carry out the annual garden ritual of tying up lettuces and earthing up celery?

The chapter on "Frost and Fog" gives an opportunity for the reproduction of two excellent pictures showing the effect of the lower temperature on the thinner, more quickly cooled leaf margins

and of the occurrence of water stomata and also some useful information about London fog, that bane of those who grow delicate orchids and Begonias—surely Bignonias on p. 82 is a misprint for Begonias ?—and other susceptible plants under glass in the neighbourhood of London.

When dealing with rock gardens and the distribution of roots in the soil and the many questions of plants in relation to water, Professor Salisbury brings a number of facts of importance before the cultivator which will be of material assistance and will help him to understand the why and wherefore of many practices he has carried out as the result of experience without an exact knowledge of why it is that such and such a method has in some cases proved a success and a failure in others. The importance of soil aeration receives its full share of attention since the success in growing many alpine plants so largely depends on due regard being paid both to the physical constitution of the soil and on the water supply. Tomato seedlings, for instance, will thrive in a good sandy compost but in a stiffer and more compact medium they will make but poor growth. The well-known garden practice of placing cuttings at the edge of a pot is also shown to be due to the better aeration of the soil where it is in contact with the porous side of the pot, and cuttings so placed, as is well known, strike far more easily than those placed in the soil in the centre of the pot.

We are constantly and very rightly reminded throughout the book how in all our garden operations due regard must be paid to the physiological needs of the plant in connexion with the absorption of water and mineral salts, the absorption of carbon dioxide and root aeration.

Then again Professor Salisbury is able to throw light on many interesting points relating to vegetative propagation, a matter which has received so much attention of late years at East Malling in particular, the underlying principles of which have proved to be of great commercial importance. He might have added that the Tree Ivy has resulted from striking cuttings of the flowering shoots of the ordinary climbing ivy which are produced when it grows away from its support. These shoots, which have leaves arranged around the stem, retain this character and the resultant plants are dwarf flowering bushes which never assume the climbing habit. A similar physiological condition is attained by some species of *Vitis* when they come to the flowering stage and cuttings taken from the flowering shoots—like the Tree Ivy—never regain the climbing habit.

Much valuable information and advice is given in the chapter

on "Fertility and Inheritance" which those who wish to reap full advantage from their fruit trees will do well to study carefully.

The quotation, "With all thy getting get understanding," with which Chapter VIII is headed, is one of the many wise sayings from the Book of Proverbs that we would do well to lay to heart. Moreover, it is a real pleasure to discover the parts of the world which have contributed so richly to the adornment of our gardens. With that end in view, Professor Salisbury might have referred to the annual display at Kew of the annuals commonly grown in gardens, which are arranged in beds according to their countries of origin, so that gardeners may see at a glance what for instance North and South America, South Africa, Australia and the Mediterranean region contribute to the feasts of colour in their gardens. Among the trees he refers to he might have mentioned, on p. 130, the remarkable fact that in addition to the well-known Tulip Tree from the Eastern United States another species, *Liriodendron chinense*, was discovered some few years ago in the mountains of Hupeh, China. Geography has led him a little astray with regard to *Leycesteria formosa*, for the specific name refers to the beautiful aspect of this Himalayan shrub and not to the Island of Formosa!

Plant names, whether English or Latin, can afford the gardener pleasant recreation on many a winter evening in hunting up their meaning and discovering now and again some piece of ancient history, as in "Candle-Wick" or in the many plants with the prefix "Lady's," dedicated to the Virgin Mary, such as "Our Lady's Bedstraw," the plant which, according to tradition, filled the Manger at Bethlehem; "Our Lady's Mantle" (*Alchemilla*); "Our Lady's Comb" (*Scandix Pecten-Veneris*), which in Puritan times was changed to Venus' Comb! or the Maidenhair Fern, "The Virgin's Hair." The wonder is, very often, how the working gardener can remember and pronounce the Latin and Greek names which can mean nothing to him: some botanists might fail to recognise in what an old gardener used to speak of as "The China Doctor" the beautiful, pale-blue "Glory of the Snow" of early spring. We are indebted to Professor Salisbury for pointing out the origin and meaning of many plant names that may seem meaningless to the layman, but why did he fail to discover that M. Charles Godet (1797–1879), after whom *Godetia* was named, was the distinguished Swiss botanist and entomologist who wrote the Flora of Jura and described many new species from the Caucasus?

Then he has been a little unfortunate over the Pæony, for there is no record of Pæon, the Physician of the Gods, ever having gone to Paris, though had he done so assuredly he would have been given

his M.D.! Had Paris, however, gone to Pæon instead of to Œnone —who also had a genus named after her—there might have been a pæan in Troy instead of a funeral dirge!

These, however, are minor points, easily rectified in a second edition which should certainly be called for.

We can now admire all the more the cleverness of our plantains, dandelions and daisies in escaping the lawn mower, now that we are shown the way they have become so ingenious; the way our bulbs remain below the ground instead of continually appearing on the surface of the soil like the stones in our garden, and something of that elusive quality of scent in plants and why sunflowers cannot grow at the North pole! The late Sir Robert Ball used to assert that it was because they had to go round and round with the sun with the result that their flower heads got twisted off!

Much more might be said in commendation of this useful book for garden workers and lovers, but obviously it must be read to be fully appreciated and the dish must not be spoilt by any attempt at a *réchauffé*. We can only conclude by congratulating Professor Salisbury on having produced so useful and interesting a book and on having had the good fortune to secure the services of Mrs. G. M. Caroe for the delightful illustrations which add so greatly to its enjoyment.

SCIENCE IN BAGHDAD IN THE NINTH CENTURY. By J. R. PARTINGTON, M.B.E., D.Sc., Professor of Chemistry in the University of London, Queen Mary College. Being a Review of an **Encyclopædia of Philosophical and Natural Sciences as taught in Baghdad about A.D. 817, or the Book of Treasures of Job of Edessa.** Syriac text edited and translated with a critical apparatus by A. MINGANA. [Pp. xlviii + 470.] (Cambridge: W. Heffer & Sons, Ltd., 1935. £2 2*s*. net.)

THE author of the treatise here edited and translated, Job of Edessa, known to the Arabs as Ayyūb al-Ruhāwī, was born at Edessa, probably about A.D. 760 and was still alive in 832. His native town was the ancient home of Syrian Christianity, which was well established there early in the third century. In the fifth century Nestorius, a monk of Antioch, was expelled with his followers from the fold of the orthodox church as holding heretical opinions, and settled at Edessa. The Nestorians were in 489 driven from Edessa by Leo the Isaurian, but by this time they had become an important power in the eastern world, and in the following century their missionary activity extended as far as India, Ceylon and China. Like most missionaries, they were keen educationists, teaching science (especially mathematics), philosophy and medicine, which

they also practised, the oldest physicians mentioned by Arabic authors being all Christians. Their sources were Greek, and translations of Greek works into Syriac, a form of the widely used Aramaic language, constituted the first important medium of transmission of Hellenic and Hellenistic culture and learning to the East. In those days the sun of the West had set, but its declining light cast over the East a long shadow of one of great stature, Aristotle, the representative of the whole of ancient learning, *ille Philosophus* as he is often simply called.

The translations of profane works from Greek to Syriac began as early as the fifth, and continued until the thirteenth century. The end of the eighth and the beginning of the ninth century are notable for a revival in the study of Greek works at Baghdad, the new capital of the 'Abbāsid Caliphs, who caused Greek manuscripts on medicine and science to be brought from Syria, Egypt and Asia Minor, and had them translated by Christians, the most famous being Ḥunain ibn Isḥāq, who died in 876. He mentions Job of Edessa as a translator of Galen into Syriac, and made use of some of his translations. Job also wrote Arabic works, none of which are known, and only two of his works, one on canine hydrophobia and the *Book of Treasures*, have come down to us, the latter having probably been written about 817.

The principal authority in the *Book of Treasures* is Aristotle, but Galen and Hippokrates, as well as anonymous Indians and Persians, are also mentioned. The *Book of Treasures* is a large work for its time: the translation occupies nearly 300 pages, and it would be a difficult and not very useful task to summarise its contents, and to indicate where they are merely borrowed from Aristotle and where they are new. The Editor has indicated this in some cases, but his treatment does not give the impression that he has made a sufficiently exhaustive study of the parts of Aristotle which might be expected to be reproduced, although he has been careful to indicate in many places where the treatment diverges from that of the Greek. In the introduction he summarises the contents of the work under the headings of metaphysics, theology, psychology, biology, anatomy and physiology, medicine, chemistry and physics, music, mathematics and astronomy. The whole forms a kind of small encyclopædia, and it would be useful to compare its contents with those of the similar work of the Persian, Muḥammad ibn Aḥmad al-Khwārizmī, written in 976 in Arabic. As the Editor remarks, many of the discussions show a desire to find reasons for natural phenomena which modern science leaves unexplored for lack of positive data, and the early scientists generally

hoped to know more of ultimate origins than we now consider possible or desirable.

In metaphysics the view is expressed that the only kind of matter is concrete, synonymous with a material body, endowed with the quantity and quality possessed by all bodies in the universe. The existence of the primary matter of Aristotle, which received the two pairs of opposites, heat-cold and dryness-humidity, is rejected on the ground that matter, not being endowed with any quality whereby it may be conceived, cannot receive things dissimilar to itself. The Aristotelian doctrine of contraries is, however, accepted and very fully worked out: when the elements cease to strive, it is said, the generating activity of the universe will come to an end. In theology, the author rejects the resurrection of material bodies, and in psychology he believes that the soul is quite distinct from the animal spirit found in the material organs of the brain, and in the union of the soul with the body it keeps its entity without change. In biology the distinctions into genera and species of animals and plants are traced back to the elements. Lower forms of life such as vermin and worms are thought to be derived from the action of the elements working in corrupted bodies, and such lower forms of life are not supposed to procreate. The origin of animal bodies is, as usual, explained as due to coction of the primordial liquid matter or chyle, which was the result of the coming together of the simple elements. It is implied that both the sexes participate in the production of the embryo, which takes its qualities from both of them.

The medical theory is that of the four humours, and health is regarded as the equilibrium of the elements. Disease is caused by predominance of an element; it is cured by drugs exerting the opposite effect and re-establishing the former harmony. The purgative and vomitive effects of certain drugs are explained by the degrees of their active powers as compared with the power of resistance of the stomach.

In his chemical and physical sections the author is essentially on Aristotelian ground, and he appears to have been ignorant of the chemical works which still exist in Syriac versions. He makes mention of alchemy and has no use for atomic theories. In the latter he follows Aristotle's judgment, and like him he explains properties in terms of the four elements, which are mixed in varying proportions in material bodies. There are no simple bodies in the chemical sense, since even these may be reduced to the four elementary properties. Combustion is explained as in Aristotle. The theories of tastes, colours (of which white and black are

primordial and the others formed from them) and sounds offer nothing noteworthy, and in meteorology Aristotle is again the main guide. In his physical geography Job says that the inside of the earth is warmer in winter than in summer, that mountains have more trees than plains because the contraction of their parts imprisons heat and moisture within them, and that the salinity of sea water is due to the extraction of the thin volatile part by the heat of the sun and the further reception of dryness and thickness from the upper surface of the earth.

In mathematics, a distinction is made between the true mathematician and the arithmetician who merely juggles with numbers, yet there is a good deal of discussion of "perfect" numbers and the relations of numbers to parts of the universe. An ingenious explanation of the origin of the cipher is given: the early mathematicians used the fingers for counting numbers, but having reached the ninth finger, which would be the forefinger, they began again with the first finger, which would be the little finger of the other hand. In order to find a link to connect the number 9 with the number 1, they formed the cipher, by linking the forefinger and the thumb together in a circle. The Editor states that Job of Edessa is the earliest writer to mention the cipher, as it was in his time that this arithmetical sign reached Baghdad.

In astronomy, the author believes that the stars are composed of the same elements as the earth, but endowed with a circular movement, as contrasted with a linear movement on the earth. Since a circular movement has no end, the stars will have no end whilst the earth will necessarily come to an end. The firmament is a real body and not an optical illusion caused by distance. The opinion of the ancients, that the stars are reasonable beings endowed with a rational soul and influencing the destiny of mankind, is severely criticised.

The foundations of European science are Greek, but the process by which the Greek works became known to the European Middle Ages is a curious one. The texts lay neglected in libraries until they were taken to Baghdad, and there they were clothed in an Arabic dress. In the twelfth century they came back in the form of Latin translations made principally in Spain, and much of the material was thought to be a gift of the Saracens. Both the beginning and end of this journey are in many respects still imperfectly known, and the publication and translation of such texts as the *Book of Treasures* cannot fail to be interesting and useful to students of the history of science. The great bulk of Syriac literature is theological, and a scientific work is, therefore,

of particular interest and importance. The Editor states that the present volume is the first of a series of scientific publications from manuscripts in his collection, and the subsequent volumes will be awaited with considerable interest.

THE CRANIAL MUSCLES OF VERTEBRATES. By G. R. DE BEER, M.A., D.Sc., Department of Zoology and Comparative Anatomy, The University, Oxford. Being a Review of **The Cranial Muscles of Vertebrates**, by F. H. EDGEWORTH, M.D., M.A., D.Sc. [Pp. x + 493, with 841 figures.] (Cambridge: at the University Press, 1935. £5 5s. net.)

PROFESSOR EDGEWORTH has put all anatomists and zoologists in his debt by the preparation and publication of this splendid volume in which he has collected, collated, and amplified the results of the work on which he has been engaged since the beginning of this century.

The development of the muscles of the head in all vertebrates is a subject which the author has made peculiarly his own, and this monograph, clearly written, logically arranged, and well illustrated, with its useful index and list of synonyms, is a notable addition to the literature of comparative anatomy. It is perhaps reserved for the specialist in vertebrate anatomy and embryology to realise and appreciate the monumental amount of work, patience, care and time, which the preparation of such a volume must have involved. The figures are beautifully clear, and so carefully labelled that reference to the explanation of abbreviations is rarely necessary.

Professor Edgeworth is to be congratulated on having brought to such felicitous completion his arduous self-set task, and on having produced a standard work of reference for all time as to matters of fact connected with the development of the cephalic muscles in vertebrates. While there cannot be two opinions about this, it must unfortunately be said that the same measure of agreement will not be obtained in regard to the more theoretical and comparative parts of the work, and to the phylogenetic and general biological conclusions which the author has attempted to draw.

Starting from the fact that in Selachii and Teleostomi the mandibular muscle-plate is divided into dorsal and ventral portions whereas in Holocephali, Dipnoi and Amphibia it is not so divided (*i.e.* remains in a condition through which the Selachii and Teleostomi pass), Professor Edgeworth draws the conclusion that the Holocephali, Dipnoi and Amphibia "show more nearly the primitive vertebrate state." Zoologists will want good evidence before accepting such a conclusion which is diametrically opposed to the

view generally held as to the relative phylogenetic positions of these groups of vertebrates.

It is therefore of importance to note that Professor Edgeworth's conclusions are based on the (now discredited) supposition that a condition which is embryonic in ontogeny must be primitive in phylogeny. Controversy regarding the justification of this supposition has not been wanting in recent years, but without reopening it, it may be noted that this very application of the view is self-condemnatory.

Continuing, Professor Edgeworth holds that the condition of the palatoquadrate bar in Holocephali, Dipnoi, and Amphibia where it is immovably fused to the skull (the use of the terms "monimostylic" and "streptostylic" as applied to the chondrocranium is inadmissible), is more primitive than the condition in Selachii and Teleostomi where it is movable in the adult (though it is often temporarily attached to the brain-case during earlier stages of development). Bearing in mind the fact that the palatoquadrate is merely a modified visceral arch, similar in every principle to the branchial arches behind it, which arches are quite clearly independent of the neurocranium, zoologists will decline to agree with the author that "there was a fixed horizontal palatoquadrate in the primary Vertebrate stock, attached anteriorly, in front of the optic nerve, to the ethmoid region or the trabecula, by a basal bridge to the posterior end of the trabecula, and by an otic process to the auditory capsule." In this connexion, it may perhaps be mentioned that the author's attribution to de Beer and Moy-Thomas of the view that the Selachii are descended from a Holocephalian stock is undeserved. Those authors were concerned merely to show that, in their possession of a non-suspensorial hyoid arch skeleton, the Holocephali show a feature more primitive than any living Selachian.

On the contrary, the only satisfactory view is that the brain-case and the visceral arch skeleton were originally phylogenetically quite distinct, that subsequently certain processes of the foremost elements of the latter became *movably* affixed to the former, and that, lastly, immovable fusion has taken place in certain forms which for other reasons are clearly specialised. A detailed study of the chondrocranium in all vertebrates shows that no phylogenetic significance whatever can be attached to temporary fusion of cartilaginous elements. In many Teleostomi the quadrate is temporarily fused with the hyosymplectic; in Ornithorhynchus the malleus is fused with the incus and the stapes: to conclude therefrom that originally the skeletal bars of the first and second visceral

arches were connected would be ridiculous, and unwarranted by any principles of morphology. The reasons for such embryonic fusions are still unknown, but it is quite clear that they are not necessarily phylogenetic. Meanwhile, it may be noted that the non-division of the mandibular muscle-plate (in Holocephali, Dipnoi and Amphibia) seems to be a natural consequence of the acquisition of an immovable palatoquadrate.

Attention may now be turned to Professor Edgeworth's treatment of the extrinsic eye-muscles. As is well known, these arise in Selachii from the first three somites of the body, simply and regularly innervated by the three ventral nerve-roots corresponding to these segments. In other forms, the conditions are obscure, owing to the fact that a complete and uninterrupted row of somites from the first segment to the segments of the trunk can no longer be demonstrated with such ease. In Teleostomi the eye-muscles appear to arise from the first two somites only, while in Dipnoi and Amphibia hey appear to arise from the first somite alone, and this is the condition which Professor Edgeworth regards as the most primitive. This implies that the three ventral nerve-roots which innervate the eye-muscles, are here concerned in the innervation of the products of a single somite. Quite apart from the question as to whether somatic motor nerves *can* innervate somatic muscles of segments not corresponding to them, no morphologist could agree that such a condition as the author describes in Dipnoi or Amphibia could be primitive. The easily demonstrable positive fact of the regular and simple arrangement of the eye-muscles in Selachii will continue to appear as the more primitive to zoologists, and it is the conditions in the other groups which fail to conform (or appear to fail to do so, for when the somites are not clearly marked, it is very difficult to decide the provenance of any given group of cells) to this scheme which they will continue to regard as specialised. It may be added that the eye-muscles in Cyclostomes arise from the first three somites, and the work of Addens has shown that in spite of gross anatomical appearances, the innervation in these animals conforms to the simple regular, segmental plan.

Professor Edgeworth's phylogenetic series are simply the reverse of those which zoologists prefer to hold.

Professor Edgeworth rejects van Wijhe's theory of the mesoderm, not only because of his views on the origin of the eye-muscles, but also because he does not agree with van Wijhe's identification of the branchial muscle-plates with the cephalic portion of the splanchnic lateral plate mesoderm. In fact, Professor Edgeworth regards the branchial arch muscles as somatic although they are innervated

by dorsal cranial nerve-roots. But as van Wijhe's theory continues to be the only one which gives a satisfactory explanation of all the facts relating to the site of origin, neurology, histology, and the segmental or non-segmental nature of the mesoderm in the vertebrate head, better-found and more cogent objections to it than those which Professor Edgeworth adduces are required before it can be given up.

The author is impressed by the purposeful character of the development of the cranial muscles. This is as may be, and his view will command and obtain such respect and attention as is rightly due to an investigator who has in so masterly a way covered the whole of a large, intricate, and difficult field of research. But if, as Professor Edgeworth thinks, the development of the cranial muscles provides evidence for teleological views, then it is a pity that he should turn to Experimental Embryology for support. That branch of science is steadily and uninterruptedly accumulating evidence that development is a sequence of causally related events succeeding one another by necessitation *a tergo* and not *a fronte*, in which sequence there is no room and no need for teleology. Even the view that the organism is always a whole has had to give way before the demonstration that during the so-called mosaic phase of development it is nothing but an aggregate of independent self-differentiating parts, and is incapable of regulation.

However, it is always open to everyone to draw his own conclusions from the facts at his disposal, and in this case it must not be forgotten that it is to Professor Edgeworth that we are indebted for the facts, in this storehouse of observation at the disposal of present and future workers.

TRANSLOCATION IN PLANTS By H. H. Dixon, Sc.D., F.R.S., Professor of Botany in the University of Dublin. Being a Review of **The Translocation of Solutes in Plants—A Critical Consideration of Evidence Bearing upon Solute Movement,** by Otis F. Curtis, Professor of Botany, Cornell University. [Pp. xiv + 273, with 13 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 18*s*. net.)

Professor Curtis has written a book which, if it is provocative, is sure to evoke research. It gives a very readable account of his own work and of the literature of the subject, and brings out the contradictory nature of the conclusions arrived at. The discussions are often lengthy and the reader is often surprised at the ingenious use which is made of apparently irrelevant facts. The whole gives the idea that the author's views have undergone modification during

the writing, and opinions will probably differ as to the clarity of its arrangement and the cogency of its arguments.

The work presents three main theses: (1) No effective translocation occurs in the wood; (2) the normal path of solutes is in the phlœm; (3) while none of the hypotheses as to the mechanism of transport in the phlœm seems adequate, yet the author favours that in which the streaming of protoplasm plays an essential part.

With regard to the first thesis, Professor Curtis relies principally on evidence supplied by ringing-experiments. He records that the shorter the region above the ring in the bark the smaller is the growth of the apical bud. He sees in this result evidence that some building material, normally supplied through the bark fails to reach the bud through the wood. There is no endeavour to show that there is a proportionality between the amount of building material within the ring and the amount of growth subsequently shown. Indeed, from the data quoted it appears that the elongations of the buds were as 6 : 8 but the volumes of the supplying tissues must have been approximately 3 : 7. It is assumed that in this reduction of growth, the supply of carbohydrates has formed the limiting factor, and hence the reduced growth indicates that the xylem does not form a channel for the movement of carbohydrates.

Double-ringing experiments isolating a defoliated region of the stem from leafy regions above and below are used on page 23 to show that, if at the start the stem is starch free, no starch, derived from the photosynthesis of the leaves above and below the rings, is deposited in the defoliated region; while, if the upper or the lower ring is omitted, deposition of starch in the defoliated region occurs. The conclusion that the double-ringed region has received little or no carbohydrate through the continuous xylem, is scarcely established when we learn—although only on page 239—that 17 of the 20 axes used in this experiment had produced weak shoots between the rings. This latter fact of course indicates that some carbohydrate has actually been translocated to the region between the rings for the construction of these shoots.

The suggestion that injury to the wood by ringing may be responsible to a greater or less degree for reducing a normal passage of carbohydrates and other solutes through the wood is discussed in various places in the book. It is admitted that exposure of the surfaces of the wounds to evaporation and clumsy experimentation, as practised by other investigators, may injuriously affect the viability of the wood and lead to plugging of the tracheæ; but ringing as carried out by Professor Curtis and wounds coated with

warm paraffin wax do not produce the slightest signs of injury. It does not, however, appear how the broken water columns in the outer tracheæ are re-established, or how the congealing paraffin prevents the exudation of colloidal substances from cells opened by the operation, or the spread of morbid changes from the wounds. It is true that the visibility of these effects may be minimised by cutting down the oxygen supply and so reducing pigment formation. The consequences of the physiological shock of ringing are also unknown.

Professor Curtis' final proof, that the xylem has no function in carbohydrate transport, is provided by his experiment in which he compares the growth and carbohydrate content of the defoliated ends of shoots which have been subjected to three different types of operation. In the first series the continuity of the bark has been completely interrupted by a ring below the defoliated region. In the second series, below a similarly defoliated shoot-ending, the wood is completely removed for a short distance in the stem. The third series act as controls in which the behaviour of the terminal bud on a defoliated stem-tip may be observed. To lessen the shock, to prevent drying of the wounds and to supply the two series of experimental shoots with water, the regions of operation are enclosed in glass collars. These collars are sealed to the stem below by melted paraffin wax so that they hold water and keep the cut surfaces immersed during the experiment. The results of this experiment show that the average growth of the buds on the ringed shoots was least; while the excision of the wood was associated with less disturbance. These results are interpreted as proving "that cutting the phlœm (bark) prevented food transfer, whereas cutting the xylem allowed for approximately normal transfer." The validity of the deduction, however, is rendered doubtful by the other records, which are given in the table setting forth these results. Here it appears that, when the continuity of both xylem and phlœm was severed, the growth in nearly every case was greater than when the phlœm alone was cut. In one series the average growth of the terminal bud, ostensibly completely cut off from *all* food supply, excelled that of the controls for the first three days. In a second table giving the sugar content of the defoliated regions in similar experiments, it appears that in none of the cases, where ringing seemed to check the growth, had the sugar supply above the ring been completely exhausted. In some there was even a considerably higher percentage of sugar in the ringed than in either that with the xylem extirpated or the control. I have referred to these experiments more fully than perhaps their

importance justifies, because Professor Curtis has done me the honour of saying it would be interesting to know how I would explain the continued transfer when the xylem is completely removed (p. 104). I may be permitted firstly to remark that I hope I will never try to explain away any clear evidence, which is brought against any theory I have advocated. In the case now before us, I see evidence that certain drastic operations have been followed by reduction in bud-growth ; I see no evidence that this reduction is due to an assumed impossibility of carbohydrate movement through the xylem. So far as the evidence of ringing experiments goes at present, all that can be said is, that sometimes ringing checks the growth of the terminal bud markedly, and sometimes as in Clements' experiments its effect is by no means so profound. The evidence presented in the records before us does not prove that the diminution in growth is due to the want of carbohydrate foodstuffs. Several other factors such as the lack of necessary growth hormones, and the disturbance of various physiological processes may equally well reduce the growth. That in many cases growth continues indefinitely after ringing, seems to me to demonstrate that carbohydrates, to some extent at least, continue to move upwards in the wood. Consequently one cannot help feeling that, where the evidence presented in favour of the movement of carbohydrates in the phlœm is so ambiguous, it is a pity Professor Curtis has not made use of Mason and Maskell's really conclusive experiments demonstrating the movement of these solutes in the phlœm. Their beautiful experimental work on the transport of carbohydrates through strips of bark, separated in some cases entirely from the wood, and into flaps of the bark is so clear and convincing that it must be regarded as the most important contribution to our knowledge of translocation in plants during the last forty years. It is remarkable that the only evidence from these investigators, quoted by Curtis in this connection, is a double-ringing experiment which indicates quite clearly that some translocation of carbohydrate has taken place through the wood. The results of the series in which this experiment is included are, in the book before us, rendered difficult of comprehension as they are explained by reference to an unsuitable figure.

With the evidence at present available it will be hard to convince anyone, who has observed the flow of sap from the wood of a cut vine-stem or sycamore, and tested it for sugar, that the wood is never the channel of translocation for carbohydrates. The usual presence of sugars in tracheal sap confirms this view, failing evidence of some peculiar mechanism for fixing the solutes at the exact

level at which they are found. The amount of carbohydrate transported by this channel varies largely from season to season and from species to species—hence the divergences in the results recorded.

The examination of the evidence bearing on the path of movement of nitrogen and ash constitutes is similarly conducted. Ringed branches produce small and poor shoots with few and small leaves. This should convince us that the substances under consideration cannot move in the wood, but normally move in the phlœm. We are to suppose that the observed growth, reduced though it be, has taken place without the addition of any nitrogen or ash constituents, and we are to discount such observations as those of Clements on the vine and other plants, where the amount of nitrogen and ash constituents have increased forty fold after ringing. Again, double ringing experiments on twigs of Ligustrum are quoted and it is noted that Mason and Maskell place a different interpretation on the same data. It may be observed that in the refutation of the view expressed by these latter investigators Curtis uses a chain of dependent probabilities which seems finally to lead to an indeterminate probability that nitrogen would enter the transpiration stream. To arrive at this conclusion Curtis uses, as in many other places in the book, a principle that the reduction of the carbohydrate content of a cell, or tissue, leads to its giving up nitrogen to surrounding tissues or solvents. Personally I am not acquainted with the evidence on which this principle rests. Before closing his statement of the evidence that soil solutes normally ascend in the phlœm Professor Curtis makes an arresting statement: "If carbohydrates are carried almost exclusively through the phlœm . . . it seems unlikely that the plant can have developed a mechanism that would carry one type of solute exclusively through the xylem and another through the phlœm." This argument depends on so many suppressed middle terms that I confess I see little relation between the conclusion and the expressed premise.

It is not clear how the long discussion on the reality of a diurnal fluctuation of nitrogen and ash constituents in the leaves was likely to supply evidence for or against the view that these substances move upwards in the phlœm. The same may be said of the discussion on the relation of transpiration to solute absorption and we are not surprised to learn that the author hardly considers that these studies can offer conclusive proof that salts are carried in the transpiration stream.

In spite of the various intimations of the uncertainty of the

second main thesis of the book, the final section of Chapter III comes as a surprise. Here it is pointed out, after certain preparatory remarks relating to the effects of the concentration of certain non-toxic soil solutions, that when nitrogen is deficient in the soil solution the living cells of the root will retain it and prevent it from passing through them into the xylem; but if nitrates and other nutrients are in *excess* in the soil, they cannot prevent the *excess* from passing into the xylem and transpiration stream (p. 79). Here Professor Curtis has the credit of "inventing a formula", which, as politicians say, will be acceptable both to the advocates of the xylem and to those of the phlœm as the normal channel of transport for soil solutes. With diplomatic craftiness the cells, and not rival physiologists, are called upon to decide when the solutes are in *excess*. Later on he amplifies this with the statement that there seems little doubt that if solutes get into the transpiration stream they will be carried with it unless removed. The final words of Chapter III would appear to seal the permanence of the reconciliation. "Recent experiments with herbaceous plants indicate a secretion into the xylem by actively absorbing roots. This is contrary to the suggestion that living root cells would prevent entrance (of nitrogen and ash constituents) into the xylem" (p. 87). One wonders if, after the statements made in Chapter II, sugars are included in these solutes. One is encouraged to think that they are from the sentence, "the cells evidently lose carbohydrates which pass into the water-conducting vessels" (p. 119). Later on there are relapses from this position, as is evidenced by the statements: "The finding of appreciable amounts of solutes (in the tracheæ) during the limited season of bleeding is not very strong evidence of transport." "Again the finding of inorganic constituents and nitrogen in the tracheal sap seems a more serious difficulty to the acceptance of the phlœm hypothesis than is that of finding sugar." "The maximum (concentration of inorganic solutes as shown by the) conductivity is shortly after full bloom (*i.e.* May, June, July and beginning of August). . . . Transpiration can hardly be taking place at a very high rate at this time, so it is possible there is no great amount of transport"—a truly marvellous conclusion.

In the following chapter the method of movement through the phlœm is considered and an account of the views of the old writers is given. The hypothesis of Münch and its modification by Crafts are described and criticised.

Then follows a rather nebulous account of the hypothesis accounting for transport by moving protoplasm. One cannot dis-

cover whether the streaming of the protoplasm is supposed to function as a mixing mechanism maintaining an uniform concentration throughout sections of the phlœm, or whether the circulating filaments are visualised as acting like transporter-bands conveying solutes through the sieve-tubes and perhaps through the plasmodesmata. Possibly both are envisaged acting simultaneously. To the reviewer the most serious objections assigning a principal rôle in transport to the streaming of the protoplasm are: (1) While Mason and Maskell have conclusively shown by experiment for the first time that sucrose is chiefly transported through the sieve-tubes, there is no evidence that streaming occurs in the mature sieve-tubes. The observations of Strasburger and others that preparations, showing protoplasmic movement in adjacent elements, reveal none in the sieve-tubes negative the idea that the method of preparation has permanently put a normal mechanism out of action. (2) It has been impossible to demonstrate experimentally that tissues showing active streaming in their cells accelerate sensibly the transport of solutes.

Finally, the suggestion of Van den Honert that transport is effected along the interface between the protoplasm and the vacuole of the sieve-tubes is described. No evidence is as yet apparently available for discussing this theory, which, to the reviewer's mind, seems to resemble the adsorption theory of Mangham (1917) in so far as adsorption may be regarded as a surface-tension phenomenon. Schumacher's observations on the transport of fluorescein are described but his opinion that its movement is independent of streaming protoplasm is not accepted. Cataphoresis also is discussed but Curtis' final conclusion is: the protoplasmic streaming hypothesis seems at present best adapted to meet the requirements of transport.

The last chapter is a discussion of the apparent interdependence of the transport of solutes in the plant and the behaviour of the parts supplied, and an endeavour is made to evaluate the evidence emerging as a criterion of the various theories of the mechanism of motion in the phlœm

REVIEWS

ASTRONOMY AND METEOROLOGY

The Binary Stars. By ROBERT GRANT AITKEN, Director of the Lick Observatory. Second edition. McGraw-Hill Astronomical Series. [Pp. xii + 309, with 4 plates and 13 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 21s. net.)

THE original edition of Dr. Aitken's well-known book on double stars has long been out of print. The new edition will be welcomed not only by those who are actively interested in double-star astronomy, but also by students of astronomy and by astronomers in general. Dr. Aitken is himself the foremost living observer of double stars, and the observation and study of double stars has been his life's work. No one is better equipped therefore than he is for dealing with this subject. It is not inappropriate that almost his last task, before retiring from the Directorship of the Lick Observatory and from active observation, should have been the preparation of this new edition.

This edition follows closely along the lines of the original, which has been revised throughout and amplified to take account of the extensive work done in the intervening period. The advance in the southern hemisphere has been particularly striking, through the observations of Dr. van den Bos and Mr. Finsen at the Union Observatory, Johannesburg, and of Dr. Rossiter, Mr. Jessup and Mr. Donner at the Lamont Observatory of the University of Michigan, Bloemfontein. In the northern hemisphere, Dr. Aitken continued to observe assiduously up to the time of his retirement. The material available for a statistical study of double stars is therefore much more comprehensive than in 1918 and it is in the portion of the book which deals with the statistical data that most changes are to be found.

A detailed analysis of the contents is not necessary, as the book is so well known. After a valuable historical introduction, methods of observing double stars and of computing their orbits are described. Only a selection of the many different methods for computing orbits is given, but references to other methods are added. Spectroscopic and eclipsing binary systems are dealt with in turn. A general discussion of the known orbits is followed by chapters dealing respectively with statistical data and with the origin of binary stars.

It is of interest to note that the general summary of the more important conclusions provided by the statistical discussion, given on p. 273, do not differ essentially from those in the first edition. The work of nearly a decade has served but to strengthen them. They lead Dr. Aitken to the same verdict as before: "We have, apparently, one unbroken progression of series of orbits from systems in which the two components revolve in a fraction of a day in circular orbits and practically in surface contact, to systems in

which the components, separated by one, two, or many hundreds of times the distance from the Earth to the Sun, revolve in highly elliptic orbits in periods of hundreds and even of thousands of years."

Dr. Aitken therefore rejects the conclusions of Jeans and others that binary systems with periods of less than about 55 days were formed by the fission of a single star, but that systems with longer periods must have been formed by some different process or processes. His final conclusion is that "although the observational data that have been accumulated clearly indicate the common origin of the binary stars, no theory of that origin and of the subsequent development of the observed systems that has so far been formulated can be regarded as satisfactory." Here is a problem of the first importance for cosmologists to make a fresh attempt to solve.

The two tables at the end of the volume, containing the elements of orbits of visual and spectroscopic binary stars, are of great value for reference purposes, the more so as the authority for the elements for each star is given.

A few errors have been noted. There are obvious errors in the equations at the top of p. 92 and near the bottom of p. 198. In the first line of Table 3, p. 207, the average period should be 2·736 days.

H. S. J.

Through My Telescope. By W. T. Hay, F.R.A.S. [Pp. xiii + 128, with 7 plates and 17 figures.] (London: John Murray, 1935. 3*s*. 6*d*. net.)

Mr. Will Hay has earned a wide reputation on the variety stage for his originality as a pedagogic humorist, but to a more select circle he is well known as an enthusiastic and successful amateur astronomer. This little book is intended to serve as an introduction and guide for the beginner who wishes actually to observe some of the wonders of the heavens, and is not content merely to read about them. In twelve short chapters the author deals completely and concisely with the various bodies in the solar system; the stars and nebulæ are treated more briefly, and the reader is also introduced in a clear and intelligible fashion to the elements of spectroscopy. Mr. Hay's experience as a practised observer adds considerably to the interest and value of the book; he tells of what he himself has seen and of what his readers may expect to see, and he concludes with a chapter of useful hints on the use of a telescope.

R. W. W.

Through the Weather House, or the Wind, the Rain and Six Hundred Miles Above. By R. A. Watson Watt. [Pp. xi + 192, with 8 plates.] (London: Peter Davies, Ltd., 1935. 7*s*. 6*d*. net.)

The "weather house" of the title is the atmosphere, which the author describes as made up of an indefinite number of stories, each about 10 kilometres in height. Mr. Watt has set out to describe in simple terms the happenings in the atmosphere, from the weather which develops in the ground floor of his weather house, up to the Kennelly-Heaviside and Appleton layers, at heights of about 100 and 225 kilometres respectively, which take part in the transmission of wireless waves.

As is perhaps natural for a radio expert, the author approaches the phenomena of radiation and absorption from the standpoint of a com-

parison with wireless, and he succeeds in giving an account of these phenomena which is clear and on the whole accurate. The best part of the book is that which deals with radio transmission and with the manifestations of electricity in the atmosphere, where the author's command of his subject has enabled him to give the essentials in very few words.

The chapters which deal with the weather as we normally understand that word give an account of the organisation which is required for the making and collection of the observations on which the forecasting of weather is based, and a brief account of the post-war conceptions of depressions. These chapters are very clear and give an excellent critical account of the present status of meteorology.

The readers of this book will learn some of the most important facts of the weather, and will further acquire an understanding of the difficulties with which the meteorologist is faced. The book owes its origin to a series of talks on weather given by Mr. Watt in the National programme of the B.B.C. during the year 1934. The picturesqueness which is perhaps an aid to the broadcaster is not always an aid to the writer, and the author is to be congratulated on the extent to which he has managed to overcome this handicap.

D. B.

PHYSICS

Intermediate Physics. By C. J. Smith, PhD., M.Sc., A.R.C.S. Second edition. [Pp. xii + 900, with 655 figures.] (London: Edward Arnold & Co., 1935. 16s. net.)

The first edition of this book was published in 1932 and it was so well received the publishers had to reprint in 1933 and again in 1934. In the new edition considerable portions have been rewritten and new matter added. Chapters on Heat have been revised, the subjects of Interference, Diffraction and Polarisation have been more fully treated and a short section on Supersonics has been added to the Acoustics. The main change is in the Magnetism and Electricity, the treatment of which in the first edition was somewhat limited. This part has been completely rewritten and now more than satisfies the requirements of the various Higher School Certificate examinations and first year University Courses.

It is refreshing to meet with a text-book which is not a hash of its predecessors. Dr. Smith has provided fresh food and he has served it up excellently. The diagrams are clearly drawn and there are numerous exercises at the end of each chapter with answers at the end of the book.

It is not a little surprising in these days of quanta that modern text-books continue to cling without question to the time-honoured objection to Newton's corpuscular theory of light. While experiment proves that the velocity of the wavefront diminishes in an optically denser medium, the corpuscular theory demands an increase in the *momentum* of the corpuscle. The two theories are not inconsistent if the momentum of the corpuscle is inversely proportional to the velocity of the wavefront. The corpuscle might be identified with the quantum in which case its momentum is the ratio of Planck's constant to the wavelength of the radiation. This merely means that the velocity of the wavefront is not the velocity of the corpuscle.

There are few errors and omissions. On p. 319, line 32, "Fig. 16.11" should be "Fig. 16.13." On p. 350, "A large dish A, Fig. 18.9"—the

" A " does not appear in the figure. " Receiver R " on p. 526, line 3, should be " Receiver T_2 " and " R " in line 9 should be " T_2 " to agree with Fig. 31.11.

The new edition is available in five separate parts varying in price from 2*s*. (Acoustics) to 6*s*. (Magnetism and Electricity).

G. W. T.

Sound. A Physical Text Book. By E. G. Richardson, B.A., Ph.D., D.Sc. Second edition. [Pp. vii + 319, with 111 figures.] (London: Edward Arnold & Co., 1935. 15*s*. net.)

Many will be familiar already with Richardson's *Sound*, first published in 1927, but, for the benefit of those who are not, we may give some idea of the range of its text by stating that, while assuming a standard of knowledge of physics and mathematics up to " Intermediate," the book aims at covering the requirements of the Universities final examinations, besides attempting to meet the needs of research workers and technicians. Not the least useful feature of the book was the inclusion of references to all complete papers of the two preceding decades. With the advent of the second edition these features are naturally continued and brought up to date, although, with the marked advance in applied acoustics, to record a complete bibliography is becoming increasingly difficult.

A survey of the new edition reveals a number of minor improvements distributed throughout the volume, together with a major change in the addition of three new chapters, *viz.* Supersonics, Acoustical Impedance and the Reproduction of Sound. All of these subjects are of increasing importance and correspondingly more space has been devoted to their consideration.

Probably no book will ever be entirely free from criticism, so that any offered here should be interpreted as constructive for a future edition. In this light corrections should be applied to the value velocity of sound in sea water (p. 33) and to the circuit on p. 247, whilst the ordinates of the curves on p. 51 are, of course, R/R_{max}. Again there are just one or two places where a greater clarity of description would be welcomed, *e.g.* the superposition of two stationary vibrations (p. 43) and the direction finding at sea by a single microphone (p. 301) where the " light body " hydrophone and the more familiar diaphragm hydrophone appear confused.

Throughout the work Richardson loses no opportunity of applying acoustical theory to musical instruments. The present edition can be recommended as extremely useful alike to students and research workers.

R. E. G.

A Symposium on Illumination. Edited by C. J. Webber Grievson, B.Sc., M.A. (Oxon.). With a foreword by Lieut.-Col. Kenelm Edgcumbe, M.Inst.C.E., M.I.E.E. [Pp. xv + 229, with 109 figures, including 20 plates.] (London: Chapman & Hall, Ltd., 1935. 13*s*. 6*d*. net.)

The subjects covered in this publication of lectures by nine specialists in different branches of the illumination art range from photometry, the physics of radiation and the technical details of gas and electric lamps to the relation between lighting and health and efficiency, the use of light in interior decoration and the organisation of municipal street lighting. The book aims at giving the reader an account of the principles of modern lighting which shall be neither too technical nor yet too elementary. On the whole

it succeeds remarkably well in this and the result is a stimulating text for the illuminating engineer or in fact for anyone with some scientific knowledge who is interested in lights and lighting. The most satisfactory chapters are those in which the contributor has had to deal with a comparatively limited topic such as the redistribution of light by means of reflecting and refracting equipment (G. H. Wilson), characteristics of electric filament lamps (W. J. Jones), daylight and artificial daylight (J. W. T. Walsh). Where a wider field has had to be covered the result is not always so good. The treatment of photometric standards, lamp photometry and illumination measurements (J. T. MacGregor-Morris) suffers from an excess of detail on some points and superficiality on others. The space allotted to barretter control, for example, might well have been devoted to a fuller description of flicker photometry including a statement of the conditions under which direct comparison and flicker photometry give the same results. The chapter on lighting for decoration and entertainment (A. B. Read) is admirably illustrated with photographs which show what has already been accomplished in this direction and the text outlines fascinating future developments, but a discussion in fair detail of two or three examples of decorative lighting pointing out the principles which guide architects in their use of light would have given the treatment of this subject a more concrete turn. The book concludes with an excellent chapter on lighting for safety, health and welfare (H. C. Weston). Evidence from statistics and from special experiments is assembled to show how much is to be gained by adequate lighting; the conclusions drawn have been carefully framed and are commendably moderate.

The production and editing of the book leave nothing to be desired. A good index is included.

W. S. S.

The Kinetic Theory of Gases. By LEONARD B. LOEB. Second Edition. [Pp. xx + 687, with 85 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 36*s*. net.)

THE first edition of this excellent and well-known book earned for its author the gratitude of many who drew upon the information it provided in so practical and convenient a form. Much effort must have gone into the preparation of this new edition, and the result will certainly be widely appreciated. In the seven years since the first edition appeared quantum mechanics has altered the treatment of molecular problems, and considerable changes have been made in the book in places where the discussion concerns the internal properties of molecules and specific heats. On the whole, however, the book remains essentially a text-book on the kinetic theory of gases from the classical standpoint, and the additions are from the point of view of quantum theory rather than quantum mechanics. The sections on Debye's dielectric theory and on equations of state have been considerably extended, and will be found useful by advanced students. The outstanding characteristic of the book is, as much as in the first edition, the attention paid to experimental verification of the predictions of the kinetic theory. An unusual feature is the prominence given to the conduction of electricity in gases, to which nearly 100 pages are devoted; the author's special knowledge of this field makes this a welcome extension of material. A very useful Table of Fundamental Physical Constants has been included among the appendices.

The book is clearly printed, and relatively free from printer's errors in the text and formulæ. (The term Jacobean transformation which appears in one or two places seems unusual; there are errors of wording or misprints in symbols on pages 98, 471, 481 and 482; on page 305 the angle of the water molecule is stated to be 64° instead of 105°.)

These are certainly difficult days for a comprehensive book on the kinetic theory of gases, and it is hard to avoid speculating whether this one will not be the last to be written predominantly from the classical point of view; be this as it may, the present volume can be very heartily recommended.

B. T.

Elementary Quantum Mechanics. By R. W. Gurney, M.A., Ph.D. [Pp. vi + 160, with 67 figures.] (Cambridge: at the University Press, 1934. 8s. 6d. net.)

The most distinctive feature of this book is the treatment of the subject by graphical methods, with special consideration for the outlook of the experimental physicist. After an introduction to the subject the wave equation and the uncertainty principle are treated, with applications to simple problems. The properties of molecules and problems of valency are specially considered, and there is a discussion of crystals, insulators and conductors, and perturbation theory, with a minimum of mathematical symbolism. The book is, in a sense, supplementary to Mott's *Outline of Wave Mechanics*: it scarcely touches, for instance, the question of electron diffraction. The book is clearly and accurately written, and should serve a very useful purpose. It is not a mere collection of material paraphrased from other books on quantum mechanics, as most elementary books on hackneyed subjects tend to be. It may be specially recommended to the experimentalist either as a sufficient treatment of a certain aspect of the subject or as a very good introduction to more comprehensive and detailed work. We regret that the author uses the moribund "Ångström Unit" instead of "angstrom," thus incurring additional cost in printing, inviting sarcastic references to the "Volta Unit" of E.M.F. and betraying lack of acquaintance with modern spectroscopic literature. This, however, is a small blot from the reader's point of view: we mention it because it is so easily corrected.

H. D.

The Mysteries of the Atom. By H. A. Wilson, M.A., M.Sc., F.R.S. [Pp. x + 146, with 40 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1934. 10s. 6d. net.)

The purpose of this book is to give a plain account of the rise of the new ideas and the new facts in physics, without technical details. There are, however, several appendices which are slightly more technical than the main body of the text. Most of the book, naturally, is concerned with microscopic physics, but a little space is reserved at the end for a brief account of relativity. The book is well written and well produced, but, considering the inherent difficulty of the subjects, the treatment appears too brief for the understanding of the persons for whom, by implication, it is intended. Perhaps the title provides the reason for this, for certainly the reader will be left with the feeling that physics is in a nice mess, and no hope of escape is offered except some hypothetical "reality" which, it seems, will be exceedingly difficult to grasp. The wisdom of producing such books

as this seems very doubtful. Sentence by sentence the text could probably be justified, but the general impression is far too vague. In particular, the references to human free will—which, from its prominence in the Preface and the Conclusion, one must assume to be regarded by the author as an important aspect of modern physics—are very unsatisfactory. It is certainly not true that the later developments of Newtonian mechanics "led to determinism in philosophy": it was there long before. And when we read that now there is a possibility of free will without any violation of natural law, and that the only answer to the question "What decides what will happen?" is that we do not know unless the brain is controlled by spiritual forces or qualities, not usually included in the physicist's scheme—we are inclined to ask when the physicist's scheme ever contained any forces or qualities which would "control" even the motion of a fly. But these ideas are very popular just now, and perhaps someone will feel better for them: only they come strangely from an intelligent physicist.

H. D.

Electron Emission and Adsorption Phenomena. By J. H. de Boer. Translated from the manuscript by Mrs. H. E. Teves-Acly. The Cambridge Series of Physical Chemistry. [Pp. xi + 398, with 150 figures.] (Cambridge: at the University Press, 1935. 21*s*. net.)

This book covers a wide range of subjects, all of which have advanced rapidly in recent years and to most of which the author has made notable contributions. It opens with a concise but balanced account of the various kinds of electron emission from metals. This is followed by a chapter on the nature of adsorption forces. The thermionic, photoelectric and other properties of surfaces of adsorbed electropositive metals and gases on various metals are then dealt with. The next chapters describe the absorption of light by matter in the gaseous and adsorbed states and the selective photoelectric effect. This is followed by an account of the photoelectric properties, electronic conduction and related phenomena in the lattices of alkali halides containing adsorbed alkali metal atoms and in other lattices. The concluding chapters deal with photoelectric cathodes with thick intermediate layers consisting of a dielectric and metal particles, thermionic emission of cathodes with a dielectric, and oxide-coated cathodes, and the emission of electrons into intermediate layers of dielectrics and into blocking layers.

Many readers will probably have difficulties with the author's treatment of the *selective* photoelectric effect. He appears to restrict this term to the violently selective effects caused by the presence of adsorbed substances. In the literature it is generally used in a wider sense as embracing any photoelectric effect which exhibits a maximum in the yield frequency curve. As the photoelectric effect from uncontaminated metals has also been found to have the property in the cases where it has been sufficiently investigated the nomenclature in general usage is clearly an unfortunate one. It may be that it would be better to make the term selective define the nature of the surface rather than that of the photoelectric effect which is virtually what the author does, but in any event the situation calls for a fuller explanation.

In general the subject-matter is presented in a very attractive manner. It is a very valuable compilation of a mass of interrelated information which has hitherto been scattered over a wide literature and consequently rather

inaccessible. It will be very welcome to a wide circle of scientists, particularly physicists, chemists and engineers, and the connected account which it contains of the author's own views will no doubt be much appreciated by his co-workers in the same fields.

O. W. R.

X-Rays in Theory and Experiment. By ARTHUR H. COMPTON, Ph.D., Sc.D., LL.D., and SAMUEL K. ALLISON, Ph.D. (Pp. xiv + 828, with 281 figures.] (London: Macmillan & Co., Ltd., 1935. 31*s*. 6*d*. net.)

THIS book has been planned to serve as a second edition of Prof. A. H. Compton's well-known work, *X-rays and Electrons*, and a comparison of the two books gives a vivid idea of the progress of physics in the domain with which they deal during the past ten years. The first book was published in 1926, just too early to be influenced by the new outlook on atomic physics which we owe to the newer quantum theory, and, like all writings of the period, it shows traces of the conflict of thought due to the apparently complete contradiction between the older quantum theory and the classical theory. A great mass of experimental facts had shown that the methods of classical wave-optics gave a good and even a fairly quantitative account of many of the phenomena of X-rays; but there were others, for example the incoherent scattering of radiation, then only recently discovered by Prof. Compton himself, which seemed imperatively to demand a quantum explanation, and which could not be brought into any classical scheme. The unification of the two points of view into one theory which, if it does not *explain* in any mechanistic sense, at least tells us when we are to use one and when the other, is apparent in the present volume. Prof. Compton has enlisted the services of an able collaborator, Prof. S. K. Allison of Chicago, who has, according to the preface, been responsible for the greater part of the writing, and the authors are to be congratulated on a book which cannot fail to be of the greatest service to all workers on X-rays. Many of the topics dealt with receive for the first time an adequate treatment within the compass of a single text-book. There are, for example, excellent sections on the scattering of X-rays by gases, liquids and solids, the dispersion of X-rays, which is treated at considerable length, and on absorption. There is an interesting critical discussion of the value of e, with which the absolute values of wave-lengths as determined by crystal gratings are so closely bound up. The authors do not, however, come to any definite conclusion concerning the conflicting values of wave-lengths given by crystal measurements, and by measurements using ruled gratings.

One cannot help being struck by the large amount of space that it has been found necessary to devote, even in a work like the present one, which deals with the general subject of X-rays, to the diffraction of X-rays by crystals. Chapters V and VI are concerned with this subject alone, and it appears directly or indirectly in much of the rest of the book, which contains, for example, a long section on the use of the double-crystal X-ray spectrometer, with the development of which Prof. Allison himself has had much to do.

The exposition throughout is clear, and has a freshness to be expected from writers who have themselves contributed largely to the subjects discussed, although, in a work of such compass, it is not to be expected that the individual reader will at all points agree that the treatment is the best possible.

There seems at times a slight lack of co-ordination between different sections. For example, the method of radial Fourier analysis is, in effect, introduced twice, quite independently, once in discussing the scattering of radiation by gases, and once in discussing scattering by crystals; and in neither case, or so it seems to the reviewer, is quite enough emphasis given to the very dangerous nature of the radial Fourier series, which is especially liable to give false detail, and which requires the use of extrapolated scattering curves. In comparison with the general excellence of the book, such points are, however, small ones, and the authors deserve the thanks of all workers in the subject.

R. W. JAMES.

Fine Structure in Line Spectra and Nuclear Spin. By S. TOLANSKY, Ph.D., D.I.C., A.Inst.P. [Pp. viii + 112, with 24 figures.] (London: Methuen & Co., Ltd., 1935. 3*s*. net.)

THIS is one of Messrs. Methuen's well-known series of small monographs on physical subjects. It stands somewhat apart from most of the others, which are suitable for use by honours degree students specialising in physics, for it assumes a degree of familiarity with the notation and results of atomic spectroscopy which few such students are likely to possess. However, it is admirably suited to the requirements of the post-graduate student, particularly, of course, if he is undertaking spectroscopic research, of whatever character. The subject is both new and rapidly advancing, and there was a real need for such a general survey of its principles and present position. The volume under notice meets the need very satisfactorily, especially considering its modest size and price. The former limitation would have been less severe had it not been necessary to devote nearly the first third of the book to an account of multiplet structure, thus seriously curtailing the space available for the discussion of fine structure proper. This is probably justifiable, since there is no such account elsewhere in this series, and there is no doubt that many readers will find it extremely useful. The manner in which nuclear spin gives rise to fine structure is then explained, for one-electron spectra in the first instance and subsequently, using the vector method, for many-electron spectra. The next two chapters deal with the analysis of fine structure patterns; the essential facts concerning intensities and term intervals are clearly presented, and the Fisher-Goudsmit graphical method is explained. The subject of isotope effects is then taken up in some detail, and is followed by a chapter on perturbations. These two chapters are particularly valuable, since both topics, although of considerable importance, have been rather inadequately treated in previous works. The table of nuclear spins, which forms the substance of Chapter X, is a very useful compilation, especially in view of the fact that by no means all of the published values are reliable, and the author has exercised a judicious discrimination which would be difficult for anyone without considerable experience of work in this field. Chapters XI and XII are devoted to nuclear magnetic moments and theories of nuclear spin. The former might well have been expanded a little at the expense of the latter.

The earliest opportunity should be taken of correcting a few grammatical and other slips which are to be found here and there. For example (p. 18), "In the two components of a doublet term that with the smaller j lies deepest, and such a spectrum is called a normal spectrum." Again (p. 96), "Mole-

cular hydrogen exists in two states, para and ortho, in one of which the electron spins oppose and in the other assist." The column labelled "spin" in Table XII contains no entries, and on p. 49 it does not appear necessary to invoke the properties of vectors and then spend three lines in proving an obvious identity.

Apart from such minor blemishes the book is well up to the general standard of the series, to which it forms an important addition.

W. E. C.

Introduction to Electric Transients. By E. B. KURTZ, Ph.D., and G. F. CORCORAN, M.S. [Pp. xvi + 335, with 194 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 22*s*. 6*d*. net.)

THE subject of this book is one that does not readily lend itself to simple treatment, and the authors are to be congratulated on having given an excellent account of the basic principles involved without going beyond the mathematical attainment of the ordinary degree student. The presentation is logical and systematic, each type of transient being considered in turn under the headings (i) physical considerations, (ii) mathematical analysis, and (iii) oscillographic verification. The mathematical investigations are preceded by an effort to establish in the reader's mind a physical conception of the problem under discussion and followed by a series of most instructive oscillograms to demonstrate the truth of the analysis.

Although the treatment is for the most part of a general nature it is designed particularly for application to comparatively low frequency systems. Circuits in which a large proportion of the energy is dissipated by radiation are not specifically included.

The book is divided into two sections. Part I, which deals with what are called direct current transients, is concerned with circuits energised by steady potential differences, whilst in Part II the corresponding conditions produced by alternating potential differences are discussed. In both cases the simple inductance circuit is considered first. This is followed by a similar investigation of the simple capacitance circuit and thence the authors proceed to complex series networks of various kinds. Special attention is given to the boundary conditions and the changes accompanying a sudden alteration in the circuit parameters. Networks of series—parallel arrangement and others incorporating mutual inductance—are treated in much the same way. Simple differential equations suffice in practically all cases to express the relationships between the quantities of interest and the solutions to these equations are easily derived. For those who have any difficulty in following the mathematics a very useful appendix is provided. Herein the differential equations of the elementary circuit theory are discussed at some length. The more ambitious student will also find some valuable hints on the application of Heaviside's operational calculus, the Graeffe method of solving algebraic equations and on the use of exponential and hyperbolic functions.

The book, incorporating as it does a large number of practical numerical examples and oscillographic records, will undoubtedly appeal to all students of engineering interested in electric transients. They will find the information which they seek readily accessible and comparatively easily assimilated.

H. M. BARLOW.

Electrical Water Heating. With Special Reference to the Domestic Storage Heater. By D. J. Bolton, M.Sc., M.I.E.E., Philip C. Honey and N. S. Richardson. [Pp. viii + 192, with 66 figures.] (London: Chapman & Hall, Ltd., 1935. 7*s*. 6*d*. net.)

A constant supply of hot water is an item of interest to every householder, and it is not surprising to find that many who are inflicted with antiquated and inefficient heaters are now contemplating a change to the simplicity and cleanliness of electricity. For such, the volume under review will provide much valuable information; it will, moreover, materially assist the contractor who deals with the installation, and it even throws out hints to electric supply corporations on the desirability of reducing their charges.

The first chapter deals with heating systems in general, and it is unfortunate that the authors, in their enthusiasm for electricity, adopt a line of argument which is fundamentally unsound. This is the comparison of an up-to-date, well-installed and carefully used electric system with existing gas and solid fuel systems which are unfortunately only too frequently defective. Few manufacturers of solid fuel heaters would agree, as stated on p. 6, that the minimum rate of burning may be only one-half the maximum, while the comparison, from the point of view of atmospheric pollution, of electricity with a gas heater with no flue is manifestly unfair. On the ground that calculations of cost may be misleading, none at all is given: a vague statement that electricity at $\frac{3}{4}d.$ to $\frac{1}{4}d.$ is likely to be competitive with gas and coke is not sufficient for a householder anxious to improve his service.

Owing to price-cutting, it is undoubtedly true that many solid fuel water-heaters leave much to be desired, but there are signs that manufacturers are now making rapid development and, within a year or two, really efficient self-regulating heaters will be available. If these are connected to well-designed pipe and storage systems, a possibility apparently not contemplated by the authors, the cost factor is bound to be greatly in favour of solid fuel. In a future edition, the authors would do well to face this fact squarely and to set off against the increased cost the undoubted advantages of electricity in other directions.

The rest of the book abounds with miscellaneous information clearly set out and well illustrated with numerous diagrams. Much of the subject matter refers to hot-water systems in general and is worthy of study by all suppliers and users of this valuable commodity.

H. E. Watson.

Earthquakes and Mountains. By Harold Jeffreys, M.A., D.Sc., F.R.S. [Pp. x + 183, with 6 plates and 9 maps and diagrams.] (London: Methuen & Co., Ltd., 1935. 7*s*. 6*d*. net.)

In this book Dr. Jeffreys covers a wider field than the title would lead one to suppose, for he includes chapters on such subjects as radio-activity and the earth's history, the bodily tide and tidal friction. By so doing, however, he presents a much more interesting account of earthquakes and mountains than would otherwise be possible—an account which gives the reader a very clear picture showing how these phenomena are related to the physical properties of the earth. The book is, to a large extent, a simplified version of his well-known treatise *The Earth*, but it includes much information about work which has been done since the publication of the larger book.

There is, for example, an account of recent observations of gravity anomalies, and their bearing on the theory of isostasy, and a very good discussion of the newer theories of the origin of the continents, but Wegener's hypothesis of continental drift is dismissed in very few words. The application of knowledge gained in other branches of scientific work to the study of the earth should interest many people who are not actively engaged in geophysics or geology; we learn, for example, how the results of the X-ray analysis of crystal structure have been applied in an attempt to explain the concentration of the radio-active elements in the upper layers of the earth. Although the aim of the book is to provide an account of the physics of the earth, much attention has been given by the author to the geological aspects of the subject. The arguments given in the book are set out in general terms so that they can be easily followed by non-mathematical readers. The book is highly recommended to all who are interested in the constitution and history of our planet.

F. J. S.

Modern Surveying for Civil Engineers. By Harold Frank Birchal, O.B.E., D.F.C. [Pp. xii + 524, with 14 folding plates and 382 figures, including 12 plates.] (London: Chapman & Hall, Ltd., 1935. 25*s*. net.)

The author, in his preface and introductory remarks, states, as his opinion, that much of the advanced theory of surveying as taught in our Universities and other Colleges, is of little practical use to a Civil Engineer.

His aim has been, therefore, to present only that information on surveying which is likely to be of direct use; and the higher work involved in geodetic surveying, and in the taking of astronomical observations, has been intentionally omitted.

It appears to the reviewer that much of the fundamental theory is not dealt with as fully as is desirable for a student and that probably the book will prove of most service to the young engineer who has had a preliminary grounding in theory, and a certain amount of practical experience.

To such a reader, the treatise could not fail to be of real value; his outlook would most certainly be widened, and he would appreciate, in proper perspective, the part played by the surveyor in engineering projects.

The author is evidently an engineer first; and the work of the surveyor is judged, in this treatise, quite rightly, by the manner in which it assists in the execution of the engineering works. Emphasis is laid on questions of costs, time, and the provision of just the data required: modern instruments, including many of those of a specialised character, are appreciated and described.

The publishers are to be complimented upon the production of the book: it is well printed, and many of the illustrations are executed upon a surfaced paper which renders the details very clearly.

A feature of the book is the inclusion of a number of folding plates, illustrative of the part played by the surveyor in the preparation of initial schemes, or in the setting out of engineering works, most of the examples being from schemes carried out in Africa.

These plates; descriptions of survey or setting-out operations on actual

schemes; extracts from reports and field notes; and accounts of field organisation, render the book so valuable that few practising engineers could fail to learn from it.

The reviewer found the book most readable and interesting, and he much appreciated it.

W. N. T.

Practical Solution of Torsional Vibration Problems. By W. Ker Wilson, M.Sc. [Pp. xviii + 438, with frontispiece and 106 figures.] (London: Chapman & Hall, Ltd., 1935. 25*s*. net.)

This is outstandingly a book for practical men in the higher—not the ordinary—sense of the term: for men who appreciate the services that theory can render in practical life and are prepared to undertake the hard work involved in its application. It is an excellent work for the attention of serious students of mechanical engineering, and all the more so because the problem of torsional vibration stands directly in front of further technical progress and admits of reliable theoretical and experimental investigation illustrating many branches of Mechanics.

A brief but sufficient first chapter outlines the main features of the problem and explains how, for convenience of treatment, the complexities of actual engines, shafts and loads are replaced by simpler "equivalent" systems. It is in this early stage of the problem that many other treatments have fallen short of their objectives: the systems adopted as "equivalent" have been either too simple to be reliable or too complex to admit of full analysis. The author adopts and gives a very lucid account of methods that should meet almost any practical case and are as simple and effective as appear to be possible. Almost identical methods, only very slightly more complex, have been used during a number of years by the present reviewer; and in the light of this experience it is believed that the author's methods of calculating natural frequencies are reliable and, probably, preferable. It seems unfortunate, however, that the author follows current practice in plotting only amplitude of vibration along the lengths of the shafts in question: much may be gained by plotting also torque or, alternatively, the stress induced by the vibration.

The stresses induced by non-resonant and by resonant vibrations respectively are studied separately in the fourth and fifth chapters; and this is a wise arrangement as the stresses at non-resonant speeds are in some respects more important and in all respects more reliably calculable. At resonant speeds, the stresses are governed by damping—which offers a group of problems of great difficulty. A later chapter gives a valuable account of different artificial methods of damping.

Chapter 6 deals with the measurement of torsional vibration and with the analysis of measurements; and an appendix gives detailed assistance in harmonic analysis. Chapter 8 is devoted to problems that arise when engines are coupled to electrical machinery.

The whole volume is eminently practical in its outlook, and it would be hard to write an exposition of the fundamental theory clearer than that provided in the solution of practical problems covering the wide field of experience.

B. P. H.

CHEMISTRY

Inorganic and Theoretical Chemistry. By F. Sherwood Taylor, Ph.D., M.A., B.Sc. Third edition. [Pp. xiv + 832, with 19 plates, 200 figures and 2 maps.] (London: William Heinemann, Ltd., 1935. 12*s*. 6*d*.)

This is the third edition of Dr. Sherwood Taylor's text-book which was first published in 1931. The book contains two concurrent courses of reading: the matter in larger type is suitable for the first year or eighteen months study after Matriculation; the matter in smaller type, which is supplementary to this, will carry the student through his second year at the University. The first eight chapters of the book deal with fundamental chemical principles and such physical chemistry as the student requires for an inorganic course, the remaining eighteen chapters are arranged in accordance with the Periodic Table. The author has, within the limits imposed by his syllabus, produced a very complete, readable and up-to-date text-book, and the 8vo pages permit of a pleasing presentation of the text, the only adverse criticism being that electronic formulæ suffer somewhat in clarity when included in the smaller type. Very few misprints occur; the formulæ BiOCl (p. 239), H_2SiF_6 (p. 463), SnH_4 (p. 468) need correcting; the colours of the manganese bead in the oxidising and reducing flame (p. 380) ought to be reversed, and the cerium atom (At. No. 58) contains 59 electrons (p. 396). The choice of text-books on inorganic chemistry is now very wide, but the student requiring a good intermediate book will not regret selecting Sherwood Taylor. The binding is very good and the price moderate.

J. N. S.

The Fundamentals of Chemical Thermodynamics. Part I: Elementary Theory and Electrochemistry. By J. A. V. Butler, D.Sc. Second edition. [Pp. xv + 253, with 65 figures.] (London: Macmillan & Co., Ltd., 1935. 7*s*. 6*d*.)

The new edition [1] of this well-known text-book will arouse much interest and without doubt will increase its popularity. For whilst the elementary thermodynamical framework remains essentially unaltered, the chapters have been revised and thoughtfully re-arranged to a greater or less extent, and *all* the additional material is important. There is something here for everyone. The inorganic chemist is treated to an exceptionally lucid and up-to-date account of most of the vital aspects of electrochemistry which the intelligent study of his subject, not to mention research, nowadays demands, and which the average text-book of physical chemistry fails to supply. The new chapter on Oxidation Potentials (a relatively insignificant section in the first edition) will appeal to the student of biology, though doubtless he will continue to use the more accurate term "Oxidation-Reduction Potentials" to which he is accustomed and for the popularising of which biologists are largely responsible. The significance of the quantity E_h, which has attained a status equal to that of *p*H in the life-sciences, is clearly explained, together with the methods of measurement. The glass

[1] The first edition was reviewed for Science Progress (1929, **24**, 150) by Dr. R. K. Schofield.

electrode and the use of oxidation-reduction indicators are given special and well-deserved prominence. Physical chemists who wish to become acquainted with modern applications of electron and atomic physics to chemistry can whet their appetites on the theory of "overvoltage" and the electrolytic separation of the isotopes of hydrogen. An excellent innovation is the inclusion at the ends of chapters of short but carefully selected bibliographies.

It will be apparent from the foregoing remarks that the second edition is quite as important an account of Electrochemistry as of Thermodynamics. This ought to be indicated by extending the printed title, for there are plenty of sound thermodynamical texts but practically none on electrochemistry which make any attempt to explain to undergraduate students the simple underlying physics. No one is in a better position to do this than the author, who has made many notable contributions to the subject.

C. H. J.

Optical Rotatory Power. By T. MARTIN LOWRY, C.B.E., F.R.S. [Pp. xiv + 483, with 186 figures.] (London, New York, Toronto: Longmans, Green & Co., 1935. 30*s*. net.)

OVER a period of nearly forty years the writer of this book has conducted investigations on the measurement of the optical rotatory power and the optical rotatory dispersion of a wide variety of compounds. His early insistence on the importance of measuring rotatory power with light of several different wavelengths has been amply justified by the wealth of new information obtained and by the insight on molecular structure to which critical consideration of the new data has led.

It is particularly appropriate, therefore, that Professor Lowry should be the author of this well documented and masterly presentation of the involved subject of optical rotatory power in which the reader is given the benefit of his wide experience and extensive reading.

That a widespread interest is taken in the subject was shown by the fruitful discussion held by the Faraday Society in 1930, and thanks are due to Professor Lowry for filling a notable gap in chemical literature.

The book is divided into four sections dealing respectively with (i) the pioneer researches of Biot, Fresnel and Pasteur, and their development by later investigators; (ii) the development of polarimetric apparatus and procedure; (iii) the application of polarimetric methods to the study of quartz and numerous typical organic compounds; and (iv) various theoretical considerations and modern ideas.

In the last section the author gives a clear account of the theories of Gray and de Malleman, together with the more recent work of Boys and his method of calculating the rotatory dispersion of simple molecules from a knowledge of the refractivities and effective radii of the chemical groups involved. The electronic theory of optical rotatory power is considered fully with a detailed consideration of Born's molecular model and its applications.

A concise account of optically active absorption bands and of rotatory dispersion in transparent and absorbing media is given. The applications of the equations of Drude, Kuhn and Braun, and Lowry and Hudson are dealt with in an able manner. It will thus be seen that the author continues his praiseworthy task of explaining to chemists the theories of mathematicians on the nature and origin of rotatory power and rotatory dispersion.

It is significant that no discussion of the Walden inversion is undertaken; it is becoming increasingly recognised that this subject properly constitutes a branch of the study of substitution reactions in organic compounds.

The book is excellent value for its rather high price. J. Kenyon.

An Introduction to the Modern Theory of Valency. By J. C. Speakman, M.Sc., Ph.D. [Pp. vii + 157.] (London: Edward Arnold & Co., 1935. 4*s*. 6*d*. net.)

In the author's words: "The electronic theory of valency is now so well established and its ramifications extend so widely into all branches of Chemistry, that it ought to form the background to any serious study of the subject. Unfortunately it is the experience of those who teach it that the theory has still not properly impregnated the text-books. In a number of books it receives some mention indeed, but the treatment is often such as might leave the impression that it is just an interesting, speculative hypothesis rather than a fundamental, workaday theory. . . . This book is to be regarded as a *supplement* to existing text-books. The primary aim has been to give students during the early part of their degree courses, and perhaps in the advanced forms at schools, the background necessary for their later, and more specialized studies." No one with experience in teaching the subject will differ from the author over the need for such an introduction as he has now given us. Dr. Speakman has produced a book which students will appreciate very much, at a price which makes it very good value indeed. The Hamlet of chemistry is the doctrine of valency, and our understanding of this central problem has been growing remarkably, but it is only too true that the deepening and the simplification of ideas which have come in recent years have yet to become, as surely they should, basic in every text-book.

To the reviewer the scope of this little book seems exactly right: The Data; The Principles; Electrovalency; Covalency; Dative Covalency; Polar and Non-Polar Compounds; the Relationship between Electrovalency and Covalency; The Application of Wave Mechanics to Valency Problems (this is a courageous effort in twelve pages, and is skilfully contrived); Valency in the Long Periods; Co-ordination and Hydration; Some Stereochemical Aspects of Valency; Index. These things are of paramount importance for chemistry. The account given in this book is straightforward and carefully reasoned, and has a pleasingly enthusiastic yet critical feeling. In one or two places there are statements which to the reviewer seem misleading or not quite correct. On p. 30 the proton is referred to as "being momentarily detached from a molecule without difficulty." On p. 84 the explanation given of chain-formation by carbon atoms seems to be no explanation. The statement on p. 67 that "there is no doubt that such phenomena as the adsorption layers studied by Langmuir are due to dative covalency" is too strong. On p. 72 it is stated that "covalent compounds do not usually undergo rapid reaction, except perhaps in a few cases where there is a large affinity"; this brief remark may give a rather misleading impression.

The book is small, well printed and bound, free from typographical errors, and inexpensive. It can be strongly commended to students of chemistry and to others who may be interested in the chemist's point of view about chemical valency. B. T.

Physical Chemistry for Students of Biology and Medicine. By D. I. Hitchcock, Ph.D. Second edition. [Pp. xi + 214, with 28 figures.] (Baltimore: Charles C. Thomas; London: Baillière, Tindall & Cox, 1934. 12*s*. 6*d*. net.)

The significance of physical chemistry for the correlation and interpretation of biological phenomena is everywhere recognised, and many text-books setting forth the relevant physico-chemical principles have been published—in the majority of cases by physical chemists. The author of the present volume is primarily a physiologist, but his exposition of physical chemistry is in general thoroughly sound and clear. The ideas and laws associated with osmosis, electrolyte equilibrium, adsorption, the colloidal state, chemical kinetics, hydrion concentration, catalysis, etc., are discussed and documented, and biological illustrations are given of their application.

Chapters of rather more specialised character deal with membrane equilibrium and equilibria in blood. In the latter field physical chemistry has been notably successful in explaining the mechanism of the respiratory function both as regards the reciprocal solubility effects of oxygen and carbon dioxide, and as regards the shift in the ionic equilibria accompanying the absorption of these gases. In the view of the author it is in explaining the mechanism of the respiratory function of the blood that physical chemistry has so far proved more useful to physiology.

There is, perhaps naturally, a definite bias towards animal physiology in the author's choice of his material, and the consideration given to the physico-chemical aspects of certain plant physiology problems is consequently less adequate than one might have wished. The phenomenon of plasmolysis is described, but the significance of permeability for the osmotic phenomena characteristic of plant cells receives all too brief consideration, and the word "permeability" does not even appear in the index.

In general, as already stated, the presentation is both sound and clear, but the reviewer is inclined to suggest in connection with some of the fundamental physico-chemical concepts such as reversibility, equivalent conductivity, and reaction velocity, that the discussion might, with advantage, be more extended. The author, for example, has made no use of kinetic ideas in introducing the subject of reaction equilibrium and velocity.

A new feature of the second edition is the inclusion of "Laboratory Directions," describing experiments which might profitably be carried out by the reader. The student interested in a particular topic is further assisted by the provision of a useful bibliography. It should be added that the volume has an attractive appearance and that due attention has been paid to its technical production.

J. C. Philip.

Thorpe's Dictionary of Applied Chemistry. Supplement: Vol. II, N–Z. By Jocelyn Field Thorpe, C.B.E., D.Sc., F.R.S., F.I.C., and M. A. Whiteley, O.B.E., D.Sc., F.I.C., assisted by eminent contributors. [Pp. xx + 727, with illustrations.] (London: Longmans, Green & Co., 1935. 60*s*. net.)

The second volume of this supplement follows closely on the lines of the first, but the authors have found that the amount of material has exceeded their estimate and they have been obliged to relegate the index and glossary to a subsequent volume to be published shortly. The common tendency

for large and expensive works to appear in a greater number of volumes than announced is to be deprecated, as private purchasers and libraries find themselves thereby committed to an expenditure greater than they have reason to anticipate.

Once again the names of the contributors are a guarantee of the excellence of the articles and special mention must be made of the comprehensive accounts of the stereochemistry of cyclic compounds by W. H. Mills and of heavy water by H. V. A. Briscoe, and also of the excellent articles on Vitamins by L. J. Harris and on the new ideas about Tautomerism by J. W. Baker; the latter is a treatise in itself running to over 37 pages.

A few omissions and errors have caught the reviewer's eye. On p. 27 no mention is made of Cremer and Duncan's work on nitrogen iodide; on p. 118 the formula for the oximino group is wrong; on p. 130 exaltone is not mentioned among the synthetic musks; and on p. 598 reference is made to Gornall and Robinson's description of sodium 2:4-dintrotoluene-3-sulphonate: actually these authors describe a hydrate, the anhydrous salt and three isomerides were described by Brady, Hewetson and Klein.

In his review of the first volume the writer expressed surprise that some of the modern solvents, for example dioxan and various furfural derivatives, were not mentioned, but expressed the hope that they might appear under the heading Solvents in Vol. II. This hope has not been realised.

In a volume of this nature minor criticisms are inevitable, and those that have been made in no way detract from the great value of the work as a whole.

O. L. B.

The Chemistry of Cement and Concrete. By F. M. Lea, M.Sc., A.I.C., and C. H. Desch, D.Sc., Ph.D., F.I.C., F.R.S. [Pp. xii + 429, with 10 plates and 80 figures.] (London: Edward Arnold & Co., 1935. 25s. net.)

The twenty-four years which have passed since the publication of Professor Desch's valuable and well-known *Chemistry and Testing of Cement* have seen great progress in the application of scientific method to the study of the important branch of the craft of building. In the same period, too, changes of technique both in manufacture and in application of materials have brought many new problems of interpretation and control. It is therefore not surprising that Professor Desch has felt that revision of his early work was not sufficient, and has turned rather to the composition of an almost entirely new treatise, in which he has been fortunate to secure the collaboration of Mr. Lea of the Building Research Station, who has himself contributed so fundamentally by his study of the lime-alumina-silica-ferric oxide system to our present understanding of the constitution and behaviour of cements.

The chapters on the components of cement and their inter-relations in the four component system naturally contain much that is new and of outstanding value to the technician. Four-component systems are far from easy to study or to describe; but the authors' ingenious choice of components and the many well-thought-out diagrams enable the student to appreciate the transformations which occur in clinker formation. The authors move easily and confidently in their field, and their exposition is always clear. They do not ignore the outstanding problems which only further research can solve—the influence of minor constituents, for instance, and the debatable

question as to how far a somewhat rapidly chilled system like cement clinker can truly be represented by an equilibrium system.

The remainder of the book contains a clear account of present knowledge of the behaviour of cement in the laboratory and in practice. It deals thoroughly with the properties of cement and discusses judiciously the problems of testing. There are also chapters on pozzolanas, slag cements, aluminous and other special cements, and concluding sections on concrete and its behaviour. Much in these careful and well-balanced chapters, and indeed in the whole book, is valuable as indicating the trends in research and manufacture which are of present-day importance.

Cement research is evidently now moving rapidly, and no doubt this treatise will have to be reproduced at frequent intervals with modification and extension. It is, however, an excellent presentation of the state of knowledge to-day, and will be widely appreciated.

M. P. Applebey.

The Principles of Motor Fuel Preparation and Application. Vol. II. By A. W. Nash, M.Sc., and D. A. Howes, B.Sc., Ph.D. [Pp. xiv + 523, with 138 figures, including 16 plates.] (London: Chapman & Hall, Ltd., 1935. 30s. net.)

The first volume of this work, when published in 1934, was received with such a chorus of well-merited congratulation that one's hopes that the standard would be maintained in Volume II were tinged with some natural doubt. Volume I was devoted to methods of production of motor fuels, both natural and synthetic. Volume II deals with methods of analysis and testing, and with the significance and relative importance of these analytical data as a means of assessing the behaviour of the fuel in the engine. It may be said at once that the high standard of the first volume has been very well if not entirely sustained by the second volume, which occupies about 500 pages, comprising nine chapters and an appendix of tabular matter.

The first and inevitably most lengthy chapter deals with many of the less important methods of determination relating to colour, sulphur (including the separate determination of elemental, mercaptan, disulphide, thiophen and other forms of sulphur), individual classes of hydrocarbons, water, alcohol and lead ethyl. It is regrettable, though of course due to no fault of the authors, that the methods they quote as I.P.T. or A.S.T.M. Standard Methods in certain cases have since been modified or abandoned. An interesting review is given of American research on the effects of sulphur on the automobile engine.

Since the really essential properties of a motor fuel are those depending on volatility, stability and anti-knock quality, the greater part of the book deals in detail with the determination and significance of these criteria. Thus the section dealing with volatility describes the methods for carrying out the normal "Distillation Test" and also the determination of vapour pressure, equilibrium boiling point, and equilibrium air distillation curves. It then reviews the bearing of these results on carburation, ease of starting, acceleration, vapour lock, crankcase dilution and exhaust gas.

The section on knock-rating similarly reviews the development of this subject from the pioneer work of Ricardo to the Uniontown tests which confirmed the adoption of the C.F.R. Motor Method, while the chapter on

gum-stability and inhibitors gives a valuable summary of recent research work on this very important subject.

Two very up-to-date sections which deserve careful study deal respectively with aviation fuels and their current specifications in various countries, and with high-speed diesel engines and their fuels. This latter chapter in particular stresses the fact, still inadequately recognised, that high-speed diesel engines are even more fastidious than petrol engines as regards the superfine quality of their diet.

One word of warning is necessary as regards numerous "typical analyses" that are quoted. These apply (the fact being clearly stated) to 1932 products, which have now, of course, entirely disappeared from the market, so that much of Chapter XVII must be disregarded except as a record of ancient history. Some of the tables in the Appendix dealing with petroleum production and imports and consumption do not go beyond 1931, and therefore need extension to be of more than historical value. These, however, are minor blemishes on a production of outstanding merit.

F. B. THOLE.

Shellac: Its Production, Manufacture, Chemistry, Analysis, Commerce and Uses. By ERNEST J. PARRY, B.Sc. (Lond.), F.I.C., F.C.S. [Pp. xii + 240, with frontispiece and 15 figures.] (London: Sir Isaac Pitman & Sons, Ltd., 1935. 12*s*. 6*d*. net.)

THE lac industry has been the subject of extensive study during recent years. Chemists, physicists, technologists, economists and politicians have all contributed to the subject with the object of placing this old-established native industry upon a firm foundation of scientific knowledge which will enable it to maintain its place in the modern world. The author of the present volume has been for many years intimately connected with the shellac trade in London as the analyst for the London Shellac Trade Association and is therefore particularly well qualified to deal with these matters which concern the merchants and users in this country.

In the first three chapters an account is given of the origin and production of lac and its conversion to shellac and other forms. The fourth chapter deals with the chemical and physical examination of shellac and should be read in conjunction with Mr. Parry's original papers reprinted here from the *Chemist and Druggist* as Appendix I, and the standards and methods of analysis in force in the United States, Appendix II. In the fifth chapter an account of the constitution of lac and the bearing of recent researches upon the problem of the formation of lac resin by the insect is given. In the sixth and last chapter a very useful account is given of the commercial aspects of the industry and in particular the rules and customs governing the London Trade. Altogether this is an original and valuable addition to the literature of the subject. To those who are familiar with the subject it will be welcome as a definite and personal contribution by one who has had unique opportunities for studying certain aspects of the shellac trade concerning which authoritative information is difficult to obtain, whilst to those who are desirous of informing themselves concerning any aspect of this extremely complex subject the book may be confidently recommended as a concise and reliable guide.

T. H. B.

The Application of Absorption Spectra to the Study of Vitamins and Hormones. By R. A. MORTON, D.Sc., Ph.D., F.I.C. [Pp. 70, with 25 figures and 6 plates.] (London: Adam Hilger, Ltd., 1935. 10s. net.)

THE vitamins and hormones exert their potent effects when present in minute traces in the living organism and the most delicate analytical methods are required for their detection and study. One of the most important of these methods is furnished by the measurement of absorption spectra, and convenient apparatus is now available by means of which it is possible under favourable circumstances to detect the merest fraction of a milligram of the substance under investigation. Dr. Morton has played a prominent part in the development of these methods and it is specially appropriate that a summary of progress should come from his hands. He has brought together in convenient form a vast number of observations dealing with Vitamin D, the sex-hormones, Vitamin A, Vitamin B_1, Vitamin B_2, Vitamin C, and Vitamin E, for which the practical spectroscopist would otherwise have to search laboriously amongst a large number of separate papers. The reader's indebtedness to the author is much increased by the numerous references and by the short summaries of the present state of knowledge concerning the chemistry and the physiological activity of the various substances. Special reference may be made, for example, to Table III of this well-produced monograph in which the principal properties of the carotenoids and related substances are set out. Dr. Morton rightly stresses the empirical nature of these applications of absorption spectra and emphasises the importance of the animal test as the first and last court of appeal. This is particularly the case when natural products are under investigation, since the characteristic absorption spectra are frequently obscured and the position is further complicated by the facts that non-absorbing derivatives of the Vitamin may be fully active (as with dehydro-ascorbic acid) and that structurally related but physiologically inactive substances may display absorption spectra indistinguishable from those of the active materials. Nevertheless his book amply reveals the extreme power of the absorption spectra measurements both as an aid in structural investigations and as an invaluable and rapid method for the characterisation and estimation of biologically important substances.

E. L. H.

Van Nostrand's Chemical Annual. Edited by JOHN C. OLSEN, A.M., Ph.D., D.Sc. Seventh issue. [Pp. xvii + 1029, with frontispiece.] (U.S.A.: D. van Nostrand Company, Inc.; London: Chapman & Hall, Ltd., 1935. 25s. net.)

THOSE who have been in the habit of using this well-known reference book of chemical constants will welcome the appearance of the seventh issue. A thorough search of the literature has been made for new data and the policy of publishing only one critically chosen value in each case, instead of all the recorded values, has been continued. New additions are tables of boiling points, vapour tension and latent heat of evaporation for the use of those who design or operate chemical engineering processes, extended tables of physical and chemical properties of metals and other substances in common use, also tables for the calculation of thermometers and thermocouples, and of combustion data of gases. All molecular weights, conversion

factors, etc., have been recalculated in accordance with the 1933 table of atomic weights and the bibliography of new books has been replaced by a list of books published since 1925. Handbooks of this kind increase in value with the appearance of each new edition, both by the inclusion of new data and the correction of errors, and van Nostrand is now well established in both America and this country.

J. N. S.

A German-English Dictionary for Chemists. By A. M. PATTERSON, Ph.D. Second edition. [Pp. xx + 411.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 15s. net.)

OF the virtues of "Patterson" which have now been tested over a period of eighteen years, every English-speaking chemist is fully aware. Its reliability, comprehensiveness, simplicity of arrangement, clarity of print, convenience of size, and durability of binding have ensured its popularity. Moreover, it is something more than a mere dictionary: it is a real guide to the translation of German chemical literature and of the wide range of technical matter which is not pure chemistry but which the chemist may need to read.

In preparing this new edition the author has not been content with mere formal revision but has re-considered the selection of entries in relation to modern usage. A few entries have been deleted but many additional words and special meanings have been introduced, with the result that the number of pages has been increased from 316 to 411—and this in spite of the fact that much wastage of space has been avoided by the adoption of the paragraph style for entries beginning with the same word element. Terms relating to atomic structure and other rapidly developing fields are well represented, and there is no doubt that this new edition will prove of the greatest value to an increasing range of chemists and other scientific workers.

H. J. T. E.

The Teaching of Chemistry in the Universities of Aberdeen. By ALEXANDER FINDLAY. [Pp. viii + 92, with 18 plates.] (Aberdeen: The University Press, 1935. 5s. net.)

THE plural "Universities of Aberdeen" in the title of this work might puzzle a southerner, but it tells an Aberdonian at once that chemistry was taught in his city earlier than 1860. For until that date King's College (1495, 1505) and Marischal College (1593) kept up for more than two and a half centuries a pre-natal feud which had begun when a Protestant Church displaced the Roman, and they continued to find in academic discord a compensation for the lost joys of religious controversy. Fused together at length seventy-five years ago, the two institutions together have enabled the University of Aberdeen to stretch its influence widely beyond its native constituency, and to join voices with its sisters in the South.

The story, as Professor Findlay tells it, is of much interest to anyone concerned in university work, whether chemist, scholar, or administrator, and whether Aberdonian "an' twal' mile aroon'" or merely English. The stepwise emergence of a subject from superficial, ancillary treatment to the dignity of a self-maintained and intensive study is clearly illustrated, and with it is seen the corresponding growth of a university's purview from that

of cliques of parish-pump schemers to that of the body of outward-looking savants which its wiser heads and founders have always wished it to be.

In establishing the regular teaching of chemistry Marischal College led the way, in 1793, with the aid of a carefully-conceived bequest from the widow of a former Principal; and under her Will, George French, an Aberdonian with some London training, became the first Professor of Chemistry. At King's College there was never a chair of the subject: the first set class there in chemistry, about 1811, was held by the Professor of Moral Philosophy and then, until 1839, by the Professor of Humanity—either pursuit a very suitable concomitant to chemistry, no doubt, but not very far. With the advent to the chair of Medicine of William Gregory, Thomas Graham's successor at the Andersonian Institution in Glasgow and a pupil of Hope and of Liebig, King's gained a worthy teacher of the science; he was, however, translated to the Edinburgh chair in 1844, and his successor Fyfe carried on the work until the Union. Meanwhile, at Marischal College, French had held his chair until his death in 1833, but for fourteen years had resigned the work and the tuition fees to a self-proposed (and duly authorised) deputy. The manœuvring for the filling of French's chair recalls the election of Abbot Samson in Jocelyn of Brakelond, and shows feats of which human nature has even now not wholly lost the knack. But the new man was a good practical chemist, Thomas Clarke, whose water-softening process is still known. He came from Glasgow, where he had defeated Thomas Graham in candidature for a lectureship; Graham later made up for it by beating Clarke for the chair at University College, London. During Clarke's tenure, Marischal College erected its present building, and chemistry gained better quarters. There were about 33 medical students of chemistry and about 20 from the Arts Faculty; they had some practical instruction as well as lectures. From 1844 to 1860, Clarke being infirm, his work was done by four successive deputies. One of these, John Smith, went out to Sydney in 1852 as the first Professor of Chemistry in the Southern Hemisphere, and continued there until 1885; he is probably still remembered by some of the now eldest generation of Australian men of science whose influx he heralded.

From the Union onwards, chemistry has been taught only at Marischal College. Fyfe, from King's, was soon succeeded by Brazier (1862–1888), a Sussex man who had been an assistant to A. W. Hofmann in London; after him for two significant years came Carnelley from Dundee, and by his energies degrees in Science were instituted. At his death Japp, already an organic chemist of recognised eminence and with experience under Bunsen, Anschütz, Frankland, and T. E. Thorpe, came in 1890 to the chair which he held until 1914. In 1896 he replaced Clarke's exiguous laboratory by those that are still used. This allowed the school to grow and be modernised, so that the chemical attainment of a Bachelor of Science could and did become equal to that in other British universities; while the treatment of the science might at the same time keep a flavour of its long inclusion as an Arts subject, in ways which to this day encourage breadth as well as depth. Professor Findlay's tale—which stops short of the occupancies of the chair by Frederick Soddy and by himself—is told with proportion and humanity; and this, No. 112 of "Aberdeen University Studies" (admirably produced and illustrated by the University Press), would very usefully be circulated and studied among the academic authorities of Britain and the Dominions.

Irvine Masson.

Qualitative Chemical Analysis, Organic and Inorganic. By F. Mollwo Perkin, C.B.E., Ph.D., F.I.C., F.C.S. Fifth edition, revised by Julius Grant, Ph.D., M.Sc., F.I.C. [Pp. x + 377, with 30 illustrations and a spectrum plate.] (London, New York, Toronto: Longmans, Green & Co., 1935. 9*s*. net.)

"The whole book has been thoroughly revised and brought up to date, the elements of microchemistry are discussed, crystal tests and drop reactions are given. It is hoped that these additions have resulted in a modern text-book which will be of use to the student from the time he starts analytical work at school, through his university studies and in the many ramifications of science in which chemical analysis plays a part." So reads the preface.

Qualitative analysis is a valuable training. It should develop orderly methods of thought and work and it should be presented to the student in such a way that these habits are encouraged. The group separations should follow a logical plan and every effort should be made to lead the beginner through them without confusion. It is precisely here that "Mollwo Perkin" fails. Regarded as a handy reference book for the reactions of organic and inorganic substances one may truthfully say that it contains 300 pages of sound information but as a working text-book of qualitative inorganic chemistry it is confusing. Pages 216 and 219 ought to precede pages 215 and 217 respectively. A phosphate table is not given but a note (p. 219) refers the reader to p. 118, whence he is referred to p. 95 for further information. There is no settled plan for the internal arrangement of the groups. The treatment of the CoS, NiS mixture (p. 217) is not correlated with the same operation on p. 218 and the precipitate identified is not "nickel hydrate." An equation is misplaced (p. 224) and a substance (p. 204) which is insoluble in concentrated hydrochloric acid but which dissolves on dilution may possibly be $Sn_5O_5Cl_2(OH)_8$ or even potassium antimonate, but it is more probably barium chloride which is not mentioned. Many other similar examples might be quoted. If pp. 194–229 could be drastically overhauled and brought up to the level of the rest of the book, then the hopes expressed in the preface would be realised.

J. N. S.

GEOLOGY

Introduction to Geology. By E. B. Branson, Ph.D., and W. A. Tarr, Ph.D., Sc.D. [Pp. viii + 470, with 456 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 21*s*. net.)

Holding the correct view that not one in a hundred first-year students of geology will pass on to make geology his life-work, Professors Branson and Tarr have compiled this introductory text-book on broad and general lines with a minimum of technical phraseology, and with emphasis on principles and reasoning rather than on the presentation of a large and confusing mass of technical details. While they aim at mental training through the excellent medium of geological science, they have also kept in mind the necessity of providing a broad foundation for later professional studies.

So far as the matter is concerned we feel that the above aims have been realised, but it is in the manner and style that the book falls short. Awkward, confused and ungrammatical sentences such as, "They [batholiths] are formed by a magma's melting the rock in place and by its displacement of the rock," abound, and give the impression that sets of lecture notes have

been hastily thrown together to produce the book. However, if one disregards these blemishes of style, the matter is good and well-arranged. Above all, the illustrations are extremely numerous, about one to each page of text, and are remarkably fresh and informative. We believe that the book will be found useful by the type of student for which it is designed. The too familiar misprint "paleographic" (*sic*) for "palæogeographic" occurs in the Preface, and olivine is apparently not included in the class of ferromagnesian minerals (p. 47).

G. W. T.

Outline of Glacial Geology. By F. T. Thwaites. [Pp. ii + 115, with 90 figures.] (Ann Arbor, Michigan: Edwards Brothers, Inc., 1935, $2.75; London: Thomas Murby & Co., 12*s*. 6*d*.—subject to variation according to rate of exchange.)

This book is a photo-lithoprint of the author's manuscript, and the format of such a production leaves much to be desired.

In the first place it is only fair to point out that the title of the work is misleading, for the book deals entirely with the Pleistocene glacial phenomena of North America, and makes no attempt to discuss the problems of the glacial history of other parts or of the pre-Pleistocene glaciations.

The book is divided into four parts: Part I—Mountain Glaciers and Glaciation; Part II—Continental Glaciers and Glaciation; Part III—The Pleistocene Glacial Succession; Part IV—Miscellaneous, in which are included such topics as The Driftless Area, Causes of Pleistocene glaciation, Duration of the Quaternary, etc. Part IV is disappointing. The author has apparently been greatly attracted by Croll's hypothesis, and although no mention is made of Simpson's theory, the author states that "the idea of changes in amount of heat received from the sun is an attractive one . . ." (p. 109). We cannot follow him, however, in his conclusion that as far as the cause of the Ice Age is concerned "Continental elevation was unquestionably a prerequisite" (p. 109). Parts II and III are the most important in the book and they undoubtedly contain a wealth of detail on glacial deposits and their topography and on the Pleistocene glacial and interglacial stages of North America. In too many cases the author's views are carelessly expressed. To give only one example we may quote these statements: "Elevations in the bed rock surface are most abraded on the *stoss* side, that exposed to the oncoming ice. Depressions are smoothed out most on the *lee* side, that which was similarly exposed to the brunt of the ice" (p. 23). The author starts away with the excellent intention of underlining "all technical terms . . . where first used" (p. 1), but many are underlined more than once while others escape on their first appearance.

Adequate references to American literature are given throughout the book after each sub-section, but in the discussion of varves where, naturally, references are given to de Geer's work in Scandinavia and to Sauramo's in Finland, it would be interesting to mention that in *Clay and the Clay Industry of Ontario*, Prof. M. B. Baker pointed out, as long ago as 1906, that varves are seasonal deposits.

The field geologist and the teacher will find this Outline a mine of information on glacial deposits.

W. J. McC.

Outline of the Physiography and Geology of Victoria. Edited by Professor E. W. Skeats. [Pp. 76–135, with 2 figures and 1 map. Reprinted from the *Handbook for Victoria*, prepared for the Meeting of the Australian and New Zealand Association for the Advancement of Science, Melbourne, 1935.] (Melbourne University Press in association with Oxford University Press, 1935. 2*s*. net.)

This useful outline consists of a number of essays by different authors under the general editorship of Prof. E. W. Skeats of the University of Melbourne. Physiography, by E. S. Hills, occupies 10 pages, and deals, among other topics, with the interesting pre-basaltic topography of the state, of economic importance because of the exploitation of gold in "deep leads" along ancient stream courses buried beneath lava floods. Geology is treated according to formations, Prof. Skeats dealing with Pre-Cambrian, D. E. Thomas with Cambrian and Ordovician, F. Chapman and D. E. Thomas with Silurian, E. S. Hills with Devonian and Carboniferous, H. S. Summers with Permo-Carboniferous, F. A. Singleton with Triassic and Kainozoic, and Prof. Skeats with Jurassic. Palæozoic formations are exceedingly well developed in Victoria, and there was a vast outpouring of basaltic lava in the Kainozoic. Each section concludes with a good list of references. The map, contained in a pocket, with the geology in line and dot shading, covers South Central Victoria, *i.e.* the region of which Melbourne is the centre. This handy booklet represents an exceedingly valuable compilation which will be found useful in the regional study of geology far beyond the boundaries of the state.

G. W. T.

The Changing Sea Level. Four lectures given at the University of London in November 1933. By Henri Baulig. The Institute of British Geographers, Publication No. 3. [Pp. xii + 46, with 11 figures.] (London: George Philip & Son, Ltd., 1935. 3*s*. net.)

Apparent changes of sea-level may be due to various causes; real changes must be due either to variation in the amount of water in the ocean or to alterations in the capacity of the ocean-basins. It is with the geomorphological evidences for the latter that the author of this pamphlet is concerned.

During the Pleistocene period the growth and decay of ice-sheets led to considerable fluctuations of the volume of water in the oceans, and it is difficult to disentangle the effects of these fluctuations from the effects of alterations in the capacity of the ocean-basins. For this reason the author deals chiefly with the Pliocene period. It was near enough to our days to permit the use of geomorphological methods, and there is no reason to suppose that it saw any great variation in the amount of water in the ocean.

The belt of sedimentary rock west of the Rhone shows wide plateaux cut across the folds of the Miocene beds, and these plateaux must have been eroded since Miocene times. The author looks upon them as peneplains formed during the Pliocene period when the sea stood higher than it does now and extended up the valley of the Rhone. He recognises three such peneplains at different elevations, and places the corresponding sea-levels at 180, 280, and 380 meters. The retreat of the sea was not gradual, but there were long pauses at each of these levels, during which the corre-

sponding peneplain was formed. He finds similar surfaces at the same elevations elsewhere.

The author realises that much more evidence is needed to prove world-wide changes of sea-level; and in these lectures his chief concern is not to prove anything but to show how the problem may be approached.

P. L.

BOTANY AND FORESTRY

Gymnosperms. Structure and Evolution. By C. J. Chamberlain, Ph.D., Sc.D. [Pp. xii + 484, with 397 figures.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1935. 21*s.* net.)

The last edition of the *Morphology of Gymnosperms* by Coulter and Chamberlain was published in 1917. The present work follows similar lines to the earlier which it is intended to replace. But the results of the research during the eighteen years that have since elapsed have scarcely been embodied to the extent one might have anticipated. The chapter on the Cycadofilices is notably deficient in this respect and indeed cannot be said to fairly represent the present state of knowledge. In this section for instance no mention is made of the very important work of Halle published in 1933.

The text is admirably illustrated with numerous drawings and photographs. The section on the Cycadales is of especial interest as representing the mature views of the author's extended investigations in this group and is embellished with a number of Professor Chamberlain's beautiful photographs showing the habit of members of the various genera growing in their native homes.

There is an extensive single bibliography of over 700 references in place of the separate bibliographies of the earlier work.

E. J. S.

Management of American Forests. By Donald Maxwell Matthews, B.A., M.S.F. American Forestry Series. [Pp. xvi + 495, with 22 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 30*s.* net.)

Books on Forest Management are in the main based upon the record of experience gained on the Continent of Europe where the highly organised forests have yielded data over relatively long periods and sufficiently precise to be expressed in complex formulæ enabling the forester to work out with some degree of accuracy expected returns and profits. Their range of application is limited and the bulk of the world's forests including those of the United States and for that matter of this country lie outside their scope. Where exact data of yield are not available and where a first approximation is immediately necessary and is all that can be expected the forest assessor must approach his problems from first principles, applying the formulæ dictated by the conditions.

This book is not a text-book of Forest Management in the ordinary sense but of Forest Management in relation to the particular needs of America, where the bulk of the forests are irregular. In Part I, the introductory chapters effectively bridge the apparent gulf between the often

pigeon-holed subjects of silviculture and management and will be welcome to the more biologically minded. The enunciation of general principles, with, of course, the normal forest as the central theme, leads up to the Report on the Forest Property. Part II, Financial Aspects of Forest Management, is a course in economics, dealing with land and stumpage values and includes useful chapters on forest taxation and forest insurance.

The book is not always easy to read: much of the matter could have been expressed more simply and lucidly. The approach from first principles, the retention of elasticity so essential in approaching the problems of the irregular forest, the detailed working out of the formulæ and the graphical presentation of the results are valuable features of the book, which, although written primarily for America, meets the needs of many in this country and forms a useful addition to English literature on this subject.

A. S. W.

ZOOLOGY

An Introduction to Comparative Zoology: A text-book for Medical and Science Students. By F. G. S. Whitfield, F.R.E.S., F.R.M.S., and A. H. Wood, M.A. [Pp. x + 354, with 141 figures, including 6 plates.] (London: J. & A. Churchill, Ltd., 1935. 15*s*. net.)

The authors of this work have made a very laudable attempt to link up the sciences of medicine and zoology, and at the same time to deal with types of animals which are not restricted by Geographical distribution. For this reason both the Cockroach and Locust are dealt with, and such a form as Amphioxus omitted. The system used is mainly that of describing "types" of each group and enlarging on them, particularly in the case of those types which are parasitic or have parasitic relatives. Finally, there are concluding chapters treating such general topics as Heredity, Evolution, and Ecology.

The chapters on the Invertebrates, especially the insects, are admirably done. Those on the vertebrates are qute good but some of the figures are a little difficult to follow, whilst others are inaccurate. For example, the diagram of the dissection of the neck of the rabbit (Fig. 108) is rather obscured by too much shading, and in the figures of the skull of the dogfish (Figs. 80–2) the branchial arches are drawn as though they are not divided into pharyngo-, epi-, cerato-, and hypohyals; the pectoral fin of the dogfish (Fig. 83) differs considerably from that of *Scyllium canicula*.

In a book of this kind it is almost impossible not to make mistakes and one inconsistency can be pointed out. On p. 318 the Amphibia (rather loosely referred to as "Frogs") are shown arising in the Upper Devonian, whereas on p. 322 they are shown arising in the Lower Carboniferous.

The concluding chapters suffer from being too short, and much that is expected of the average medical student is omitted.

J. A. M.-T.

Heredity and the Ascent of Man. By C. C. Hurst, Ph.D., Sc.D. [Pp. x + 138, with 9 figures.] (Cambridge: at the University Press, 1935. 3*s*. 6*d*. net.)

Dr. Hurst took up breeding experiments with plants and animals before Mendel's principles were rediscovered. His work has therefore covered the

whole period of genetics, and has included experiments with orchids, fowls, rabbits, and especially roses, as well as the inheritance of such features as eye colour and intellect in man. His book on the Mechanism of Creative Evolution, which is a summary of modern genetics, is well known. He is therefore entitled to express his views in the present small book, which briefly and clearly summarises the main results of modern experimental work on heredity. The work is by no means a mere statement of the principles of heredity and variation involved, but the author indulges rather freely in pleasant speculations regarding such problems as the origin of life and the far distant future of mankind. These speculations are made not without philosophical background or scientific foundation, but some geneticists will probably regard them as somewhat hazardous.

Following chapters on the mystery of life and the laws of heredity, Chapter III discusses the gene and the origin of life. The juxtaposition of these two things results from the author's identification of genes and "progenes" with the bacteriophage and virus particles, the original and simplest of all organisms being regarded as genes. Somewhat similar views of the nucleus or chromatin as the oldest part of the cell have been put forward by Minchin and others. Such views, of course, encounter the difficulty of accounting for the later development of the cytoplasm. If the earliest organisms lived and multiplied as free genes, what induced them to accumulate about themselves the much less organised and specialised cytoplasm? There are no present means of deciding whether the highly organised genic material came first or whether it arose through a specialisation and re-orientation of an original undifferentiated protoplasm.

The chapter on the gene complex uses the colour factors in rabbits and in sweet peas to explain the nature of gene inheritance with a minimum of terminology, and the layman will find it easy to follow. Chapters V and VI with equal simplicity deal with the various types of change taking place in chromosomes and with the nature of sex. "The experimental creation of new species" sketches clearly some of the outstanding recent developments. The last two chapters are devoted to man, his mental development from Eolithic times and finally his future, which will be in some measure determined by his own control.

A few slips may be pointed out. The second division of the pollen grain nucleus generally takes place in the pollen tube (p. 23). The evidence does not favour the view that the polar bear is a single mutation from a dark coloured animal (p. 48). The unqualified statement (p. 54) that the disappearance of "the dominant major gene for normal intellect" may produce a genius or an idiot, is based on the author's own particular view which is not yet supported by evidence from other sources.

This little book can be quickly read and should serve to make the conceptions and results of genetics much more widely understood by those to whom genetics is at present only a name.

R. RUGGLES GATES.

Fishes: Their Ways of Life. By LOUIS ROULE. [Pp. viii + 312, with 52 figures.] (London: George Routledge & Sons, Ltd., 1935. 12*s*. 6*d*. net.)

THIS is frankly a popular book, and as such it will no doubt have a well-deserved success. It is lightly and gracefully written and covers a large

range of fishy topics ; if you have eaten Bouillabaisse on the Mediterranean littoral you will appreciate Prof. Roule's delightful account of the queer fish that compose it and how they are caught. You may read also of tunnies and sharks, the sinister *Muræna* and the placid carp, and a host of others. Other chapters deal in an interesting way with the sensory physiology of fish. The book has been admirably translated by Conrad Elphinstone.

E. S. R.

Report on the Fisheries of Palestine. By J. Hornell, F.L.S. F.R.A.I. [Pp. 106, with 14 figures.] (London : The Crown Agents for the Colonies, 1935. 7*s*. 6*d*.)

Those who have read Mr. Hornell's previous work expect a lucidity not elsewhere met with in Fishery Reports. They will not be disappointed in the contribution under review. The Report deals with an investigation of three months' duration made in the summer of 1934, and runs to 106 pages containing 317 numbered paragraphs whose contents are fully indicated in a synopsis. There is also an admirable glossary containing the local names of 74 species, the greater part of which have been identified by Mr. J. R. Norman of the British Museum of Natural History.

From statistics given in the appendix it would appear that the total local yield of Palestinian fish, excluding lake and river catches which are inconsiderable, was 1,045 metric tons in 1931 and 1,131 metric tons in 1933. The corresponding values in Palestinian pounds were £45,655 and £44,733. It has already been shown that fish becomes dearer as one goes eastward along the Mediterranean to Egypt. The present figures show a continuance of that effect, for comparing the price of fish in Egypt for 1931 with that for Palestine we have the figure ·87 shillings per kgm. in Palestine and ·49 in Egypt. Fish is thus a scarce and dear commodity and it is not surprising to see that about 75 per cent. of the fish consumed in Palestine is imported while in neighbouring Egypt the comparable figure is of the order of 10 per cent. The local fishery is therefore one of the least considerable in the Mediterranean.

It is considered that there is an inadequate exploitation of sea fisheries which contain no dearth of good quality food fishes. The lake fisheries show evidence of depletion, particularly Tiberias, where fish were abundant under the old concession system, whereas since the lake has been thrown open to all and sundry fish are relatively scarce.

There will be those who think that Mr. Hornell's very complete recommendations would have more properly succeeded biological studies which it is recommended should be deferred. At present judgments as to the degree of exploitation of the sea fisheries " which contain no dearth " must be largely speculative.

R. S. W.

The Hake and the Hake Fishery. By C. F. Hickling, M.A. [Pp. 142, with frontispiece and 13 figures.] (London : Edward Arnold & Co., 1935. 3*s*. 6*d*. net.)

Mr. Hickling is an Assistant Naturalist on the staff of the Ministry of Agriculture and Fisheries, and his book, which represents the Buckland

Lectures for 1934, contains a lucid summary, attractively written and illustrated, of the author's ten years' adventurous researches on this West Coast fish, as well as of their bearings on the past, present, and future of the Hake fishery.

Being a predaceous fish the Hake was originally caught by hook and line, and those who knew Devon and Cornwall 40 or 50 years ago must still remember the delicious dish which resulted from a fresh-caught Hake served up with Devonshire cream in lieu of sauce. Such local and seasonal fisheries, however, have long disappeared, having been superseded by a great deep-sea trawling industry based on Milford, Cardiff and Fleetwood instead of Plymouth and Newlyn, and ranging the fishing grounds from Morocco to the Hebrides. The problems now confronting this industry repeat with certain modifications those of the East Coast Plaice fishery. Large hake, which previously formed the chief element in the total catch, are now so scarce that last year they supplied merely one-sixth of it, while the total landings, which in earlier years mounted year by year to a climax of 900,000 cwt. in 1909, have since declined by more than a third, and show no signs of any natural recovery. How this follows from the slow growth-rate of the fish and the intensity of fishing can be followed in Mr. Hickling's pages. His advocacy of an increase in the mesh of the trawl, to save the smallest fish from premature destruction, is certainly worthy of careful consideration by the trawling companies. In this case, unlike that of the Plaice, there seems to be no increase in the growth-rate of the small fish to compensate for the disappearance of the large.

Readers of this little book will find it full of interesting detail, on which we have no space to dwell, and an admirable addition to the Buckland library of the applications of science to fishery problems.

W. G.

The Wasp. The Genus Vespa in Scotland. By W. B. R. Laidlaw, D.Sc., F.R.E.S. [Pp. 32, with 8 plates.] (Edinburgh: John Baxter & Son, Ltd., 1934. 3*s*.)

The common wasps of the genus *Vespa* are one of the few kinds of Hymenoptera in which everyone is interested. Few, however, apart from specialists, realise that the "Common" wasp is represented in this country by six species (four of which are common), besides the Hornet. The discrimination of these species requires some care, especially as their colour-markings, which have largely been relied on in text-books, are very variable. In the males, at any rate, identification by means of the structure of the genitalia is always certain and Dr. Laidlaw's figures of these will be helpful to British students. The coloured plates of the males are also attractive, though the colour-patterns are too variable to be very safe guides to recognition. The collected data on the distribution and the relative abundance of the species will be useful, though in this field Entomologists still lag far behind the Botanists. The accurate mapping of species-distribution and its relation to various climatic factors, maps of which the Meteorologists can already supply, is still largely an unworked field. Dr. Laidlaw's speculations as to the phylogeny of the species are less well founded. There is a large American literature relevant to this subject which he has not explored. In particular, in spite

of the valuable work of Carpenter and Pack-Beresford, it can now be taken as certain that *Vespa austriaca* is a social parasite of *V. rufa* and has evolved from one of the forms of that species.

O. W. R.

Lac and the Indian Lac Research Institute. By DOROTHY NORRIS, M.Sc., F.I.C., P. M. GLOVER, B.Sc., and R. W. ALDIS, Ph.D., D.I.C. [Pp. vi + 53, with 12 plates.] (Nankum, Ranchi, Bihar and Orissa: Indian Lac Research Institute, 1934. Rs.2/8.)

ALTHOUGH some lac is produced outside India much of what is produced outside finds its way into India via Calcutta and so India, including Burma and Assam, has a virtual monopoly of the industry.

The Indian Lac Research Institute was founded in 1925 as a direct result of the Government of India's enquiry into the best means of saving the industry which was seriously affected by the Great War. The present volume gives a survey of the Indian lac industry, an account of the work of the Institute since its inception, and also deals with the present outlook. It should appeal to a wide public.

On the discovery of aniline dyes the lac industry changed from being a trade in dye stuff to being a trade in resin, both in the unmanufactured state and as shellac. Many industries use shellac, notably the gramophone, electrical and the paint and varnish trades. The hatting industry also consumes lac extensively. In addition shellac is used in finishing leather, rubber, tinfoil and paper; in the manufacture of sealing wax, in photographic work, in lithographic inks; in the confectionary trade, in munitions and fireworks; in the toy and furniture trades and also in cements and glues.

Since 1925 the chief danger to the industry has been the ever increasing use of substitutes, such as the resins of the phenolformaldehyde condensation type (the Bakelite products). So far, however, shellac itself has not been synthesised. Such competition has opened up new fields of use, both for shellac alone and in combination with other resinous materials.

This interesting account ends on an optimistic note, stating that it appears that the lowest level of the depression period has been passed, and forecasts the fusion of shellac research interests in America, the United Kingdom and India.

H. F. B.

School of Biology. By C. THESING, M.D. Translated by E. and C. Paul. [Pp. xii + 368, with 91 figures.] (London: George Routledge & Sons, Ltd., 1935. 15*s*. net.)

THE first feeling on discovering that one has undertaken to read 300 pages written entirely as a dualogue between a teacher A and his pupil B is one of dismay. However, the technique proves to be less tiresome than might be expected. It is difficult to visualise the character of B, who alternates between stupidity and erudition, complaining repeatedly that he is unable to comprehend great and small figures, and yet able to volunteer that 1,867,848 barrels of oil were produced from whales in 1928–29. He seems to be an irritating and humourless fellow, but perhaps some of his more annoying remarks would have seemed more pleasing in their original German.

The statement on the dust-cover that the book contains no "errors

of fact or of interpretation" presents a target that challenges the sportsmanship of a reviewer. One would disdain to shoot at such a mark were it not for the dangerously biassed discussion in parts of the book, especially those dealing with eugenics. We have a first hint of this on p. 37 where we read that a "racial decline (dysgenic movement) . . . has manifestly been going on in Europe for many centuries." Is it manifest to most people that we are so noticeably inferior to our forefathers? Then later there is much of the familiar type of half truth and distortion of evidence which is only too common in discussions of these questions. A thorough-going policy of sterilisation is advocated, but nothing at all is said of the extreme difficulty of eliminating genetic factors in this way. We are simply told that "even if the community were to protect itself by only sterilising the worst cases . . . the good effect upon the general capacity and health of our population would become manifest within a very few decades."

Outside this section the biologist will find less with which to quarrel. Brachiopods are said to be Molluscs and to show "kinship to certain worms." Although the relationship of Balanoglossus to the Echinoderms is mentioned yet this fact is held to show that the latter were evolved from bilaterally symmetrical worms, and nothing is said of possible affinities with the Vertebrates. However, it would be unfair to stress the errors, for there are many subjects, difficult to handle in an elementary way, which are treated with considerable detail and accuracy. The book is full of interesting information and might be read with profit by schoolboys as a non-technical introduction to Biology, provided always that they be warned to mistrust the sections on Eugenics.

J. Z. YOUNG.

Education and Biology. By J. A. LAUWERYS, B.Sc., A.I.C. With the assistance of F. A. BAKER. [Pp. xvi + 207, with 4 plates.] (London: Sands & Co., 1934. *5s.*)

THE author of this book is very properly anxious that his purpose should be clear from the outset. It is to discuss the teaching of biology, especially to Catholics, and to give it a vitalistic basis which shall accord with the doctrine of the Roman Catholic Church. He is anxious that biology—of this kind—should not be excluded from Catholic schools. For "it should be the task of Catholic teachers to prepare their pupils for the criticisms which they will undoubtedly meet in after life." While, as he judiciously remarks, "It is difficult to see how the Catholic cause would be advanced by a policy which ensured that all biological posts, in the Colonies, for instance, were filled by non-Catholics."

Clearly such a book is of advantage to the Catholic doctrine. Certain statements which it contains make it perhaps questionable if it is an equally deserving contribution to science. For we are told that "it is only profitable to communicate to the individual those data which he is by age, nature and constitution, adequately fitted to understand," and that this is a maxim of the Church. Who is to perform the feat of judging what any individual is adequately fitted to understand, does not appear to be specified. Two other maxims of the Church are also offered for our acceptance. "Where innocence exists, it must not be disturbed," while we are told that "any decision of the Roman congregations are binding on her clergy and teachers from the point of view of discipline, but not necessarily as matters of faith.

Thus a view frowned upon may be held privately and personally, *but not propagated*" (the italics are mine). Such views may read rather strangely to the many scientists who endeavour not only to discover the truth, but to teach what they believe the truth to be.

The book criticises a number of biological theories and interpretations. This is always desirable, but it would be of more use if the subjects discussed had been brought up to date. One which receives special attention is "the theory of genes" which, it is contended, explains almost everything except *heredity*, in the biological sense. When isolated from the ideas of genotype and phenotype, and given without reference to the advances of modern physiological genetics, as it is here, this objection is naturally justified. When it is pointed out that, in addition to these omissions, there is no mention anywhere in the book of the theory of dominance and of the concept of the gene-complex, which have so greatly modified our views on the evolutionary aspect of heredity, it will be appreciated that this cannot be regarded as a serious contribution to the analysis of modern genetics.

These latter omissions necessarily affect the author's treatment of evolution itself. He takes the old view that selection merely preserves certain varieties and rejects others: a theory of survival, not of evolution. In neglecting the gene-complex, he fails to show that selection can alter the varieties themselves. Furthermore, he does not point out that evolution is generally a failure—for it most usually leads to extinction; a proposition which should colour any discussion on the subject.

The errors of fact which this work contains are relatively unimportant. By reference to the appropriate text-books, students may easily correct for themselves the account of cytology given on pp. 11–12, or the statement (repeated more than once!) that all mammals have seven cervical vertebræ.

The practical syllabus suggested for schools has considerable merits, though an account of photo-synthesis should be added. The author does good work in stressing the need for nature study outside the laboratory. He certainly seems to be right in insisting that instruction on sexual matters (if necessary) should not be given to *classes* of children, and that behaviour in children and adults must be judged on very different criteria.

Finally, mention must be made of an extraordinary mistake: the statement (p. 94) that variations taking a Normal Distribution are not inherited. In the section on genetics, the author has failed to develop the concepts of genotype and phenotype; here he has misunderstood them.

E. B. Ford.

MEDICINE

Individual Health. A technique for the study of individual constitution and its application to health. By E. Obermer. Vol. I: Biochemical Technique. By E. Obermer and R. Milton. [Pp. xvi + 244, with 40 figures and 4 folding plates.] (London: Chapman & Hall, Ltd., 1935. 15*s*. net.)

This book merits careful consideration because it embodies rather a novel idea. In the past, the direct application of biochemical analysis to problems of clinical Medicine has, in the main, been concerned with specimens derived from patients already ill, with the object of detecting and diagnosing disease. But the present time is witnessing a gradual change in the centre of gravity

of Medicine from its curative to its preventive aspects. It seems a natural consequence that an attempt should be made to explore the possibilities of the use of biochemical analysis in the service of preventive Medicine. The present book consists essentially of two parts. The more interesting, perhaps, is the short introductory section in which are explained the ideas which underlie the work on which the authors have been engaged. Emphasis is laid on the study of constitution in its widest aspects and the constitution of an individual involves his chemical as well as his physical make-up. A wide and extensive series of analysis, as complete as possible, of the blood, food and excreta of numerous individuals of all types is therefore required. Two main purposes would be served by such a survey—an increased knowledge of the behaviour throughout life of the physical-chemical make-up of individuals and the development of a method whereby threatened abnormalities might be detected at the earliest possible stage. To enable extensive analysis such as are contemplated to be carried out efficiently and economically, a special laboratory organisation and technique have been developed. The second part of the book—much the larger portion—describes this in detail. In a sense, of course, this part is independent of the general ideas already referred to and any reporting laboratory dealing with large numbers of specimens may find useful suggestions within these pages. A second volume is promised dealing with the application of physical methods in the examination of individuals on a similar extensive scale, and it is indicated that later on we shall learn the results and practical effects of this ambitious enterprise. We shall then be able to judge how far it has been labour wisely directed.

W. O. K.

Rats, Lice and History. By HANS ZINSSER. [Pp. xii + 301.] (London: George Routledge & Sons, Ltd., 1935. 10*s*. 6*d*. net.)

THE author, a professor of bacteriology in Harvard University Medical School, has written this book as a hobby chiefly for his own amusement, but partly also to stimulate the sympathy of the general reader in a subject of such wide interest as the influence of infectious diseases and their distributing agents rats and lice and fleas on the fate of nations and civilisations.

Mr. Zinsser sets out to write a " biography " of typhus fever. It is only at the twelfth chapter after over two hundred pages of general history and many side issues unrelated to the story that the subject is introduced. The first two chapters are superfluous; apologies are rarely necessary. Authors should not direct attention to their failings; these are obvious to the reader. Humour is individual and misplaced in this book; Mr. Zinsser seems so often to be laughing alone at his own jokes. Apart, however, from ill arrangement, irritating repetition, and above all his unrestrained habit of digression (though admittedly some of his bye paths are quite worth while), the author has given us an interesting and instructive book. He tells us that typhus fever belongs to a family of maladies known as rickettsia diseases, to which trench fever, Japanese river valley fever, Rocky mountain spotted fever, etc., belong. It is conveyed to man by insects, lice, fleas and mites who have picked up the virus, the rickettsia bodies, from the infected blood of rats and mice, the natural reservoirs of the disease.

Of the free living ancestral forms of rickettsiæ nothing is known, they are probably allied to true bacteria. Few of us realise how much and how

often "Man" has been at the mercy of plagues which arrested his progress and all but annihilated the human race. Conceivably similar epidemics may have attacked preceding races, and the resultant of elimination and immunisation has been one of the natural selection factors in animal evolution.

P. J.

Furneaux's Human Physiology. Revised edition by WILLIAM A. M. SMART, M.B., B.S., B.Sc., M.R.C.S., L.R.C.P. Nurses' edition. [Pp. viii + 348, with 210 figures.] (London, New York, Toronto: Longmans, Green & Co., 1935. 4*s.*)

FURNEAUX'S HUMAN PHYSIOLOGY has for many years been a popular book with nurses reading for their State Examination, and others working for similar examinations. The new edition, thoroughly revised by Dr. Smart, presents an up-to-date account of the subject.

One important feature of the book has always been the condensation of the very many facts of Human Physiology into a minimum of space. This feature of the book is maintained, and important matter and new words are emphasised by various forms of heavy type.

A number of the old diagrams have been replaced by new line drawings and photographs. Should further editions be required, as they undoubtedly will, it might be advisable to replace still more of the old line diagrams by new ones, and if the various parts of the anatomical sections could be labelled with either the name or the initial letters of the name of that part, rather than with numerals, this would help the elementary reader, who has so many things to remember when studying Human Physiology for the first time.

A note in the Preface states that this book is available for schools without the chapter on reproduction.

T. C.

PHILOSOPHY AND THE HISTORY OF SCIENCE

Aspects of Dialectical Materialism. By H. LEVY and others. [Pp. vi + 154.] (London: C. A. Watts & Co. 5*s.* net.)

ACCORDING to the Foreword, the essays contained in this book are published "in response to an urgent demand from large numbers of interested individuals that the philosophy guiding the practice of Modern Russia might be expounded in a form intelligible to the layman." They can hardly be said to have met the alleged demand. If one looks here for the meaning of Dialectical Materialism he will be told by Prof. Levy in the first essay that it is not dialectical, and by Prof. McMurray in the second that it is not materialism. Mr. Fox follows with a criticism of views attributed to Prof. McMurray which we cannot find in that gentleman's essay, the essential points of which Mr. Fox scarcely considers. The opening of Mr. Page Arnot's essay tells us that Dialectical Materialism is the "general method, which may also be regarded as the *world outlook* of Marxism," and, in the same breath, that it is only one aspect of Marxism. Dr. Bernal's essay begins: "Dialectical Materialism is the most powerful factor in the thought and action of the present day. Even its most bitter enemies are forced to recognise its analysis and ape its methods." This fittingly introduces a discussion whose extremely unscientific temper, astonishing in view of the occupation

and eminence of the author, is nowhere shown more surprisingly than in the absurd statements concerning physics—unless, indeed, the supreme thrill is reserved for the end, where an intelligible objection on the part of the "interested individuals" for whom the book is written is met by the reply: "The plain answer to this is to get Communism first, and argue about it afterwards." ("But," he complains, "that will not satisfy the philosophic critics.") A criticism of the theory by Mr. Carritt, followed by a note from Dr. Bernal explaining where Mr. Carritt has misunderstood or distorted the fundamental ideas of Marxism, concludes a volume which, with the exception of the contributions by Professors Levy and McMurray, is scarcely likely to appeal to readers who try to think scientifically.

H. D.

Pareto's General Sociology : A Physiologist's Interpretation. By PROF. L. J. HENDERSON. [Pp. x + 119.] (Cambridge, Mass. : Harvard University Press; London : Humphrey Milford, 1935. 5s. 6d. net.)

THE name of Pareto is not generally familiar to English ears—a fact much to be deplored in view of the fundamental importance of his sociological work in relation to current history. "It is a fact," says the author of the book before us, "that Signor Mussolini has attributed his abandonment of socialism to the teaching of Pareto"; and although Prof. Henderson does not approve of the description of Pareto as the Karl Marx of Fascism, it is clear enough that his work cannot be neglected by anyone who wants to understand what is happening in Europe to-day. It would be a mistake, however, to suppose that the book is merely topical. Pareto's *Trattato di Sociologia Generale*, which was published in Florence in 1916 and translated into English and published in America in 1935 in four volumes under the title, *The Mind and Society*, is a thorough-going scientific analysis of the influence of the sentiments upon human affairs. It is closely related to the work of Machiavelli, and in consequence must overcome the initial handicap of prejudice before it can make its rightful appeal to critical consideration. To attempt in 300 words to summarise a 25,000-word summary of four closely reasoned volumes would be absurd. All we can say is that Prof. Henderson has given a clear and interesting account of a doctrine which deserves the fullest consideration. We cannot say from direct experience how accurate it is, but the reputation of the author and the fact that, although found ultimately to be free from ambiguity, it is not to be understood without very careful reading (an essential characteristic of accuracy with such a high degree of condensation) are sufficient guarantee that we may read it with an easy mind. In temper and logical coherence it stands in striking contrast to the subject of the preceding review—a fact which suggests that, among scientific men, the most effective propaganda is that which has no intention of being propaganda at all. We can recommend the book unreservedly as a worthy introduction to an important subject.

H. D.

British Scientists of the Nineteenth Century. By J. G. CROWTHER. [Pp. xii + 332, with 12 plates.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1935. 12s. 6d. net.)

MR. CROWTHER'S choice of the lives and work of Davy, Faraday, Joule, Thomson and Maxwell as the subject of his book is an admirable one,

although in thus limiting himself to the chemistry and physics of a century which saw great achievements by Englishmen in other branches of science, he prompts the thought that a less comprehensive title would have given a better indication of the scope of his work. This is, however, a minor criticism of a book which attempts, and to a considerable extent succeeds in, an explanation of the significance of the work of these great men to the twentieth century.

In an essay of sixty or so pages to each, their education and scientific development are described. The discussions of their important researches in relation to the progress of scientific thought, and to industrial applications, are interesting, and often illuminating. Much biographical information not generally met with, for example, in the life of Joule and in the comparisons between Kelvin and Maxwell, is included. But Mr. Crowther is also concerned to treat his subjects as sociological phenomena, and to show them in their relation to the "capitalist industrialism" of the nineteenth century. Here he is on more controversial ground. When he is led, in the pursuit of his theme, to make such assertions as that "Davy's chief greatness was in his work as an expositor of the significance of science, and as the voice of the industrialists who desired to apply science to the production of wealth," the reader is inclined to put down his book, and dispute his conclusions.

T. M.

The Discovery of Specific and Latent Heats. By DOUGLAS MCKIE, Ph.D., B.Sc., and NEILS H. de V. HEATHCOTE, B.Sc. With a foreword by E. N. DA C. ANDRADE, D.Sc., Ph.D., F.R.S. [Pp. 155, with 6 plates and 2 figures.] (London: Edward Arnold & Co., 1935. 6*s*. net.)

THIS volume is a most welcome addition to the literature of the history of Physics, for it is the result of research into what has hitherto been a very obscure epoch, namely that of the beginnings of the scientific study of Heat. It is especially valuable as the authors have studied the work of the original workers in the languages in which they wrote.

In these days of organised research with the finest and most sensitive instruments it is salutary to try to realise the difficulties and immense labour which were involved in laying sure foundations and to realise how slowly ideas which now appear commonplace were evolved. For this reason alone it behoves every student of Physics to study this book, the main interest of which is centred around the work of the Scotsman Black (1728–99) and the Swede Wilcke (1732–96), who, working independently, were the first to evolve clear ideas in regard to latent and specific heats and so laid the foundations of the art of calorimetry. The authors show that Black's work has priority over that of Wilcke by several years. Hitherto the work of the latter has not been available to English readers, which makes the book of unique interest, for here we have a full account of his experiments and can follow the progress he made in gradually realising the sources of error and his efforts to overcome them.

A very interesting account is given of the attempts to find a mathematical formula for calculating the temperature attained by mixing two masses of water at different temperatures, a problem which occupied philosophers of several countries for nearly a hundred years and which was solved

at last by the Russian Richmann (1711–53). He was the first to realise clearly the difference between Heat and Temperature and thus played an important part in making progress possible.

The authors have made a judicious selection of the results of the various experimenters, and their critical and sympathetic comments, enlivened by flashes of humour make the book very readable and we recommend it wholeheartedly to the attention of all serious students and teachers of Physics.

E. NIGHTINGALE.

MISCELLANEOUS

The Poetical Works of Kenneth Knight Hallowes. Vol. 1, 1896–1934. [Pp. xvi + 211.] (London: Methuen & Co., Ltd., 1934. 7*s.* 6*d.* net.)

AGAIN we have before us a volume of verse by Mr. Hallowes. It is prefaced by an autobiography. We are there told of his work in the Geological Survey of India; of how on retirement after eighteen years of that Service he proceeded to take Holy Orders; and of how at fifty-five years of age he has felt it desirable to collect his scattered poems.

These in the present volume he separates into four groups, the final one entitled "Poems of Science." It is with notice of this last group that our review here must perforce be content. To this group the author has attached a special "Appendix," by which he delivers himself of the message that in our day it is high time for Poetry to make a certain new departure. There is a call, he says, for Poetry to take as central theme the Natural Sciences themselves. One result which, he argues, will accrue from such a movement will be the enhanced interest in, and feeling for, Science communicated to the man-in-the-street.

The view thus put forward and the collection of verse in illustration of it received notice in these pages in July 1934. The "poems of Science" gain, we think, from being set as now within a less-restricted collection. Their geology comes with fresher effect by contrast with more usual themes. If the scientific theme itself still fails somewhat in power to reach and stir the wells of individual emotion, these verses do yet convey to us an impression of their author as being truly moved by reflections which he would that his reader shared with him even with feelings like his own. That close-up glimpse of the author, moved by communion with Nature, emerges, so it seems to us, as the upshot of main interest to the reader in these pages. That it should be so, tends, in so far, to confirm the view taken in our previous notice, that human personality rather than the impersonal facts and generalisations of Science has potency of poetical appeal.

C. S. S.

The World of Colour. By DAVID KATZ, Dr. Phil. Translated by R. B. MACLEOD, Ph.D., and C. W. FOX, Ph.D. [Pp. xvi + 300, with 12 figures, including 1 plate.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1935. 15*s.* net.)

THIS treatise is essentially a discussion of the psychological problems inherent in colour perception. Prof. Katz analyses, for example, the psychological characteristics of film and surface colours, lustre, memory colours,

after-images, the psychological relation of illumination to the appearance of achromatic and chromatic surfaces and so on; but undoubtedly the most interesting part of the book is that in which the problem of colour constancy is debated. The nature of this problem can be understood from the simple experiment in which a black surface, when illuminated by an intense beam of light concentrated solely on the surface, appears to be white or whitish-grey; but on the instant at which a piece of white paper is inserted in the beam, the black surface suddenly looks black. What are the mental associations called into play to bring about such a result? This fascinating problem is discussed for a variety of conditions, all more complex than in the simple case quoted above. We cannot accept all of Prof. Katz's arguments and we would particularly challenge his dislike of physical and physiological purism. It is no doubt very praiseworthy to investigate the problems of visual perception as they occur in the highly complicated surroundings of our everyday life, but it is surely a thorny path to travel on the road to knowledge. Or does Prof. Katz regard thorns as having value in themselves? This criticism seems particularly relevant in view of our very scant knowledge of the physiological processes in vision, and as a particular example, we may quote the analysis of the effects of adaptation. It is impossible to say, with certainty, the part that adaptation may play in producing physiological modifications of the visual responses and until more information is forthcoming, the psychological functions must remain indeterminate. Psychologists must, for the moment, wait upon physiologists. For this reason we feel that the book is over-long. We cannot afford to be burdened with too detailed arguments on matters on which our views are liable to change as new facts are discovered.

W. D. W.

Science and the Human Temperament. By ERWIN SCHRÖDINGER. Translated and with a biographical introduction by JAMES MURPHY. [Pp. 154, with 13 figures.] (London: George Allen & Unwin, Ltd., 1935. 7*s*. 6*d*. net.)

THIS is a book intended chiefly for the layman. In it Prof. Schrödinger considers the influence, on modern scientific views, of dominant ideas in other branches of human activity, such as literature, art, sociology and politics. It also contains some lectures which the author delivered on special occasions, such as that at Stockholm when he received the Nobel Prize.

It may be said at once that there is not a dull page in the whole book, and that it makes both interesting and stimulating reading.

In discussing the question "Is science a fashion of the times?" and pointing out the difference between Hellenic and Gothic science—between static and dynamic views of nature—Prof. Schrödinger comes very near to the Spenglerian thesis regarding the morphology of science. He makes clear the influence of the Darwinian theory of evolution on the development of cosmogony.

The effect of the prevailing temper of the age on physical science, the author classifies under three headings: (*a*) Simplicity and purposefulness in the arts and crafts: "Just as we are no longer afraid of bare surfaces in our furniture and our dwelling-rooms, so in our scientific picture of the external world we do not try to fill up the empty spaces"; (*b*) Desire for

change and freedom from authority: "There is to-day a profound scepticism in regard to traditionally accepted principles"; (c) The idea of relativity and invariance: "A statement is very seldom simply either right or wrong, but that nearly always a point of view is to be found from which it is right and another point of view from which it is wrong."

Other lectures discuss the meaning of laws of nature, the fundamental ideas of wave-mechanics, the value of conceptual models, and indeterminism. Here, unfortunately, a measure of obscurity enters into the author's otherwise clear presentation owing to lack of definition of the terms used. The present reviewer has, for some time, been a voice crying in the wilderness, imploring the indeterminists to define the words they use so loosely; chance, chaos, random motion, etc., etc. What, for instance, is the physical meaning to be attached to the phrase "*statistically* regulated phenomena" (p. 115)? How has a method of dealing with numbers (statistics) become a cause in nature capable of altering the motion of bodies?

Questions such as this are bound to arise in the reader's mind; and he will remember that all good science starts with clear definitions. It is this lack of clear definition of the terms used in the indeterminist's description of nature that makes it so unconvincing. It is to be hoped that Prof. Schrödinger will soon give us another book in which these last lingering doubts are removed.

G. Burniston Brown.

The Tyranny of the Mind: A Common Sense Psychology. By Sir Bampfylde Fuller, K.C.S.I., C.I.E. [Pp. 253.] (London: T. Werner Laurie, Ltd., 1935. 8*s.* 6*d.* net.)

The contention of this "common sense" psychology is stated on the wrapper. "We think of the Mind as the Body's inspiring helpmate; and, in fact, its early influence is of this kind. But it gradually thrusts its way into the position of a tyrant that drags our eyes from the steady beacons of experience and fixes them upon the passing brilliance of shooting stars. We might avoid these misdirections if we realised them; and this book traces their origin in the growth and working of our mental faculties." This statement is quoted because it not only indicates the purpose of the volume, but also throws light upon the kind of "common sense" information used in the construction of the argument. There is hardly a page in the book in which the professional psychologist of whatever school will not find assertions to which he would be obliged to take exception. Probably the author intended to anticipate this criticism by writing at the outset of Chapter I: "We all know, intuitively, that we are composed of Body and Mind, both of which are inspirited by Emotional Energy. This knowledge is, however, confused by the language of psychology." But if one writes a psychology, even if it also is to be "common sense," psychological terms must be employed; and they should be used in their technical signification. It is perhaps chiefly, though not entirely, because they are not so used in this book that the psychologist will find fault. The term "intelligence" is an instance. The author writes of "physical" and "mental" intelligence, and defines the latter as "the analogical reproduction of ideas." It is confusing to equate intelligence with reproduction, and thus close the door to novel discovery and invention. By "physical"

intelligence he means "a faculty which can act independently of the Mind," and by "mind" apparently a thinking substance (or power); for "Purely physical life . . . includes sensory impressions, emotions and impulses, but not the ideas that would bring them into consciousness." The foregoing is a sample, of many, of the unusual employment of psychological terms. Other criticisms, not merely terminological, will be raised against such assertions as that acquired experiences are transmissible, or that ideas of movement automatically reproduce themselves in conduct or speech. The old ideo-motor theory has been discredited. Apart from the subject-matters of criticism by the professional psychologist, the book is highly interesting, packed with information on a large range of topics, and abounding in illustration by simile and metaphor. Indeed, it would seem to be the author's very free use of analogy that is responsible for the views the psychologist would criticise.

F. A.

Ancient Egyptian Materials and Industries. By A. Lucas, O.B.E., F.I.C., F.S.A. Second edition. [Pp. xii + 447.] (London: Edward Arnold & Co., 1934. 16*s*. net.)

It would be difficult to exaggerate the importance of this book for archæologists in general and for Egyptologists in particular. The latter often possess no more than an elementary scientific knowledge of the materials, whether raw or manufactured, which excavations in Egypt continually bring to light. To Mr. Lucas has fallen the onerous task of filling up the gap, and for this he has been admirably fitted by his long experience as chemist in the Department of Antiquities under the Egyptian Government. The first edition of his book *Ancient Egyptian Materials* appeared in 1926, the present book is practically a new work, re-written and abundantly enlarged. In the course of its 447 pages the learned author discusses the nature and source of every material employed by the ancient Egyptians, from gold and less precious metals to incense and cosmetics, supplementing his classifications by an invaluable description of the various industries which flourished so many thousands of years ago. Among the latter the lively industry of mummification is accorded a detailed treatment, the author reaching a conclusion which many archæologists will find it hard to accept. He believes that the old view that the corpse was soaked for many days in a *bath* of natron solution can no longer be maintained, and that it was on the contrary packed with *dry* natron, thus avoiding the difficult process of desiccation which the soaking would have afterwards necessitated.

If the author is to be congratulated upon his almost exhaustive treatment of the materials he should be doubly felicitated upon his attempt, in Chapter XV, to draw up a history of ancient Egypt in terms of her materials and industries and to relate these to the rest of the Eastern World; an attempt which should encourage others to give more thought to this important subject. For the spread of modern scientific excavation over the East is rapidly depriving archæology of its merely local applications of former days, and is forcing specialists more and more to extend the range of their studies over the ancient world in general. In the compilation of mankind's history Mr. Lucas's subject must play one of the leading parts.

Alan W. Shorter.

The Identification of Firearms. By JACK D. GUNTHER, A.B., LL.B., and CHARLES O. GUNTHER, M.E. [Pp. xxviii + 342, with frontispiece and 148 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 20s. net.)

IN crimes of violence by shooting it is often of vital importance to be able to determine whether a fired cartridge case found on the scene of the crime, or a fired bullet extracted from the victim, was fired by one particular weapon found in the possession of a suspect man, or was not fired by this weapon. The authors of this book explain how this work of identification is carried out.

Very briefly it can be stated that the principle on which this work is based is the fact that no manufactured metal surfaces can ever be microscopically identical, for even consecutive cuts made by the same tool leave surfaces which have individual characteristics when viewed under the microscope. The pressure developed by the powder gases forces the base of the brass cartridge back against the steel breech of the weapon, and thus the softer brass becomes imprinted with the microscopic individuality of that particular weapon.

Similarly every bullet which travels up a rifled barrel becomes engraved with the striations which comprise the microscopic individuality of that barrel.

Thus it is that every weapon leaves what the authors aptly term its "signature" on every cartridge case or bullet which is fired in it.

The work of microscopic identification is explained, and the subject is covered fully and carefully; but the possibility of using any other instrument than a Comparison Microscope is not considered, and the illustrations are not always clear.

Two-thirds of the book are devoted to verbatim reports of different trials, one-third being occupied by the Saccho-Vanzetti case.

The book, however, will be most useful to the professional investigator.

G. B.

The Design of Experiments. By R. A. FISHER, Sc.D., F.R.S. [Pp. xii + 252, with 5 figures.] (Edinburgh and London: Oliver & Boyd, Ltd., 1935. 12s. 6d. net.)

THE Galton Professor of Eugenics is well known for the services which he rendered in the cause of agricultural science while associated with the Rothamsted Experimental Station. New methods of experimentation, based on statistical principles, were invented by him, and have since spread widely throughout the world. His well-known book, *Statistical Methods for Research Workers*, first published in 1925, contains an account of the methods, but their importance, combined with the difficulty of providing for an adequate treatment of the subject in a general manual of statistical practice, has made a new work necessary.

In the book under review Professor Fisher puts his knowledge of the general subject of experimental design, to which he has himself made many notable contributions, at the disposal of his readers. Much of the book is devoted to field experimental methods in agriculture, but it is not only students of this subject who will profit by the book. The aim rather is to show general principles, and to formulate the rules governing good design; the book should appeal, therefore, to all scientific workers who have recourse to

experiments for aid in advancing knowledge. The author begins by quoting from an essay of Robert Boyle of 1673 "concerning the unsuccessfulness of experiments," but this is only a prelude to a disquisition aimed at showing how they can be made successful, not necessarily, that is, in producing results at all cost, but rather in enabling clear and definite answers to be recorded on the questions put at the outset.

A preliminary chapter on inductive inference and the "logic of the laboratory" paves the way for statements of hypotheses to be confirmed or refuted, and for discussion of the necessary statistical methods, based on two simple experiments. Field experimental methods follow, and there is little in this subject that is left unsaid. In particular the results of recent researches on methods involving factorial design and "confounding" are minutely described. In the concluding chapter the author deals with the measurement of amount of information, always a favourite topic with him, and there is much here, including applications to genetics, that will repay careful study.

JOHN WISHART.

The Scientific Journal of the Royal College of Science, Vol. V. [Pp. 138, with illustrations.] (London: Edward Arnold & Co., 1935. 7*s.* 6*d.*)

THE subject matter of the fifth volume of this journal, which consists of some of the papers read before the scientific societies at the Royal College of Science, London, during 1934–35, is as varied and of the same high standard as that of its predecessors. Eleven lectures are treated at length, others are summarised. Papers of interest both to the specialist and the general reader will be found, and of the latter may be mentioned Prof. W. E. Garner's paper on *Solid Reactions and Detonation*, Mr. D. Tabor's *An Introduction to Television* and Mr. A. F. Baker's lecture, *The Significance of Smell.* Dr. Rawling's contribution, *Some Chemical Themes in Photography*, deals with photographic sensitive materials, outlining the method of preparing the sensitive film and discussing researches on the effects of exposure to light, the formation of the latent image and theories of development. *Some Aspects of Timber Research*, by Mr. W. P. K. Findlay, describes some of the work at the Forest Products Research Laboratory at Princes Risborough and shows the manner in which a commodity, timber, is receiving investigation in the hands of specialists in different fields of science: he gives particular attention to the decay of wood and the methods which are being tested at the laboratory for combating it, and also some interesting information on Dry Rot.

A perusal of the book leaves a feeling of regret that more University Scientific Societies are not able to put on record at least some of the lectures delivered to them.

F. W. J.

The Subject Index to Periodicals, 1934. [Pp. xii + 566 columns.] (London: The Library Association, 1935. £3 10*s.* net.)

THE Library Association is to be congratulated upon its success in producing its annual Subject Index to Periodicals some six weeks earlier this year than last. To have listed and classified some 26,000 articles from some 600

journals and to have produced the resulting volume in a space of eight months is a piece of work of which the editor, Mr. T. Rowland Powel, and his body of voluntary contributors may well feel proud. The value of the present volume is still further increased by the inclusion for the first time of much fuller bibliographical detail concerning the periodicals indexed. The title of each journal in the list is now followed by the address of the publishers or learned society responsible for its issue, the frequency of publication, and the price. This information will be of obvious advantage to those who wish to obtain copies of periodicals in which they are interested.

The list of French and German periodicals indexed has been revised under the guidance of a number of specialists and made more comprehensive. Nine additional foreign journals appear for the first time in this volume. If this list of titles still appears at first sight to omit a number of publications of obvious importance to English readers, it must be remembered that the Library Association very reasonably abstains from indexing periodicals already dealt with in the recognised specialistic indexes such as Engineering Abstracts or Index Medicus.

J. W.

The Chemistry of Thought. By CLAUDE A. CLAREMONT, B.Sc. [Pp. 259, with 4 plates.] (London: George Allen & Unwin, Ltd., 1935. 8*s.* 6*s.* net.)

MR. CLAREMONT is known as an exponent of the Montessori system; and he claims that the observation of children being educated by her method provides one of the best indications of the nature of the thought processes. Using this line of approach, he analyses thinking into seven elements, "fundamental happenings out of which all thoughts are made up." These are "the complex unit" (such as the power of walking or counting), the combination of such units in systems, associative recall, the direct perception of causation, and the elements of conation, manipulation and character. The whole personality is involved in thinking. No claim of originality is made except in respect of the complex unit and the direct perception of causation. While this book is suggestive and provocative, it will meet with criticism not only at the hands of philosophers, but of psychologists also, on several counts. The statement that, though limited to certain of its types, we can apprehend causation in the external world by direct perception is an assertion that is made but not proved. The one instance given of such a perception is drawn from our experience of ourselves as causes. "We take a hammer to strike a nail, and it seems to us that we understand perfectly why the nail goes into the piece of wood; how and why, if the wood be softer, the nail goes further, etc." We certainly do. Maine de Biran would say it is because we experience effort; others would point to the volition that causes the action; but surely it is not solely on the strength of the perception of the nail penetrating the wood. There is a succession, which may be a sign of causality; but it is not itself a cause. The author, however, wishes to enlarge the concept of causation to include that of necessity. Possibly this is why he claims a direct perception of causation in the sense he does. For clearly we may have direct awareness of reasons; and, if these are to be equated with causes, no doubt he could support his case. A serious criticism lies also against Mr. Claremont's treatment of the doctrine of Noegenesis. He takes the solving of a crossword puzzle as a typical

example to show that Spearman's Principles cannot account for the mental processes involved. They obviously cannot if some of them are ignored; and Mr. Claremont ignores all save the noegenetic ones. Spearman would never have tried to account for the processes involved in crossword solving without making use of the quantitative principles as well as the noegenetic. Indeed, one of his own examples is precisely on the lines that should have led Mr. Claremont to see how such puzzles are in fact solved—by a succession of eductions and reproductions until the required solution of the clue is obtained. Despite such criticisms *The Chemistry of Thought* will well repay the attention of psychologists and educationalists.

F. A.

Unsolved Problems of Science. By A. W. Haslett. [Pp. xi + 317, with 5 figures.] (London: G. Bell & Sons, Ltd., 1935. 7*s.* 6*d.* net.)

Mr. A. W. Haslett, scientific correspondent to the *Morning Post*, has already gained a reputation during recent years as a writer of popular scientific articles; and he has done much to provide the intelligent public, who read their newspapers for serious and accurate information, with reliable accounts of the practical concerns and problems of modern science. In view of the "sensational headline" and "hot news" complexes that most newspapers appear to have formed where scientific discovery and advance are concerned, the presentation of these to the public by a responsible journalist with a proper scientific training is a reformation much to be welcomed by all serious scientific people. That Mr. Haslett should here extend his work in this way is still more to be welcomed; for he has very successfully summarised the present achievements of science and specially studied the problems that call for immediate solution in the future. After describing appreciatively the unending quest of science, he proceeds to discuss the origins of the universe, the existence of other worlds than ours and the changes undergone by our planet in its long history. The next chapter on weather and the problem of forecasts is, to one reader at least, the most interesting in the whole book. Messages from space, the origin of man, the beginnings of civilisation, the problem of whether man is a machine, the riddle of sex, nature's building bricks, natural law and statistical probability, and the strength of materials are dealt with in succeeding chapters. Every chapter is thoroughly well informed and written in plain non-technical words. The book should have an appeal, not only to the intelligent general reader, but also to students and teachers of science and to scientific readers in general, since the scientist trained in one field is usually little more than a general reader in others: it should certainly find a place in every public library as well as in school and college libraries.

D. McKie.

BOOKS RECEIVED

(*Publishers are requested to notify prices.*)

Analytical and Applied Mechanics. By Guy Roger Clements, Ph.D., and Levi Thomas Wilson, Ph.D., Professors in the Department of Mathematics, United States Naval Academy. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. x + 420, with 343 figures.) 21*s*. net.

Interpolation and Approximation by Rational Functions in the Complex Domain. By J. L. Walsh, Professor of Mathematics in Harvard University. American Mathematical Society Colloquium Publications, Vol. XX. New York: American Mathematical Society, 1935. (Pp. x + 382, with 3 figures.) $5.00.

Factor Table giving the complete decomposition of all numbers less than 100,000. Prepared independently by J. Peters, A. Lodge and E. J. Ternouth, E. Gifford, and collated by the British Association Committee for the Calculation of Mathematical Tables. British Association Mathematical Tables, Vol. V. London: Office of the British Association, 1935. (Pp. xvi + 291.) 20*s*. net.

Geometry of Time and Space. By Alfred A. Robb, Sc.D., D.Sc., Ph.D., F.R.S. Cambridge: at the University Press, 1936. (Pp. viii + 408, with 57 figures.) 21*s*. net.

Astronomy. A Textbook for University and College Students. By Robert H. Baker, Ph.D., Professor of Astronomy in the University of Illinois. Second edition—Third printing. London: Macmillan & Co., Ltd., 1935. (Pp. xx + 522, with frontispiece and 286 figures.) 16*s*. net.

A Treatise on Heat. (Including Kinetic Theory of Gases, Thermodynamics and Recent Advances in Statistical Thermodynamics). Being the second and revised edition of A Text Book of Heat. By M. N. Saha, D.Sc., F.R.S., Professor of Physics, and B. N. Srivastava, M.Sc., Lecturer in Physics, Allahabad University. Allahabad and Calcutta: The Indian Press, Ltd., 1935. (Pp. xii + 815, with 2 plates, 257 figures and 76 tables.)

Sound. An Elementary Textbook on the Science of Sound and the Phenomena of Hearing. By Floyd Rowe Watson, Professor of Experimental Physics, University of Illinois. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. x + 219, with 179 figures and 19 tables.) 12*s*. 6*d*. net.

A Fugue in Cycles and Bels. By John Mills. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. viii + 269, with 34 figures and 4 tables.) 13*s*. 6*d*. net.

Modern Views of Atomic Structure. By Dr. Karl Rast. Translated from the German by Dr. W. O. Kermack. London: Frederick Muller, Ltd., 1935. (Pp. x + 156, with 25 figures and 8 tables.) 7*s*. 6*d*. net.

The Atom. By Professor E. N. da C. Andrade, D.Sc., Ph.D., F.R.S., Fellow of University College, London, Quain Professor of Physics in the University of London. Nelson Classics. London: Thomas Nelson & Sons, Ltd., 1936. (Pp. x + 129.) 1*s*. 6*d*. net.

Thermionic Emission. By T. J. Jones, M.Sc., Research Physicist for Lissen Ltd., London, and for Pye Radio Ltd., Cambridge. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1936. (Pp. viii + 108, with 17 figures.) 3*s*. net.

Electron Diffraction. By R. Beeching, A.R.C.S., B.Sc., Demonstrator in Physics at the Imperial College of Science and Technology. With a Preface by G. P. Thomson, F.R.S. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1936. (Pp. viii + 107, with 1 plate and 39 figures.) 3*s*. net.

Infra-Red and Raman Spectra. By G. B. B. M. Sutherland, M.A., Ph.D., Fellow of Pembroke College, Cambridge. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1935. (Pp. xii + 112, with 27 figures and 11 tables.) 3*s*. net.

Force and Matter Anno 1935. By Dr. W. Tombrock. With a Preface by The Hon. Sir Shah Sulaiman, M.A., LL.D. Oxford: The Shakespeare Head Press, Ltd., 1935. (Pp. xii + 27.) 2*s*. net.

The Revolution in Physics. By Ernst Zimmer. With an Introduction by Max Planck. Translated, and with a Preface, by H. Stafford Hatfield. London: Faber & Faber, Ltd., 1936. (Pp. xvi + 240, with 58 figures, including 13 plates.) 12*s*. 6*d*. net.

Laboratory Manual in Physics. By A. A. Knowlton, Ph.D., Professor of Physics, and Marcus O'Day, Ph.D., Assistant Professor of Physics, Reed College. Second edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xii + 137, with 67 figures.) 7*s*. 6*d*. net.

The Science Masters' Book. Series II. Part I: Physics. Part II: Biology, Chemistry, Experiments for Receptions. Being experiments selected from *The School Science Review*, by a committee of the Science Masters' Association. Edited and arranged by G. H. J. Adlam, O.B.E., M.A., B.Sc. London: John Murray, 1936. (Part I: pp. xvi + 289, with 217 figures. Part II: pp. xvi + 267, with 97 figures.) 7*s*. 6*d*. net each.

Flame. By Oliver C. de C. Ellis, M.Sc., Ph.D., F.I.C., F.R.P.S., A.M.I.Min.E., Head of the Flame Section, and William A. Kirkby, M.Sc., Ph.D., F.I.C., of the Flame Section, The Safety in Mines Research Board. Methuen's Monographs on Chemical Subjects. London: Methuen & Co., Ltd., 1936. (Pp. vi + 106, with 1 plate and 14 figures.) 3*s*. net.

Electrolytic Oxidation and Reduction: Inorganic and Organic. By S. Glasstone, D.Sc., Ph.D.(Lond.), F.I.C., Lecturer in Physical Chemistry, University of Sheffield, and A. Hickling, M.Sc., Ph.D. (Sheffield), Assistant Lecturer in Chemistry, University College, Leicester. Vol. IX of a Series of Monographs on Applied Chemistry under the Editorship of E. Howard Tripp, Ph.D. London: Chapman & Hall, Ltd., 1935. (Pp. x + 420, with 31 figures and 61 tables.) 25*s*. net.

Principles and Applications of Electrochemistry. Vol. II: Applications. By W. A. Koehler, Professor of Chemical and Ceramic Engineering, West Virginia University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xiv + 545, with 245 figures and 35 tables.) 25*s*. net.

Lehrbuch der organischen Chemie. By Paul Karrer, Professor an der Universität Zürich. Fourth edition. Leipzig: Georg Thieme, 1936. (Pp. xxiv + 955, with 1 plate, 3 figures and 39 tables.) RM.34.—, paper covers; RM.36.—, bound.

The Chemistry of Milk. By W. L. Davies, Ph.D. (Cantab.), D.Sc. (Wales), F.I.C., Research Dairy Chemist and Analyst, National Institute for Research in Dairying, Shinfield, near Reading. Being Vol. X of a Series of Monographs on Applied Chemistry under the Editorship of E. Howard Tripp, Ph.D. London: Chapman & Hall, Ltd., 1936. (Pp. xii + 522, with 26 figures and 126 tables.) 25*s*. net.

A Laboratory Course in Elementary Chemistry. By E. B. R. Prideaux, M.A., B.Sc.(N.Z.), D.Sc.(Lond.), F.I.C., and F. C. Laxton, B.Sc.(Lond.), A.I.C., both of University College, Nottingham. London: William Heinemann, Ltd., 1935. (Pp. xiv + 258, with 37 figures.) 3*s*.

The Book of Minerals. By Alfred C. Hawkins. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. xii + 161, with 5 plates and 62 figures.) 7*s*. 6*d*. net.

The Structure of the Alps. By Léon W. Collet, D.Sc., LL.D., Professor of Geology and Palæontology in the University of Geneva. With a Foreword by O. T. Jones, D.Sc., F.R.S. Second edition. London: Edward Arnold & Co., 1936. (Pp. xvi + 304, with 12 plates and 84 figures.) 20*s*. net.

Tertiary Faunas. A Text-book for Oilfield Palæontologists and Students of Geology. Vol. I: The Composition of Tertiary Faunas. By A. Morley Davies, D.Sc., A.R.C.S., F.G.S., Hon. F.R.G.S., Late Reader in Palæontology in the University of London (Imperial College of Science and Technology). London: Thomas Murby & Co., 1935. (Pp. xii + 406, with 565 figures.) 22*s*. 6*d*. net.

Invertebrate Paleontology. By William H. Twenhofel, Professor of Geology and Paleontology, and Robert R. Shrock, Assistant Professor of Geology and Paleontology, University of Wisconsin. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xvi + 511, with 175 figures.) 30*s*. net.

Kenya. Contrasts and Problems. By L. S. B. Leakey, M.A., Ph.D., F.S.A., F.R.A.I. London: Methuen & Co., Ltd., 1936. (Pp. xvi + 189, with 8 plates.) 7*s*. 6*d*. net.

Soviet Geography. The New Industrial and Economic Distributions of the U.S.S.R. By N. Mikhaylov. With a Foreword by The Rt. Hon. Sir Halford J. Mackinder. London: Methuen & Co., Ltd., 1935. (Pp. xx + 232, with 38 maps and 20 tables.) 10*s*. 6*d*. net.

Salinity and Temperature of the English Channel. Estimation of Mean Values for the Upper Water Layer over the 25-Year Period 1903 to 1927. By J. R. Lumby, Fisheries Laboratory, Lowestoft. Ministry of Agriculture and Fisheries, Fishery Investigations, Series II, Vol. XIV, No. 3, 1935. London: H.M. Stationery Office, 1935. (Pp. 151, with 8 figures and 16 tables.) 7*s*. net. (Atlas of Charts sold separately, 20*s*. net.)

Intermediate Botany. By L. J. F. Brimble, B.Sc. (Lond. and Reading), Associate of University College, Reading. London: Macmillan & Co., Ltd., 1936. (Pp. viii + 562, with 337 figures.) 8*s*. 6*d*.

Pollen Grains. Their structure, identification and significance in science and medicine. By R. P. Wodehouse, Ph.D., Scientific Director of the Hay-fever Laboratory, The Arlington Chemical Company, Yonkers, New York. McGraw-Hill Publications in the Agricultural and Botanical Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1935. (Pp. xvi + 574, with 14 plates, 123 figures and 6 tables.) 36*s*. net.

The Story of the Plant Kingdom. By Merle C. Coulter, Professor of Botany, The University of Chicago. U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1935. (Pp. x + 270, with 119 figures.) 13*s*. 6*d*. net.

Tropical Planting and Gardening. With special reference to Ceylon. By H. F. Macmillan, Late Superintendent, Royal Botanic Gardens, Ceylon, and Adviser to the Anglo-Persian Oil Company. Fourth edition. London: Macmillan & Co., Ltd., 1935. (Pp. x + 560, with 4 plates and numerous text figures.) 25*s*. net.

The Living Garden or The How and Why of Garden Life. By E. J. Salisbury, D.Sc., F.R.S. London: G. Bell & Sons, Ltd., 1935. (Pp. xii + 338, with 17 plates and 60 text illustrations.) 10*s*. 6*d*. net.

Vergleichende Morphologie der höheren Pflanzen. Vol. I: Vegetationsorgane. Part 1. By Dr. Wilhelm Troll, o.Professor der Botanik an der Martin-Luther-Universität Halle-Wittenberg. Berlin: Gebrüder Borntraeger, 1935. (Pp. viii + 171, with 104 figures.) RM.11.50 (subscription price RM.9.60).

A Text-book on Forest Management. By M. R. K. Jerram, M.C., Assistant Lecturer in Forestry, University College of North Wales, Bangor. London: Chapman & Hall, Ltd., 1935. (Pp. x + 156, with 17 figures.) 10*s*. 6*d*. net.

Forest Bibliography with the Index Number 634.9 F. An International Decimal Classification on the basis of Melvil Dewey's system. Adopted on the recommendation of the International Committee on Forest Bibliography, 1906–1933. Oxford: Hall the Printer, Ltd., 1936. (Pp. viii + 100.)

Practical Biology for Medical Students. By C. J. Wallis, M.A.(Cantab.), Master-in-Charge of Biology, University College School, Hampstead. London: William Heinemann (Medical Books) Ltd., 1936. (Pp. xii + 247, with frontispiece and 98 figures.) 12*s*. 6*d*. net.

Cold Spring Harbor Symposia on Quantitative Biology. Vol. III. Cold Spring Harbor, L.I., N.Y.: The Biological Laboratory, 1935. (Pp. xvi + 359, with illustrations.)

Objective Evolution. By Christopher Pleydell-Bouverie. London: Williams & Norgate, Ltd., 1936. (Pp. 233.) 7*s*. 6*d*. net.

Genetic Variations in Relation to Evolution. By H. S. Jennings, Henry Walters Professor of Zoology and Director of the Zoological Laboratory, The Johns Hopkins University. Princeton, N.J.: Princeton University Press; London: Humphrey Milford, 1935. (Pp. viii + 139, with 21 figures.) 9*s*. net.

The Cranial Muscles of Vertebrates. By F. H. Edgeworth, M.D., M.A., D.Sc., Professor Emeritus of Medicine in the University of Bristol. Cambridge: at the University Press, 1935. (Pp. x + 493, with 841 figures.) £5 5*s*. net.

Insect Pests of Glasshouse Crops. By Herbert W. Miles, M.Sc., Ph.D. Adviser in Agricultural Entomology in the North-Western Province, Lecturer in Entomology in the Victoria University of Manchester, and Mary Miles, M.Sc., lately Philip Buckle Research Scholar in Agricultural Zoology in the Victoria University of Manchester. With a Foreword by J. C. F. Fryer, O.B.E., M.A. Published by H. C. Long, "The Birkins," Orchard Road, Hook, Surbiton, Surrey, 1935. (Pp. 174, with 21 plates and 15 figures.) 8*s*. 6*d*. net (by post 9*s*.)

Lac and the Indian Lac Research Institute. By Dorothy Norris, M.Sc., F.I.C., P. M. Glover, B.Sc., and R. W. Aldis, Ph.D., D.I.C. Second edition. Nankum, Ranchi, Bihar and Orissa: Indian Lac Research Institute, 1935. (Pp. x + 66, with 15 plates and 13 tables.) Rs.2/8.

Sea-Trout of the Montrose District. Part II: Growth on the Scales of Recaptured Fish. Part III: The Migrations of Sea-Trout. By G. Herbert Nall, M.A., F.R.M.S. Fishery Board for Scotland, Salmon Fisheries, 1935, Nos. II and III. Edinburgh: H.M. Stationery Office, 1935. (Part II: pp. 22, with 15 plates. Part III: pp. 24, with 1 map and 5 tables.) 2*s*. net each.

Scale-Absorption in Salmon and Sea Trout. By M. Ian Crichton, B.Sc., Department of Zoology, University of Edinburgh. Fishery Board for Scotland, Salmon Fisheries, 1935, No. IV. Edinburgh: H.M. Stationery Office, 1935. (Pp. 8, with 4 plates.) 1*s*. net.

Human Ecology. By J. W. Bews, M.A., D.Sc., Principal of the Natal University College. London: Oxford University Press, 1935. (Pp. xii + 312.) 15*s*. net.

Essentials of Physiological Chemistry. By Arthur K. Anderson, Ph.D., Professor of Physiological Chemistry, The Pennsylvania State College. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. vi + 257, with 31 figures.) 13*s*. 6*d*. net.

Elementary Morphology and Physiology for Medical Students. By J. H. Woodger, D.Sc., Reader in Biology in the University of London. Second edition. London: Oxford University Press, 1935. (Pp. xii + 498, with 255 figures.) 12*s*. 6*d*. net.

The Integration of the Endocrine System. Being the fifth Horsley Memorial Lecture delivered by Sir Walter Langdon-Brown, M.D., F.R.C.P., Emeritus Professor of Physic in the University of Cambridge and Consulting Physician to St. Bartholomew's Hospital. Cambridge: at the University Press, 1935. (Pp. 54.) 2*s*. net.

Foundations of Short Wave Therapy. Physics and Technics, by Wolfgang Holzer, Dr.-Ing., Assistant in the Physiological Institute of the University of Vienna; Medical Applications, by Eugen Weissenberg, Dr.-Med., Medical Superintendent of the Short Wave Section of the University Clinic for Nervous and Mental Diseases in Vienna. Translated by Justina Wilson, F.R.C.P., D.M.R.E., and Charles M. Dowse, B.Sc., A.M.I.E.E. London: Hutchinson's Scientific and Technical Publications, 1935. (Pp. 228, with 53 figures and 10 tables.) 12*s*. 6*d*. net.

Vitamins in Theory and Practice. By Leslie J. Harris, Sc.D., D.Sc., Nutritional Laboratory, University of Cambridge, and Medical Research Council. Cambridge: at the University Press, 1935. (Pp. xx + 240, with 66 figures and 44 tables.) 8*s*. 6*d*. net.

A Text-book of Pharmacognosy. By J. W. Cooper, Ph.C., Pharmacist, Leeds Public Dispensary and Hospital, Special Lecturer in Pharmacy, Bradford Technical College, and T. C. Denston, B.Pharm., Ph.C., Lecturer in Pharmacognosy, Chelsea Polytechnic. With illustrations and drawing notes by M. Riley, A.T.D., Head Master, Selby School of Art and Crafts, and D. W. Shaw, B.Sc., Ph.C., Lecturer in Pharmacognosy, Bradford Technical College. Second edition. London: Sir Isaac Pitman & Sons, Ltd., 1935. (Pp. xiv + 522, with 78 figures and 16 maps.) 18*s*. net.

Invisible Radiations of Organisms. By Otto Rahn, Professor of Bacteriology, Cornell University. With an Introduction to the Physics of Radiation by Sidney W. Barnes, Research Associate in Physics, University of Rochester. Protoplasma-Monographien, Vol. 9. Berlin: Gebrüder Borntraeger, 1936. (Pp. x + 215, with 52 figures and 54 tables.) RM.13.20.

The Aims and Methods of Medical Science. An Inaugural Lecture. By John A. Ryle, M.A., M.D., F.R.C.P., Regius Professor of Physic in the University of Cambridge, Consulting Physician to Guy's Hospital, London. Cambridge: at the University Press, 1935. (Pp. 44.) 2*s*. net.

New Pathways in Medicine. By Henry Cohen, M.D., F.R.C.P., Professor of Medicine in the University of Liverpool. A Public Inaugural Lecture delivered in the University of Liverpool. Liverpool: at the University Press; London: Hodder & Stoughton, Ltd., 1935. (Pp. 19.) 1*s*. net.

Chemical Discovery and Invention in the Twentieth Century. By Sir William A. Tilden, D.Sc., LL.D., Sc.D., F.R.S. Revised by S. Glasstone, D.Sc., Ph.D., F.I.C., Lecturer in Chemistry, University of Sheffield. London: George Routledge & Sons, Ltd., 1936. (Pp. xvi + 492, with 100 plates and 28 figures.) 15*s*. net.

James Watt. Craftsman & Engineer. By H. W. Dickinson. Cambridge: at the University Press, 1936. (Pp. xvi + 207, with 18 plates, 14 figures and 2 woodcuts.) 10*s*. 6*d*. net.

Tylor. By R. R. Marett, M.A., D.Sc., LL.D., Rector of Exeter College, Oxford. Modern Sociologists, Vol. I. London: Chapman & Hall, Ltd., 1936. (Pp. 220, with frontispiece.) 6*s*. net.

Pareto. By Franz Borkenau. Modern Sociologists, Vol. II. London: Chapman & Hall, Ltd., 1936. (Pp. 219, with frontispiece.) 6*s*. net.

The Freedom of Man. By Arthur H. Compton. The Terry Lectures, 1932. New Haven: Yale University Press; London: Humphrey Milford, 1935. (Pp. xiv + 152.) 9*s*. net.

Language, Truth and Logic. By Alfred J. Ayer, Research Student of Christ Church, Oxford. London: Victor Gollancz, Ltd., 1936. (Pp. 254.) 9*s*. net.

An Examination of Examinations. By Sir Philip Hartog, K.B.E., C.I.E., and E. C. Rhodes, D.Sc., Reader in Statistics in the University of London. International Institute Examinations Enquiry. London: Macmillan & Co., Ltd., 1935. (Pp. 81.) 1*s*.

Training in Industry. A Report embodying the results of inquiries conducted between 1931 and 1934 by the Association for Education in Industry and Commerce. Edited by R. W. Ferguson, B.Sc., A.R.C.S., Honorary Secretary of the Association for Education in Industry and Commerce, 1919 to 1930, Executive Chairman of the Association, 1931 to 1934. London: Sir Isaac Pitman & Sons, Ltd., 1935. (Pp. xii + 156.) 6*s*. net.

The Young Engineer. A Manual dealing with Indian practice designed to assist students of Engineering generally, and particularly those requiring a knowledge of colonial methods. By J. N. Digues la Touche, M.Inst.C.E., Chief Engineer, Indian State Railways (Retd.). With a Foreword by Colonel Sir Gordon Risley Hearn, C.I.E., D.S.O. London: The Technical Press, Ltd., 1936. (Pp. xii + 242, with 3 figures.) 5*s*. net.

Tomorrow's Children. The Goal of Eugenics. By Ellsworth Huntington, in conjunction with the Directors of the American Eugenics Society. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. (Pp. x + 139.) 6*s*. net.

The Next Hundred Years. The Unfinished Business of Science. By C. C. Furnas, Associate Professor of Chemical Engineering, Yale University. London: Cassell & Co., Ltd., 1936. (Pp. xx + 366.) 8*s*. 6*d*. net.

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GENERAL INDEX

INDEX OF REVIEWS